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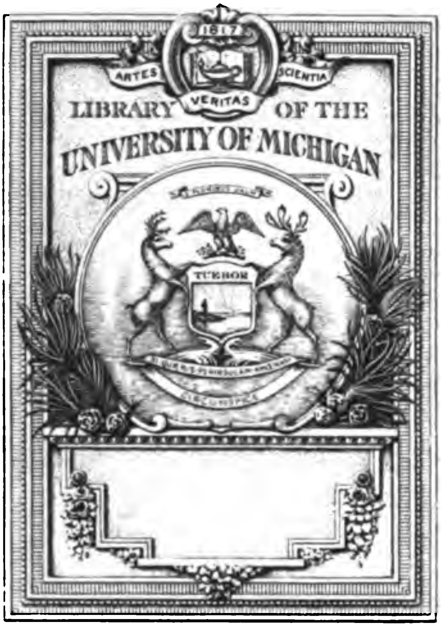

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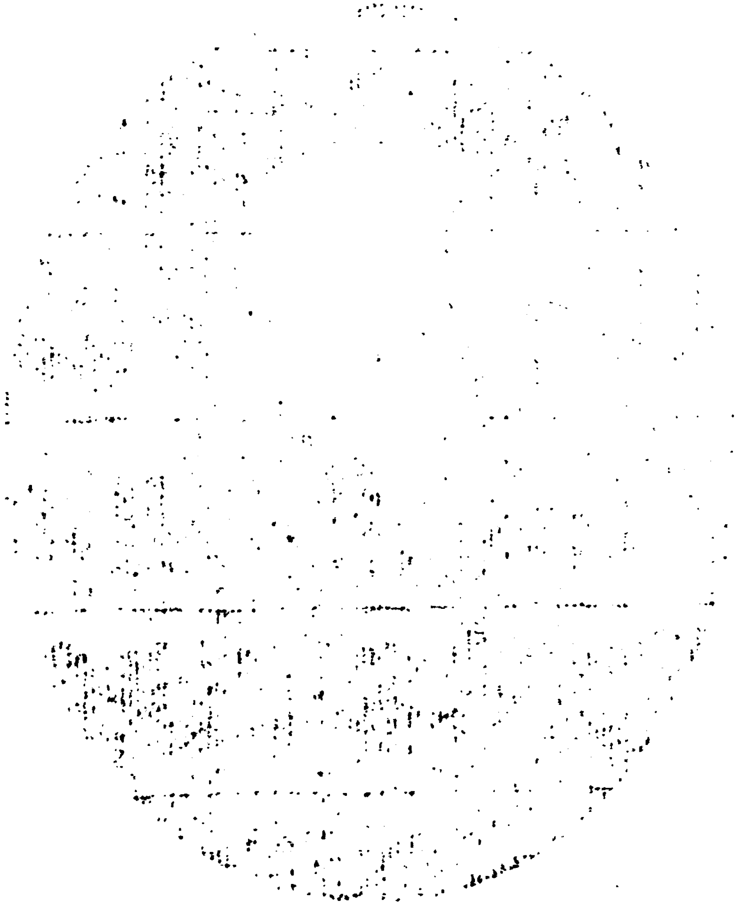

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PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

> "Nec aranearum ſane textus ideo melior, quia ex ſe fila gignunt. Nec noſter vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE
PHILOSOPHICAL MAGAZINE.

I. *On Indian Dogs.* *By Dr.* BARTON, *of Philadelphia*[*].

IT would require much obſervation, many experiments, and a great deal of time, to collect all the neceſſary materials for an exact hiſtory of the native dogs of North America. I am by no means prepared for the ample taſk. My object, in the preſent article, is much more limited—to bring together a number of ſcattered facts relative to the origin and manners of the different kinds of dogs which were found among the Indians when the Europeans firſt took poſſeſſion of the countries of North America. In the inveſtigation of this inquiry I cannot pretend to be very methodical: nor do I expect to avoid errors. I aim, however, at correcting ſome of the errors of preceding writers.

It has been aſſerted, by many hiſtorians and naturaliſts, that there were no dogs in America prior to the diſcovery of this portion of the world by the Europeans[†]. Mr. Pennant, one of theſe naturaliſts, remarks: "As it is certain that the dog of North America, or rather its ſubſtitute, on its firſt diſcovery by the Engliſh, was derived from the wolf tamed and domeſticated, ſo it is reaſonable to imagine that of South America had the ſame origin[‡]." I do not think it ſufficiently aſcertained that there were not originally in America any ſpecies of dog of the ſame ſtock as thoſe of the old world. It has not yet been proved, that the dog of Newfoundland, of which I am afterwards to make more particular mention, was unknown in that iſland before its diſcovery by the Europeans. But admitting that the Newfoundland ſpecies or variety originated from an admixture of the European dog with the wolf, or ſome other native animal of the country, it will ſtill appear ſomewhat probable, that the Indian dogs in many other parts of America were not *ſpecifically* different from thoſe of the old world. This queſtion is well worthy of the attention of the naturaliſt, and is even entitled to the notice of the civil hiſtorian of the new world.

---

[*] Communicated by the Author.

[†] Acoſta, Gomara, Herrera, Joannes Fabri, Buffon, Pennant, &c. &c.

[‡] Hiſtory of Quadrupeds, vol. i. p. 237.

Techichi, or Alco.

In Mexico and in South America there was a ſmall ſpecies of dog, which the Mexicans called *techichi*, and the Peruvians *allco*, or *alco*. This is particularly mentioned by the jeſuit Joſeph Acoſta [*], and other of the earlier viſitors of America. The *alco* had a melancholy aſpect, and was perfectly mute, or dumb. Hernandez ſpeaks of it as being ſimilar in nature and manners to the common dogs of Europe, and not very different in form [†]. It is remarkable that Linnæus has taken no notice of this ſpecies, though he ſo frequently refers to the work of Hernandez. Mr. de Buffon has confounded it with the *itzcuintepotzotli*, or next ſpecies, from which, however, it appears to have been diſtinct [‡]. Gmelin, who has fallen into a ſimilar miſtake (though the words of Hernandez are ſufficiently plain), conſiders the *alco* as a variety of the canis familiaris, or faithful dog. He calls it canis familiaris, Americanus. Although I have little doubt that the *alco* was a true canis [§], I think it is too ſlightly mentioned or deſcribed to enable us to determine, with as much certainty as the naturaliſt could wiſh, whether it was merely a variety of the common dog, or an entirely diſtinct ſpecies. It is to be regretted, indeed, that the naturaliſts who viſited America in the ſixteenth century, whilſt the alco was ſtill a common animal, have left us ſo much in the dark concerning its origin and nature. Owing to their negligence, we are, at this diſtance of time, only permitted to ſay, with ſome degree of probability, what it was not. I do not, with Mr. Pennant, think it probable that it was derived from the wolf. Its entire muteneſs is, I think, greatly oppoſed to this idea;

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[*] The Naturall and Morall Hiſtorie of the Eaſt and Weſt Indies, &c. p. 301, 302. Engliſh tranſlation. London 1604.

[†] Hiſtoriæ Animalium et Mineralium Novæ Hiſpaniæ Liber Unicus, &c. p. 6, 7.

[‡] Hiſtoire Naturelle, &c. tom. xxx. p. 200, &c.

[§] Independent of the teſtimony of Hernandez, there would ſeem to be very little doubt that the *alco* was a true ſpecies of canis, and, indeed, very ſimilar to ſome of our ſmall houſe-dogs. The Spaniards, according to Clavigero, gave it the name of *perro*, which ſignifies a dog; and Acoſta obſerves, that the Indians called all the dogs which were brought from Spain *alco*, from the reſemblance between them and their native animal. It is probable that the Indians in ſome parts of South America had afterwards (perhaps when the *alco* became either very rare, or extinct) adopted the Spaniſh word *però* for dog. The Jaioi, in Guiana, uſed this word at leaſt as early as 1633. De Laet's Novus Orbis, p. 643.

as is alſo, perhaps, its complete domeſtication. If it were immediately derived from the wolf, we ought, at leaſt, to allow the Americans ſome ſhare of merit for the ſucceſsful pains they had taken to reclaim this animal from the wild ferocity of his nature.

This ſpecies or variety of dog appears to have been pretty extenſively diffuſed through the ſouthern parts of the continent of America, and certainly exiſted in ſome of the iſlands when they were firſt diſcovered by Columbus. Peter Martyr, and after him other hiſtorians, mention the diſcovery of mute little dogs in the iſland of Juanna in the year 1492. Martyr ſays they were deformed in ſhape, and that the ſavages ate of them as the Europeans did of goats*. I cannot find that this ſpecies was diſcovered among any of the Indian tribes inhabiting the tract of country now called the United States: It is not certain, indeed, that the dogs which Soto found in Florida were not of the alco kind: I think it highly probable, however, that they were not, but that they were much more allied to the wolf and fox, like the modern Creek dogs, which I am afterwards to mention.

It would appear from Clavigero, that the alco is now entirely extinct. "After the conqueſt of Mexico," ſays this author, "the Spaniards, having neither large cattle nor ſheep, provided their markets with this quadruped; by which means the ſpecies was ſoon extinct, although it had been very numerous †."

Itzcuintepotzotli.

The *itzcuintepotzotli* was the Mexican name for another ſpecies or variety of dog, which is figured and deſcribed by Fabri ‡ and by Clavigero, whoſe figure is borrowed from that of the Italian naturaliſt. If the figure be an accurate one, the animal muſt have been of a very deformed aſpect; and as ſuch, indeed, it is deſcribed. It was about the ſize of a Malteſan dog, or rather larger. The head was very ſmall, the ears pendulous, and the eyes ſoft and pleaſing. The noſe had a conſiderable prominence in the middle, and its tail was very ſmall. But the moſt ſtriking feature of the animal was a protuberance upon its back, not unlike that upon the Arabian camel. The ſkin was varied with white, tawny, and black.

This ſpecies particularly abounded in the kingdom of Michuacan, the moſt weſterly part of the old empire of Anahuac. The natives of Michuacan called it *abora*, or *abora*. It is ſaid, by Clavigero, to be almoſt wholly extinct [*].

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* The Decades of the Newe Worlde or Weſt India, &c. The firſt decade, p. 15. Engliſh tranſlation. London 1555. 4to.

† The Hiſtory of Mexico, vol. i. p. 40.

‡ Rerum Medicarum Novæ Hiſpaniæ Theſaurus, &c. p. 466, &c.

The *itzcuintepotzotli* bears no reſemblance whatever to the wolf, from which it is not probable that it was derived. It has much more the aſpect of ſome of the domeſticated dogs; and Hernandez informs us, that it reſembled them in nature and in manners. That it was a ſpecies of canis is very probable; but that it was a mere variety of the common dog is much more uncertain. I rather ſuſpect it was not. We are not, indeed, permitted to decide this matter with certainty. One eſſential difference between the two animals we are able to collect: the Mexican dog is ſaid to have ſix teats, whereas the common dog has ten.

Buffon, Pennant, and Gmelin, have confounded this animal with the *alco*. Hernandez, however, plainly ſpeaks of them as two diſtinct animals [†]; as does alſo the abbé Clavigero [‡]. It is probable, however, that they were conſiderably allied to each other.

Either this ſpecies or the *techichi*, perhaps both, were brought to the market of the city of Mexico, along with deer, rabbits, and many other animals, before the conqueſt of the Spaniards. Gage ſays theſe animals were ſold either "by quarters or whole." It would appear from the ſame writer, that theſe dogs were ſometimes caſtrated for food [§]. Theſe ſimple facts are calculated, with many others, to ſhow that the Mexicans, at the time they were diſcovered, had actually advanced, in many reſpects, towards the attainment of that police, thoſe arts and practices, which are never obſerved among people in the ſavage forms of ſociety. Another century, but for the diſcovery of Columbus, would have conducted theſe unfortunate Americans much nearer to the condition of their conquerors; but it is to be feared that many centuries would have been requiſite to have weaned them from their hideous religion, which was the foundation of their ſavage practices and manners. The hiſtory of mankind exhibits abundant proofs of this poſition, that the arts which they practiſe, and the police which they obſerve, are no certain evidences of a truly civilized ſtate. A mild religion appears to be abſolutely neceſſary to the attainment and the preſervation of this happy ſtate of man.

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[*] The Hiſtory of Mexico, vol. i. p. 44.

[†] Hiſtoriæ Animalium, &c. Liber Unicus, p. 7.

[‡] The Hiſtory of Mexico, vol. ii. p. 382 and p. 323.

[§] A New Survey, &c. p. 111.

Wolf Dogs.

We know not whether the *techichi* and the *itzcuintepotzotli* were found in any of the countries conſiderably to the north of Mexico. We are well aſſured, however, that different kinds of dogs were very common in many of the countries of North America, when this continent was firſt diſcovered by the Europeans, in the 16th and 17th centuries. I am even inclined to think that North America was much better ſupplied with dogs (I mean theſe animals in the domeſticated ſtate) than South America and Mexico. There ſeems to be little doubt that in the northern countries there was a greater variety than in the ſouthern countries. Florida abounded in theſe animals. When Fernando de Soto marched his army through that country, in the year 1540, the Indians ſupplied him with great numbers of dogs. On one occaſion, an Indian cacique ſent the Spaniſh general no leſs than three hundred dogs *. Theſe were eaten by the Spaniards, who deemed them not inferior to the beſt of ſheep †. But we are informed that the Indians did not eat them ‡. It would ſeem that the Spaniards did not always ſtand upon the ceremony of waiting to have the dogs preſented to them. The Portugueſe author of Elvas, who accompanied Soto and his ſucceſſor in their mad ramble, informs us, that, during the time the army laboured under a ſcarcity of meat, "he who could catch a dog in any village thought himſelf a very happy man; for ſometimes (he obſerves) we found thirty in a place; but the ſoldier that killed one, and ſent not a quarter to his captain, ſuffered for it, paying dear for his incivilities when he was to go ſentinel, or upon any guard of fatigue §."

We are not told what kind of dog it was that the Spaniards found among theſe Indians. There do not appear to be good grounds to ſuſpect that they were of the *alco* or *itzcuintepotzotli* kinds. It is certain that neither of theſe animals is now known among any of the Floridian Indians; and it does not ſeem likely that the breeds which theſe Indians at preſent poſſeſs have been reclaimed from the wild ſtate ſince the time of Soto's "mad adventures."

---

* This was the cacique of Quaxule, which, if we can depend upon the old maps of Florida, was in the country of the Chikkaſah Indians. A Relation of the Invaſion and Conqueſt of Florida, &c. &c. p. 71.

† See A Relation, &c. p. 55.

‡ A Relation, &c. p. 71. I do not think it certain that the Indians did not eat their dogs. The preſent which Soto received at Ocute, in the country of the Creek Indians, rather favours the opinion that they did. The cacique ſent the Spaniſh general "two thouſand Indians, with a preſent of rabbets, partridges, maes-bread, two pullets, and a great many dogs." A Relation, &c. p. 55. If the Indians did not eat their dogs, why did they ſuppoſe the Spaniards were fond of them? It is true, there was a great ſcarcity of meat and ſalt at Ocute, and the Indians may have ſuppoſed that any kind of food would be acceptable to an army of hungry men. Beſides, it is probable they had many opportunities of ſeeing the Spaniards employed in ſtealing their dogs.

§ A Relation, &c. p. 56.

The dogs which are now in uſe among the Creeks, Chikkaſah, and other ſouthern tribes, are of different kinds. As far as I have been able to collect information concerning them, they, in general, bear a very ſtrong family reſemblance to the wolf. One kind is very ſimilar to the *canis lycaon*, or black wolf, of which I have already made mention. It is not, however, always black, but of different colours, commonly of a bay colour, and about one-third leſs than the wild black wolf. It carries its ears almoſt erect, and has the ſame wild and ſly look that the wolf has *.

The other kind of dog is ſmaller than the one juſt mentioned, and is more like the common red fox. Both kinds bark, but not ſo much as the common dogs; and their bark is different from that of our dogs, being more nearly allied to the howl of the wolf †.

I am unable to ſay, with certainty, whether theſe ſouthern dogs differ very eſſentially from thoſe among the northern Indians. I rather ſuppoſe they do not. I know, at leaſt, that among the latter, as well as among the former, there are two ſpecies or varieties; one which has generally been conſidered as the wolf merely altered by the domeſticated ſtate, and the other more allied to the fox. But as my information concerning the northern dog is more correct and particular than it is concerning the ſouthern, I wiſh to be underſtood as ſpeaking principally of the former, in the following deſcription of the Indian dogs.

The Indian dog (I mean that which is moſt allied to the wolf) is frequently called, by the traders and others, *the half-wolf breed*. His general aſpect is much more that of the wolf than of the common domeſticated dogs. His body, in general, is more ſlender than that of our dogs. He is remarkably ſmall behind. His ears do not hang like thoſe of our dogs, but ſtand erect, and are large and ſharp-pointed. He has a long ſmall ſnout, and very ſharp noſe [*]. His barking is more like the howling of the wolf. When attacked, and when fighting, he does not ſhake his antagoniſt, like our dogs. His teeth are very ſharp, and his bite ſure. When he ſnarls, which he is wont to do upon the ſlighteſt occaſion, he draws the ſkin from his mouth back, preſenting all his teeth to view. Our dogs, when once attacked by theſe Indian dogs, always fear and ſhun them. It is a very curious circumſtance, that the Indian dog will never attack or purſue the wolf, which the common dogs ſo readily do. This fact ſeems to point very ſtrongly to the origin of the American animal. For the purpoſes of hunting, the Indian dogs are very uſeful; but, in other reſpects, they are by no means ſo docile as the common dogs. They have leſs fidelity; for, though never ſo well fed, they will ſteal from their maſters [†]. In ſhort, every thing ſhows that the Indian dog is a much more ſavage or imperfectly reclaimed animal than the common dog.

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* From the information of Mr. William Bartram.

† I have been informed, that among the Cheerake Indians the dogs are of a more mixed breed, more like thoſe of the whites. This is doubtleſs owing to the greater intercourſe which has ſubſiſted between theſe Indians and the whites. The Cherakee themſelves are ſo much mixed with the Europeans that they are often named by the traders, the "Breeds."

If my information has been correct, this ſpecies or breed is ſtill preſerved in the greateſt purity among the Six Nations, from whom the Delawares acknowledge that they received it. The Delawares call this dog *lenchum*, or *lenni-chum*, which ſignifies "the original beaſt." The Nanticokes call him *ihn-wâllum*; the Mahicans, *annun-neen-dee-a-oo*, or "the original dog," to diſtinguiſh him from our common dogs, which they call ſimply *dee-a-oo*, or *de-a-oo*.

Theſe appellations ſhow that the Indians conſider their wolf dog as a native of the country, and that they are not incapable of diſcerning the differences between this animal and the greater number of the varieties of dogs which have been introduced into America by the Europeans.

The origin of the Indian dog is a queſtion of much more difficulty than ſome naturaliſts have imagined. Thus, Mr. Lawſon ſeems to ſuppoſe that the dogs which he ſaw among the Indians of North Carolina were merely wolves, "made tame with ſtarving and beating [‡]." This is eaſy natural hiſtory. Mr. Pennant, as we have already ſeen, ſuppoſed that the dog of North America was derived from the wolf, tamed and domeſticated *. This opinion has been maintained by other writers; but it is an opinion which muſt be admitted with ſome limitation. I am, indeed, much inclined to believe that the Indian dog, in many parts of North America, was derived from the wolf; but it remains to be proved that it is, in any part of the continent, the pure or unmixed wolf, in a ſtate of domeſtication. It is, more probably, an hybrid animal, begotten between the wolf and ſome other animal, perhaps the fox. Mr. Joſſelyn, a long time ago, conſidered the dog of the New-England Indians as the produce of the wolf and fox †. This is alſo the opinion of many well-informed perſons who have reſided among, or viſited, the Indians. I believe it is the opinion which many of the Indians themſelves entertain concerning the origin of their dog.

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[*] Some perſons inform me, that many of the Indian dogs have a large white ſpot upon the breaſt.

[†] This aſſertion, I muſt confeſs, is oppoſed by the teſtimony of ſome writers. Thus Carver ſays, the Indian dogs are "remarkable for their fidelity to their maſters; but, being ill fed by them, are very troubleſome in their huts or tents." Travels, &c. p. 416.

[‡] A New Voyage, &c. p. 38.

I have already obſerved that the Indian dog is ſometimes called *the half-wolf breed.* This plainly ſhows that thoſe who have impoſed this name did not view the American dog as a mere domeſticated wolf. In other words, it ſhows that they conſidered him as an hybrid animal. I may add, that Carver and other writers, who have enjoyed pretty extenſive opportunities of obſerving the dogs of the Indians, merely ſpeak of their reſemblance to the wolf, without pretending to aſſert that they are only domeſticated wolves ‡.

Owing, however, to the great affinity which ſubſiſts between the Indian dog and the wolf, the ſavages in ſome parts of North America beſtowed the ſame name upon both of theſe animals. Thus father Hennepin expreſsly informs us, that *chonga* is a dog or wolf, in the language of the Iſſati and Naudoweſſies. In general, however, the Indians apply different names to the wolf and to the dog, whether it be their own (or native) dog, or thoſe varieties which they have received from the whites. I may add, that the Indians ſeem alſo to have remarked the reſemblance of ſome of their dogs to the fox; for the Mohawks (or at leaſt the Cochnewagoes, who have ſprung from the Mohawks) call the red fox *cheets-hoo.* Now the Tuſcaroras, who ſpeak a dialect of the language of the Mohawks, call a dog *cheeth* and *cheetht.* Much dependence, however, ſhould not be placed upon this application of names; for ſavages ſometimes beſtow the ſame names upon ſpecies that are unqueſtionably diſtinct.

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* See p. 1.

† "The Indian dog is a creature begotten betwixt a wolf and a fox, which the Indians lighting upon, bring up to hunt the deer with." Joſſelyn's New England's Rarities, &c. p. 13.

‡ "The dogs employed by the Indians in hunting appear to be all of the ſame ſpecies; they carry their ears erect, and greatly reſemble a wolf about the head." Theſe are Carver's words. See his Travels, &c. p. 416.

We are not yet prepared, it is obvious, to give an exact genealogical hiſtory of the Indian dog. We are compelled to mix conjecture with fact. The anatomical ſtructure of the animal ſhould be examined. But, whatever may have been the origin of this breed of dogs, I am diſpoſed to think, with Joſſelyn, that the ſavages found it in the woods, and that it has exiſted as a diſtinct ſpecies, or breed, for a very long period of time. Several of the earlier viſitors of different parts of North America ſpeak of the exiſtence of wild dogs in the country. Renatus Laudonerius invaded Florida in the year 1564, only a few years after the death of Soto. In his enumeration of the native productions of the country he mentions wild dogs. There is no reaſon to ſuppoſe that he has confounded them with the wolves: for he expreſsly ſays that the country produced, beſide theſe dogs, ſome ſpecies of wolves *.

The diſcoverers of the iſland of Cape Breton, in the Gulf of St. Lawrence, found in that iſland black dogs, which, we are informed, the Indians were very careful to bring up to hunting †. I think it probable that both theſe and the dogs mentioned by Laudonerius were the ſame as the half-wolf breed which I have deſcribed.

[To be continued.]

II. *An Account of a new Method of ſupplying Diving-bells with freſh Air.* By Robert Healy, *A.B.*

SIR, *To Mr. Tilloch.* Dublin, Jan. 8, 1803,
43, James's-ſtreet.

I TAKE the liberty of communicating an experiment on diving, which was made laſt Auguſt by my father, Mr. Samuel Healy. Should it appear worthy of holding a place in your very uſeful and inſtructive Magazine, you are at full liberty to inſert it. Robert Healy.

THE method of ſupplying a diving-bell with air, which has hitherto been generally adopted, renders it almoſt impracticable either to deſcend in water to any conſiderable depth, or to remain there a length of time ſufficient to perform any thing uſeful. Much inconvenience reſults from the attention which it is neceſſary to pay in admitting the ſupply from the barrels, which are uſed as reſervoirs of air; much alſo from the labour and time which are expended in cauſing them to reach the bell. The mode of ſupply which Mr. Healy contrived appears much calculated to remedy theſe inconveniences. A recital of the circumſtances that attended Mr. Healy's experiment, it is hoped, will not prove unintereſting.

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* See De Laet's Novus Orbis, lib. iv. p. 215.

† See the ſame, lib. ii. p. 37.

Captain Lonſdale, of the Experiment, was employed to raiſe a brig which had foundered, in the year 1799, in the Bay of Dublin, between Dunleary and Howth. This he endeavoured to accompliſh by faſtening chains round the bow and ſtern of the ſunken veſſel, and by connecting to theſe a ring on each ſide, through which cables paſſed, and were laſhed at low water acroſs the deck of his ſhip, that acted as a buoy. Conſequently, on the tide riſing, if the faſtenings had not given way, the floating veſſel muſt either have ſunk itſelf, or drawn the other upwards.

Mr. Healy accompanied captain Lonſdale in this attempt, in order to put in practice his mode of ſupplying a diving-bell with air. The bell, which reſembled a truncated cone, was made of wood, conſiſting of ſtaves united by cooperage: the mouth was two feet and a half in diameter; the top one foot and a half: the height was four feet. Windows were placed at proper diſtances round the ſides, and one at the top: there was alſo an aperture in the top for letting out foul air. In the inſide was ſuſpended a ſtage for the purpoſe of reſting on. On the top was fixed an iron eye, through which a cable paſſed for raiſing or lowering the bell. This eye was ſecured to the bell by four iron bars, of an inch ſquare, that went down the ſides and lapped under its edges. Within ſix inches of the bottom was fixed a broad iron hoop, of an inch thickneſs, from which weights were ſuſpended to ſink the bell.

On the ſhip's deck was laſhed a forcing or condenſing ſyringe, capable of containing about two quarts, to which were connected five fathom of iron tube, and to the end of this an equal length of leathern tube that turned into the bell. When the piſton was depreſſed, the contained air paſſed through the tubes, and was forced into the bell. Thus a conſtant ſtream of air was forced down.

Four hundred weight and a half being ſuſpended in the manner deſcribed, Mr. Healy was firſt let down rapidly, when, on ſignifying his deſire of aſcending, his wiſh was immediately complied with. He ſtated, that great uneaſineſs was felt, particularly in his ears, from ſuch a ſudden deſcent; that there was ſufficient light in the bell to enable him to read by light reflected through the mouth of the bell without removing the ſhutters of any of the windows; and alſo that he could ſee a good way down in the water. Having reſted a few minutes, and matters being arranged more ſatisfactorily, he was lowered gradually, and remained ſtationary about a minute at each fathom's depth; the ſyringe ſupplying ſuch a conſtant ſtream of air, that the bell was ſupercharged, and the ſignal of ſufficiency was often repeated. Having been more than half an hour down, and for ſome time on the deck of the ſunken veſſel, he gave the ſignal for aſcending, and was drawn up in the ſame gradual manner in which he had been let down.

He ſaid, that the very inſtant the mouth of the bell was immerſed a noiſe ſtruck his ears, which went off upon his reſting at the diſtance of a fathom from the ſurface: the next deſcent cauſed the ſame deafening ſenſation, the removal of which was effected, to a conſiderable degree, by repeated yawnings. No inconvenience was felt by his reſpiring air condenſed by the preſſure of ſix or ſeven fathoms water. A ſlight giddineſs remained a few minutes after his emerging, and the blood-veſſels about his face were a little ſwollen.

In the night, the laſhings having given way at high water, one of the rings through which the cables paſſed fell to the bottom. On the following day he deſcended, in a gradual manner as before, to the depth of ſeven fathoms, in order to raiſe the ring, and remained below for an hour and ſome minutes. Not the ſmalleſt inconvenience was felt in his breathing. After coming up he coughed, and a ſlight tinge of blood appeared in his ſpittle. The fullneſs in his face, as alſo the giddineſs, occurred as in the laſt experiment. In this the bell did not appear to admit ſo much light as in the former experiment, although the papers explanatory of the communication of ſignals by pulling the ropes (which, for precaution, were fixed to the inſide of the bell) were ſtill legible. With the ſyringe one man ſupplied ſeven gallons of air in a minute, and, if neceſſary, could have ſupplied double that quantity.

The following morning the wind roſe to ſuch a degree as to break both the cables and chains that were attached to the ſunken ſhip, and of courſe interrupted the proſecution of further experiments; which, however, at a future period, Mr. Healy hopes to reſume. It is to be wiſhed that other adventurers will make ſimilar attempts, and improve on the hint which this trial affords; for, to uſe the words of Seneca,

Patet omnibus ars, nondum eſt occupata, multum ex illâ etiam futuris relictum eſt.

III. On

III. *On Painting.* *By Mr.* E. Dayes, *Painter.*

Essay VIII.

On Manner.

Peculiar marks I hold to be generally, if not always, defects; however difficult it may be wholly to eſcape them. *Sir Joſhua Reynolds.*

THE word *manner* may be applied to colour, light and ſhade, and penciling. It is expreſſive of certain peculiar marks that invariably characterize the works of each individual, as in ſome a blueneſs in the colouring prevails, in others a gray or yellow, while others are diſtinguiſhed by a harſhneſs in the ſhadows; in one the penciling is round, in another ſquare or forked. So far is a new manner from being a mark of genius, as ſome aſſert, that, could perfection in painting ever be attained, it would be unaccompanied by any peculiarity whatever.

In that part of our education which is to be obtained by copying, we ought to be particularly careful that the works we copy, or the maſter we imitate, have a manner the pureſt and the leaſt vicious poſſible; for we may reſt aſſured, ſingularity, which in ſome is glaringly abſurd, will be the firſt portion we ſhall inherit.

He who forms to himſelf a model in a maſter will be always inferior to the archetype: the heads of all the great ſchools have been ſuperior to their imitators. Nature riſes in the ſame degree over the ſervile and baſe. M. Angelo was ſuperior to his diſciples, and that in proportion to their dependent habit of thinking. The ſame cauſe placed Raphael, Titian, the Caracci, &c. at the head of certain claſſes of artiſts, many of whom have followed their maſters limpingly and awkwardly. The ſame baneful deſire of imitation is equally detrimental to poets; for the arts cannot be called liberal in the hands of thoſe who want ſpirit to think for themſelves. Not to acknowledge the favours we receive would we illiberal, but to ſink under them into a ſtate of ſlavery is baſe. The wiſdom of the world may inform, but we muſt improve from ourſelves; for precept will do but little if the mind is not ſuſceptible of it; the ſeed muſt be ſuited to the ſoil. The mind, like a fine ſpring of water, will become more productive and clear the more it is uſed.

Peculiarity is what chiefly characterizes the different maſters. We ſay, for inſtance, the manner of Raphael is dry and hard, thereby indicating the too violent diſplay of outline, the deciſions and abrupt diviſions in the ſhadows, with the parts not ſufficiently loſt in their grounds. That of Spagniolet is forcible, with much red in the fleſh; while in that of Rembrandt we expect little light, with a glow of colour, that may too often be called rotten-ripe. We may further obſerve the dark manner of Guercino and the ſilver of Guido, with many others not neceſſary to mention. What are thoſe differences in the various maſters, but ſo many ſingularities that characterize and diſtinguiſh the individual?

As perfection cannot be attained, every artiſt, of neceſſity, will have a manner; but in proportion as he ſucceeds in approaching perfection will his manner become the more pure. This he can only hope to attain by an extenſive inquiry, that is, by not ſlaviſhly tying himſelf down to the imitation of an individual. He is bound to ſhow in his works that he has opinions of his own, and that he dares to think for himſelf.

Raphael's firſt manner was like that of his maſter Perugino; but this he ſoon quitted for a ſweeter mode of colour, which he caught from Da Vinci and Bartolomeo, and for a more noble and elevated ſtyle, which he acquired from M. Angelo. Julio Romano imbibed much of the fire of his maſter, as did Roſſo and Prematicio of theirs. The Caracci adopted a moſt liberal manner of imitation, founded on a combination of the excellences of all the great ſchools, and ultimately produced one entirely new. Domenichino, Guido, Guercino, and Schidone, exhibit in their works but ſlight traces of the ſchool they were formed in. Van Dyck is perfectly original. Our countryman Reynolds is an example of this liberal manner of imitation; we ſee in his works the grace of Corregio and Parmegiano combined with the beauty and richneſs of Venetian colouring, accompanied with the *chiaro-ſcuro* of the Flemiſh and Dutch. Le Sueur's firſt manner reſembled his maſter Vouet, which he ſoon quitted, and made himſelf ſuperior in every part of the art. Le Brun was influenced by the ſame ſpirit of independence, and left his maſter far behind.

Of thoſe who have been deſtitute of the pride of independence, and have fallen into a narrow, confined, and illiberal kind of imitation, we will point out a few. Guido was imitated by Sirani, Pouſſin by Verdier, Paul Veroneſe by his brothers, and Jacomo Baſſan by his ſons. Rubens was imitated by Jacques Jordans, who has increaſed the exceſſes of his maſter without adding one perfection of his own. Rembrandt was followed ſervilely by Bramer, Eckhout, De Gelder, &c. We might enumerate many others, which muſt occur to every one's recollection, whoſe works paſs with the ignorant for thoſe of their maſters. Perhaps this difference in the works of artiſts may ariſe from the external objects and their images painted in the eye of ſome, not agreeing; which may alſo account for the variation we obſerve between the bulk or altitude of bodies and their repreſentations by various artiſts. This peculiar habit of ſeeing may conſtitute what we call manner.

It is certain too much copying, or too great a devotion to the works of ſome favourite maſter, brings on a habit of ſeeing even nature with his imperfections. Hence it is, that what is termed the ſchools have in the end proved the ruin of art; not becauſe they were bad, but becauſe weak men have been content with nature at ſecond-hand: ſome author calls ſuch people not nature's ſons, but her grandſons.

Before we quit this part of our eſſay we will juſt touch on the manner of penciling, alſo called handling. The uſe of the pencil is diſtinguiſhed into the ſmooth or mellow, and the expeditious or bold. The application of thoſe manners muſt, in a great degree, depend on the ſize of the picture to be painted, alſo its ſituation and ſubject. As far as ſubject is concerned, the former method beſt applies to objects in themſelves beautiful; as elegant female figures, young and ſleek animals, paſtoral, and all objects intended to delight. The latter manner will beſt aſſociate with aged, broken, and irregular objects, and ſuch ſcenes as are intended to excite terrible emotions. The bold pencil of Rubens is highly admirable, and is a great beauty in his pictures; while the pencil of Van Dyck is more ſoft and mellow. Claude had a ſmooth pencil ſuiting his choice of ſubject; on the contrary, Salvator Roſa is bold, and well impaſted with colour. The pencil of Titian is a fine example for the heroic in landſcape, or what may be termed the hiſtorical ſtyle, and which, as mentioned in our firſt eſſay, derives its name from the dignity of the objects of which the picture is compoſed. Thoſe are miſtaken who imagine it derives its character from ſlightneſs and indeciſion, and who dignify their own crude and undiſtinguiſhable maſſes with that noble appellation. The pencil of Titian is firm, bold, and at the ſame time deciſive, and, in the higher ſtyle of landſcape, may be offered as an example worthy of our attention. We ſhall find numberleſs examples in the Dutch ſchool to direct our hand in the paſtoral; and the pencil of young Teniers is, perhaps, the fineſt in that part of the art. The pencil is not a mere vehicle for laying on the colour, but its motion muſt expreſs the character of the various objects it may be employed on. As in landſcape the foliage of trees, incruſting of the bark; on cattle, in repreſenting hair and wool; the character and folds of drapery, the thinneſs of flowers, &c. &c.

As well as the manual practice, painting requires, 1. A boldneſs of hand in the dead-colouring: 2. In the ſecond colouring, more circumſpection and labour: and, 3. Thorough patience and attention in the retouching and finiſhing of the picture. Theſe qualities can no more be ſeparated than Venus and the Graces.

He who wiſhes to inſure himſelf a good manner of penciling ſhould avoid copying ſuch pictures as are imperfect in that reſpect; for, if we begin with a ſlovenly or bad one, every ſtep will plunge us deeper in error. But all attempts at painting will be vain if we do not poſſeſs the power to determine the form at once, which can only be acquired through a previous practice in drawing; for on this the clearneſs as well as the firmneſs of the penciling depends.

There are two methods of preparing our picture: one is, to draw in the objects with their ground colour, carefully laying in the ſhadows, dead-colouring the lights ſolid, and *preſerving the ſhadows tranſparent throughout the work*; in the ſecond ſtage, to correct the forms, and add to the darks where wanted; and, thirdly, after oiling out the parts, bringing the whole into harmony by glazing, ſcumbling, and finiſhing with the extreme lights and darks. This direction is general; for, after the lights and darks are added, parts may require to be kept down, enriched, &c., as practice muſt direct. The ſecond method is to dead-colour *ſolidly throughout*, and to finiſh the ſhadows by glazing. The latter method admits of greater changes being made in the work during its progreſs than the former.

*** In our laſt Eſſay, for W. Scope, read W. Scrope, eſq. of Caſtle Comb.

IV. *Experiments on the Colouring and Mordant Property of the Oxide of Molybdena.* *By* M. D. JAEGER [*].

THESE experiments were made with foliated molybdena of Altenberg, in Saxony, prepared in the following manner:

After having ſeparated the metal from all quartzy parts, it was reduced into an acid by long expoſure to a red heat in an open crucible. This acid was then diſſolved in boiling water, and ſaturated with potaſh. The molybdate of potaſh being ſoluble in a much larger quantity of cold water than the free molybdic acid, this acid was united to potaſh with a view that a more concentrated ſolution of it might be applied to the ſtuff, which, in regard to linen and cotton, is a matter of great importance.

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[*] From Scherer's *Allgemeines Journal der Chemie*, January 1802.

A. One part of this ſolution was diluted with ten or twelve parts of rain water; and a ſhred of raw white woollen ſtuff, and another of white Saxon kerſeymere, were boiled in it for half an hour. The two ſhreds aſſumed a grayiſh green colour. Both of them were then cut into ſmall bits, for the purpoſe of ſubjecting them to the following experiments:

Theſe bits of cloth were put, 1ſt, Into a boiling ſolution of ſulphate of zinc very much diluted: 2d, A ſimilar ſolution of acetite of lead: 3d, Another of ſulphate of copper: 4th, Another of tin in the ſulphuric and muriatic acids: 5th, Another of ſulphate of iron: 6th, Of tin in ſimple muriatic acid: 7th, Another of tin in acetous acid: 8th, Another of tin in nitro-muriatic acid by muriate of ammonia: 9th, Into one of cobalt in nitric acid, rendered ſlightly muriatic by muriate of ſoda: 10th, And into aqueous tincture of gall-nuts prepared cold, which communicated to woollen ſtuff boiled in it a ruſſet colour.

The different pieces, after being boiled for a quarter of an hour in ſolution of molybdena, when waſhed and dried exhibited different ſhades.

B. A ſhred of white cotton cloth was immerſed, cold, for twelve hours, in a part of the ſolution of molybdate of potaſh much diluted; and it was then removed to a diluted ſolution of tin in a mixture of the ſulphuric and muriatic acids. The ſtuff, which had not changed its colour in the ſolution of molybdena, aſſumed a bright blue colour ſomewhat dirty.

The above two pieces of cloth which were treated with the ſolutions of tin exhibited this peculiar phænomenon, that on the edge, when cut, they were of a darker and purer blue than externally. Another circumſtance, no leſs remarkable, is, that theſe colours experienced from the light an effect contrary to that produced on vegetable colours, which loſe their colour by expoſure to the ſun; while that of molybdena acquires a double degree of intenſity. The green ſhades generally paſſed to blue; but after ſome time reſumed in the ſhade, or when expoſed to the moiſt atmoſphere, their primitive colour.

However ſatisfactory theſe reſults might be, M. Jaeger carried his reſearches ſtill further, and endeavoured to obtain colours better defined, and brighter; and particularly pure blue and green ſolutions of tin were thoſe which ſeemed moſt likely to accompliſh that end.

C. The ſolution of molybdate of potaſh was mixed, in certain proportions, with pure ſolutions of tin in muriatic acid, diluted in cold rain water. Shreds of raw white woollen ſtuff, moiſtened with boiling water, were boiled for half an hour in this mixture. The liquor was decompoſed, depoſited the tin under the form of a grayiſh powder, and the reſiduum aſſumed an agreeable blue colour, which was communicated alſo to the ſtuff, but with a ſhade leſs pure.

D. Not ſatisfied with this reſult, M. Jaeger tried to prepare the ſtuff, before it was immerſed in the bath of molybdena, with different mordants; ſuch as ſulphate and acetite of alumine, diluted ſulphuric and muriatic acid, acidulous tartrite of potaſh, and others: but the reſult did not anſwer the object he had in view. He then thought it neceſſary to employ other means.

E. A ſolution in exceſs of molybdate of potaſh was then mixed with a ſaturated ſolution of tin; the mixture was boiled for half an hour in a retort, and was afterwards left at reſt for eight or ten days. After this time, the liquid, which at firſt was of a grayiſh blue colour, was found changed into a bright dark blue, and had depoſited a large quantity of gray oxide of tin. The liquid was decanted, and filtered through double paper.

1ſt, This tincture communicated a bright blue colour to a ſhred of muſlin, which had been left in it for a quarter of an hour after it had been diluted in two parts of water.

2d, A ſhred of raw white woollen ſtuff was ſo ſtrongly coloured by this tincture, that after ebullition for half an hour it came out dyed of a pure ſaturated blue colour.

As the tincture did not appear to be exhauſted of colour, there were boiled in it for half an hour, each after the other,

3d, A ſhred of woollen ſtuff.

4th, A ſhred of the ſame kind. The two ſhreds acquired a blue colour, decreaſing in intenſity.

To aſcertain how far the blue colour of the woollen cloth could be fixed, a ſhred of ſtuff was boiled for an hour in one part of blue tincture diluted with half a part of water. Its colour was not darker than that of No. 2.

The dyed ſhred was then cut into two parts, and one of them was boiled for a quarter of an hour in the ſame tincture, to which a little of the ſolution of tin had been added. Its colour this time was more intenſe.

The colour in theſe experiments had diſtributed itſelf perfectly, and had united in ſo intimate a manner with the fibres of the wool, that the ſtuff had all the appearance of wool dyed with indigo. After this ſucceſs, M. Jaeger endeavoured to unite the colour of molybdena with other vegetable colours, and to tranſport it in that ſtate to ſtuffs. The reſults of this labour were as follow:

F. Different ſhreds of woollen cloth, to which a greater or leſs blue colour had been communicated by tincture of molybdena, were boiled for half an hour in a cold aqueous infuſion of quercitron bark. They acquired different ſhades of green, but which did not poſſeſs all the purity and uniformity which could have been wiſhed. He therefore thought that it would be neceſſary to change a little his proceſs.

G. Different ſhreds of woollen ſtuff were boiled for a longer or ſhorter time in an infuſion of quercitron bark, with a view to communicate to them different ſhades of yellow. The quantity of blue tincture neceſſary for each ſhade of yellow, beginning with the weakeſt, was added to the infuſion, which was in a great part exhauſted. In this manner, ſhades of green, not inferior in beauty to Saxon green, were obtained by an ebullition of half an hour.

H. In other experiments, ſometimes the blue tincture and ſometimes the molybdate of potaſh was tried, either by combination with vegetable colours, or alone, as mordants or modifiers. The reſults in general were very ſatisfactory. Several of the colours thus obtained were ſo fixed and unalterable in the ſun and air, and by ſtrong acids, that no other colour could be compared with them in this reſpect.

M. Jaeger terminates his intereſting labour by announcing a black colour, furniſhed to him by a compoſition of molybdate of potaſh, acetite of alumine, and logwood. This black having been tried, in regard to durability, as compared with common black, by boiling both for a quarter of an hour in diluted ſulphuric acid, that of molybdena was ſcarcely changed, while the other loſt its colour entirely, and paſſed to a burnt yellow.

The author adds alſo, that the blue tincture of molybdena, evaporated in a gentle heat, furniſhed a very fine blue, ſoluble in water, which might be uſed for writing, and even for painting, in the ſame manner as any other colouring juice.

It appears from theſe experiments that molybdena may become a very valuable colouring matter in the art of dyeing.

V. *Remarks on the preſent State of Aëroſtation.* By *Mr.* G. J. Wright.

[Concluded from p. 346 of our laſt Volume.]

THE machine being conſtructed, it only remains to fill it with inflammable air (hydrogen gas). The uſual method of procuring inflammable air is by the ſolution of iron in ſulphuric acid or common oil of vitriol: for this purpoſe a number * of air-tight caſks are diſpoſed in circles of ten or twelve in each circle. The caſks compoſing one circle communicate, by their individual tubes, to one centre caſk containing water, whoſe height muſt be ſuch as that the orifice of each tin tube ſhould be ſome inches under the fluid †. The centre caſks (or *coolers*, as they are technically termed) are the only ones immediately connected with the balloon, and that by varniſhed ſilken tubes proceeding from their ſummits: there muſt alſo be a ſecond orifice in the head of each caſk compoſing the circles; each ſuch orifice being for the purpoſe of introducing the ingredients, and provided alſo with an air-tight plug. The iron (of which the turnings from the boring of cannon are reckoned the beſt) being placed in the caſks, the diluted acid ‡ (having been previouſly well mixed in a ſeparate veſſel purpoſely provided) is to be poured upon it, ſtopping up the orifice with the plug as ſoon as the ſmell of the gas is perceived: the quantity of acid firſt put into each caſk muſt be half the requiſite proportion for the weight of iron therein contained; and this rule muſt be obſerved throughout the whole number of caſks, ſo that on the ſecond ſubſequent addition the aggregate quantity of gas ſhall be produced §.

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* This was the method adopted by Mr. Garnerin; but its complexity renders it very exceptionable, as in other experiments two or three caſks have been made to anſwer all that he effects with thirty, and in much leſs time.

† Without this precaution the inflammable air is liable to carry along with it ſome of the acid in a vaporous ſtate, which in one inſtance became gradually condenſed in the balloon when arrived to a colder region, deluging the aëronaut with ſome quarts of this acidulous liquor.

‡ It is neceſſary to dilute the acid in a ſeparate veſſel, not merely on account of the great difference in the reſpective gravities of the acid and water, which renders ſtirring neceſſary, but alſo on account of the great quantity of heat liberated, which is ſufficient to occaſion ebullition in ſome inſtances, and might derange the apparatus.

§ This is the method adopted by ſome experimenters; but a more expedient one deſerves to ſuperſede it; eſpecially as by nearly the like and undermentioned proceſs Meſſrs. Baldwin, Sadler, and Lunardi, ſucceſsfully inflated their machine in two hours.

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The iron muſt be particularly attended to to inſure ſucceſs; whatever is ruſty is not only not ſerviceable, but actually injurious, by generating hydro-carbonate, a gas ſpecifically

The quantity of hydrogen gas developed during the action of diluted ſulphuric acid upon iron, varies not only according to the ſtrength of the acid, but the quality of the iron employed. Dr. Prieſtley (Experiments and Obſervations on Air, vol. vi.) found that annealed caſt iron invariably afforded more inflammable air than iron which had not undergone that proceſs; the difference being nearly 1-8th. Thus iron turnings, as being in ſome degree annealed, gave conſiderably more air than only caſt iron, yet leſs than iron nails perfectly annealed. Mr. Cavendiſh alſo obtained from an ounce of zinc 356 ounce meaſures of inflammable air; from the ſame quantity of iron 412 meaſures; while tin afforded him only half the quantity yielded by iron. Suppoſing then we are about to inflate a balloon of 30 feet diameter, it will require, agreeably to Mr. Cavendiſh's experiments, about two ounces and a half of iron for each cubic foot of inflammable air, or 2200 pounds of iron to be diſſolved in order to completely fill the machine; and to produce this ſolution there will altogether be required an equal weight of concentrated oil of vitriol, and ſix times this weight of water. But completely filling a balloon before its aſcenſion is uſeleſs; becauſe the denſity of the ſurrounding medium, immediately decreaſing, will cauſe an equally premature expanſion of the gas, and require its eſcape even before it attains 1000 yards height: the proportional product, therefore, of 2000 pounds of metal will be as much as may with propriety be applied to a machine of 30 feet diameter.

Having proceeded according to the rules hereafter laid down for aſcertaining the requiſite quantity of materials agreeably to their reſpective purity, provide next two large cylindrical wooden veſſels open at the top, and ſhaped as inverted truncated cones, each veſſel capable of containing about 1000 gallons, ſo that the two may hold in the ſequel the whole quantity of materials. In each of theſe veſſels diſpoſe planes of lattice or baſket-work, at intervals of ſix or eight inches above each other (their diameters correſponding to the diameter of the part of the veſſel each plane is to reſt upon), adding a ſufficient number of them till within a foot of the ſurface at which the diluted acid will ſtand when the whole quantity ſhall have been added. The uſe of theſe planes is for dividing the iron into many portions, ſo as to expoſe as large a ſurface as poſſible to the action of the acid: the weight of iron, ſuppoſing the veſſel to contain ten of theſe planes, will be 100 pounds upon each; which weight will ſuffice for keeping them ſteady, without having recourſe to other methods of fixing them to the veſſel itſelf; while the gas will eaſily find a paſſage for itſelf from the lower through the ſuperior planes by the interſtices in the baſket-work.

Having thus diſpoſed of the iron ſo as that the two veſſels ſhall contain their ſpecific proportion, amounting together to the whole of the required quantity, provide for each veſſel an appropriate head, of ſomewhat leſs diameter than its upper rim, that when fixed therein it may reſt an inch or two below the level of the ſame, in order that a thin ſtratum of water may lie over the whole ſurface, and render the head completely tight. From the centre of the head muſt proceed a cylindrical tube of tin ſix inches in diameter and one foot in height; whence forming a right angle it ſhould proceed about ſix feet, and then deſcend by an obtuſe angle whoſe extremity ſhould terminate in an aſcending nook, converging till its extreme

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cally heavier than even common air. It muſt be alſo totally devoid of greaſe, as the ſmalleſt ſtratum of greaſy matter will effectually ſecure the iron from being attacked by the acid.

To

treme orifice be diminiſhed to an inch diameter*, and in ſuch a direction as eaſily to be placed under, and fix or eight inches below, the baſe of the inverted funnel of the reſervoir. Beſides this central tube there muſt be a lateral one of an inch diameter and ſix inches in height, provided with an air-tight wooden ſtopper. Through the head muſt alſo be fixed a glaſs or pewter tube bent to form an inverted ſyphon, the ſhorter leg of which muſt be placed ſo as to be included in the veſſel, while the longer leg (of two feet in length) will remain on the outſide. A third veſſel muſt alſo be provided, as a reſervoir or cooler, capable of holding from 200 to 300 gallons, open at top, yet not more than three feet deep. Acroſs its diameter, and eight or ten inches below its upper rim, fix two ſtrong wooden bars as ſupports, to which fix an inverted wooden funnel (the baſe of which muſt not be leſs than two feet diameter) whoſe upper part ſhould end in a tube, round which the varniſhed ſilken tube immediately connected with the balloon is to be faſtened. After properly fixing the inverted funnel, provide a ſquare wooden tube, and fix it to the inſide of the reſervoir ſo as to reach within ſix inches of its bottom. while its upper orifice ſhould ſtand at leaſt a foot higher than the ſurface of the water therein. This being done, and the extremities of the large tin tubes being properly placed under the inverted funnel, begin with pouring ſeveral gallons of cream of lime (made by ſlaking quicklime in water, a gallon of fluid to two pounds of lime) into the reſervoir; then gradually fill the ſame with water till within an inch or two of the top. In the next place, pour through the inverted ſyphon of each tub a mixture of one part acid and ſix of water, while an aſſiſtant ſtanding over the ſmall lateral tin tube (now open) replaces the plug in the ſame as ſoon as the ſmell of the hydrogen gas is perceived, indicating that the atmoſpheric air is now all expelled from the tub. And thus muſt the workmen proceed, pouring the diluted acid into the tubs through the ſyphons till ſuch time as the required quantity for each veſſel (*i. e.* 1000 pounds of acid, and 6000 pounds, or 750 gallons, of water) has been added; the column of fluid retained in the ſhorter leg of the ſyphon forming a ſtopper, whereby any eſcape of gas at this aperture is prevented.

A ſeparate aſſiſtant muſt in the mean time attend the pneumatic cooler to renew the water therein, which will be at intervals ſaturated with all the matter it can abſorb from the gas; he muſt therefore occaſionally diſplace this water by pouring freſh quantities down the ſquare wooden tube for the purpoſe, which will cauſe the ſaturated fluid to flow over the top of the veſſel, and in doing the ſame muſt, for every freſh addition of water, add a proportionate quantity of the cream of lime. During the whole of the experiment the water in the cooler muſt be ſtirred with a wooden rod, that the unſaturated lime (continually tending to precipitate itſelf) may be perpetually brought into contact with the gas; while, the more frequent the renewal of the water in the reſervoir, and the larger the proportion of freſh cream of lime, the lighter will the gas be obtained. Alſo, to facili-

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* By this contrivance the gas will iſſue in a continued ſtream, which will prevent that great agitation of the water under the funnel which is occaſioned by the gas forcing its way through the water in large quantities at a time, yet at intervals of ſome diſtance, whereby much of it is frequently loſt.

To provide againſt theſe two circumſtances, the aëronaut ought not to omit ſubjecting the iron turnings to the action of a ſtrong fire amidſt carbonaceous matters, for at leaſt an hour, a day or two before his propoſed uſe of them. The thickneſs of the iron muſt be alſo noticed, as, under the moſt favourable circumſtances, the acid never penetrates it more than half an inch: therefore all the weight of iron that remains over and above a ſurface of half an inch depth every way, becomes uſeleſs; the coat of newly formed oxide of iron effectually guarding the remaining metal from ſolution: for want of attending to this circumſtance, the quantity of metal has frequently proved deficient to the end propoſed, to the great diſappointment of the experimenter.

Alſo where the public have to rely on the good faith of the exhibiter, he ought to try the ſtrength of his acid previous to employing it; and this, not by the ordinary methods of ſpecific gravity, nor the quantity of alkali it will ſaturate [*], but by the actual proceſs, in the ſmall way, of what he propoſes to perform on the day of aſcent: by theſe means he cannot fail of ſucceeding.

But the above is not the only method of procuring inflammable air [†]: the paſſing of water over red-hot metals is fol-

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tate the entrance of the inflammable air into the balloon, the varniſhed ſilken tube, proceeding from the inverted funnel, muſt rather aſcend than otherwiſe in its courſe to the balloon, which may be eaſily managed by ſupporting it along a ladder having a gentle inclination upwards. I apprehend the coſt of the larger tubs of 1000 gallons each will be leſs expenſive than to have recourſe to twenty or thirty butts for the purpoſe; which, by dividing the proceſs, cauſes greater apprehenſions for failure with reſpect to the tightneſs of ſo many veſſels and tubes; beſides the inconvenience of having to procure ſo great a number of caſks for every freſh voyage, when undertaken as a public exhibition in places far diſtant from each other.

The ſulphate of iron, the product of the mutual action of the acid and iron, is a ſubſtance much uſed in the arts under the name of green copperas: the chemiſt, dyer, calico-printer, ink-maker, refiner, &c. have great demand for this article. It may therefore be either diſpoſed of, or diſtilled *per ſe* to regain the acid; while the reſiduum, by a further calcination, may be converted into that ſpecies of oxide known by the name of colcothar, an article much employed for poliſhing glaſs and metals, and alſo as a pigment.

[*] We are liable to miſtakes if we rely on the ſpecific gravity of the acid (uſually varying from 1·6 to 1·8), which is augmented by its containing in ſolution neutral ſalts, eſpecially ſulphate of potaſh; an adulteration not unfrequently practiſed. The quantity of alkali required to ſaturate a given quantity of an acid, can never detect the abſolute quantity of the individual acid we are in ſearch of, but only indicate the degree of acidity of the whole, without any regard to the ſpecific radicals.

[†] Inflammable air is procured in many chemical operations; but it is uſeleſs to mention here any other than ſuch as are advantageouſly applicable to aëroſtatic purpoſes.

lowed by the extrication of a large quantity of hydrogen gas from the decompoſition of the water; but the metal is apt to run to a ſlag reſembling finery cinder, preventing its further action upon the water, and that before any conſiderable proportion is decompoſed.

The diſtillation of pit-coal alſo affords this gas, but not above one-fourth lighter than atmoſpheric air. In obtaining it in this way, a number of receivers muſt be interpoſed between the retort and the balloon, to condenſe the volatile, aqueous, and oleaginous products of this diſtillation *.

If a parachute is required, it ſhould be conſtructed ſo as when diſtended to form but a ſmall ſegment of a ſphere, and not a complete hemiſphere; as the weight of this machine is otherwiſe conſiderably increaſed, without gaining much in the oppoſing ſurface. The parachute of Mr. Garnerin is particularly defective in a too great extenſion of its diameter; an unneceſſary addition to its weight of a lining of paper both withinſide and without; the too near approximation of the baſket to the body of the parachute; and eſpecially in the want of a perpendicular cord paſſing from the car to the centre of the concave of the umbrella, by the abſence of which the velocity of the deſcent is certain to be very rapid before the machine becomes at all diſtended; whereas, if a cord were thus diſpoſed, the centre of the parachute would be the portion firſt drawn downwards by the appended weight, and the machine would be almoſt immediately at its full extenſion.

Having found, by experiment, the diameter required for inſuring ſafety †, the further the baſket or car is from the umbrella, the leſs fear ſhall we have of an inverſion of the whole from violent oſcillations; yet the longer the ſpace between the car and the head of the machine, the longer will be the ſpace run through in each vibration when once begun, yet by ſo much the more will they be ſteadier; and this ought to be attended to, as when by the violence of the oſcil-

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* During the diſtillation of pit-coal, water and carbonate of ammonia are afforded, together with a pitchy oil reſembling tar: theſe will be condenſed in the neareſt receivers, while the gaſeous product (conſiſting of a mixture of hydrogen with azotic and carbonic acid gaſes) will proceed onward; and which if made to paſs through lime-water will be rendered more pure: a pound of coal will afford about three cubic feet of inflammable air, and prove the cheapeſt method of inflation. Dr. Prieſtley alſo found that the product of inflammable air, in all operations in the dry way, was always greater in proportion as the fire had been ſuddenly raiſed.

† Mr. Baldwin adviſes the diameter 15 feet; by which means, "the man weighing 140 pounds and the parachute 10 pounds, with a ſurface of 150 ſquare feet, he would feel no greater ſhock than if he had fallen from the height of ſix feet." At the Pantheon, Mr. Garnerin's parachute was announced as being 40 feet in diameter, and weighing above 80 pounds.

lations the car became (in Garnerin's experiment) on a line with the horizontal axis of the machine, (or, in other words, the point of ſuſpenſion,) the force of gravity, or the gravitating power of the weight in the car, on the umbrella, being at that criſis reduced to nothing, the ſlighteſt cauſe might have carried the body of the machine in a lateral direction, reverſing the concavity of the umbrella, and Mr. Garnerin, perhaps, have fallen upon the now convex yet internal portion of the bag, and the whole have deſcended confuſedly together *.

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* The umbrella being acted upon in a perpendicular direction from the car, if by any, the moſt trivial cauſe, the latter vibrates from the perpendicular it will draw the machine in the ſame inclined direction; the car alſo with its cords of ſuſpenſion, now acting the part of a pendulum, will continue to oſcillate to each ſide till a perfect equilibrium is reſtored. The ſpace of the arc deſcribed in each vibration will depend upon the diſtance between the car and the parachute conjunctly with the reſiſtance afforded by the latter to the air, whereby on decreaſing the reſiſtance (or, which is the ſame thing, the ſurface of the umbrella,) the weight in the car will ſooner draw the machine into the perpendicular, and the oſcillations conſequently ceaſe: thus a parachute whoſe ſurface is ſmall enough to obviate oſcillations in the middle regions (*i. e.* the half of the uſual heights ſurmounted in aëroſtatic experiments, or about 5000 feet), would be defective for want of ſurface to moderate the velocity of deſcent when near the ground, and *vice verſa*: if, therefore, we can augment or diminiſh at pleaſure the ſurface and conſequent reſiſtance of the parachute during its deſcent, we ſhall have attained the perfection of the machine.

Thus in Mr. Garnerin's experiment the oſcillations did not commence till the machine felt the increaſed reſiſtance of a more denſe zone of the atmoſphere; and having then no means of diminiſhing the ſame, the vibrations began, continued, and increaſed, till, by arriving in a yet *more* denſe region, a greater reſiſtance was oppoſed to the oſcillations of the baſket, gradually diminiſhing the arc of the ſame to nothing, this alſo being aſſiſted by a diſcharge of ballaſt from the car, by which means the weight of the pendulum and its power to ſweep through a denſe atmoſphere were conjointly diminiſhed; for *the vibration of a pendulum will be in proportion to its length, and alſo to the denſity of the medium through which it is to vibrate*; a fact too much overlooked. There is therefore a medium height at which the oſcillations will be the greateſt; and the diſtance of this medium from the earth's ſurface will vary with the different ſtates of the barometer at the earth; or, in other words, as the denſity at different heights. Thus, when the altitude of an aëronaut deſcending by a parachute is great, the atmoſphere is there too much rarefied to occaſion reſiſtance to the umbrella ſufficient to allow of an oſcillation; but when in a lower zone a vibration has unfortunately begun, the atmoſphere is here too denſe to allow of that velocity of deſcent which only could have prevented the commencement of the oſcillations, yet not denſe enough to afford ſufficient reſiſtance to retard the ſweep of the baſket; while in a yet more denſe region, nearer to the ſurface of the earth, the air affords ſo much reſiſtance to the ſwing of the car, as ſoon effectually to ſtop the ſame, eſpecially if the weight therein could now be diminiſhed by a diſcharge of ballaſt; a fact evinced in Mr. Garnerin's experiment, when he threw out a quantity of ballaſt when near the earth to moderate the ſhock of his deſcent, which immediately

I ſhould propoſe the parachute to be conſtructed of varniſhed cambric muſlin (or light linen), covered externally with a netting, the meſhes of which ſhould converge ſmaller as they approach the central portion of the machine; obſerving alſo to fix the netting on the ſurface by ſewing it down upon various parts of the ſame. The diameter of the parachute (forming only a ſmall ſegment of a ſphere), when extended, not to exceed twenty feet. The central aperture of the umbrella (to allow the eſcape of the air through, as the machine deſcends), not leſs than three feet in diameter, and provided with a ſhutter moving upon a hinge and opening inwards *; furniſhed alſo with a cord ſliding over a ſingle pulley to ſhut the aperture, and a ſecond cord to open the ſame (each cord reaching to the car), thus ſubjecting the ſhutter to the will of the aëronaut. By this mechaniſm the ſtupefying vibrations of the car may be moderated, if not wholly prevented, by drawing down the ſhutter ſo as to fully expoſe the aperture, whereby the reſiſtance of the parachute to the atmoſphere being leſſened by all this diminution of its oppoſing ſurface, the gravitating power of the appended weight (tending to draw the body of the machine continually in the perpendicular) will have proportionally a greater effect: the deſcent in the interim will be alſo relatively more rapid; but this may be checked in a moment by cloſing the orifice, eſpecially when near the ſurface of the earth †.

A ſtrong cord ſhould proceed perpendicularly from the centre of the car to a point formed by the convergence of a number of cords proceeding from the ſecond ring of wicker-work, the effect of which would be an immediate extenſion of the parachute to its full diameter at the inſtant of deſcent, as before adverted to. Alſo, and laſtly, a third hoop of wicker-work, of ſix or eight feet diameter, fixed in the internal concave of the umbrella, to ſecure againſt any accidental collapſe of the ſides before the machine has become diſtended

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diately reſtored the machine to its perpendicular ſituation, but for which he could not account. An attempt to obviate theſe inconveniences is the object of the valve of the parachute mentioned hereafter.

* This aperture, or, more properly, valve, of the parachute, may be conſtructed either of cloſe wicker-work, or varniſhed linen ſtrengthened by a piece of netting externally, and fixed to a ring of wicker-work adapted to cloſe the diameter of the ſecond ring of wicker-work, to which are fixed the pieces of varniſhed linen compoſing the concave umbrella. This latter circle is independent of the third hoop of wicker-work of ſix or eight feet diameter, afterwards mentioned, whoſe uſe is to prevent a collapſe of the ſides of the machine.

† By an enlargement of the diameter of the valve, this diminution of ſurface might be carried to ſuch extent as to inſure a ſafe deſcent by the parachute even in the moſt tempeſtuous weather.

diſtended by its preſſure on the cylinder of air immediately beneath it.

With reſpect to the probability of directing aëroſtatic machines [*], we may infer it to be poſſible, although the methods hitherto tried have been inadequate; perhaps becauſe they were not ſufficiently powerful; as, to expect to make ſo large a body as a balloon to vary from the wind by the impulſion of an oar of ſix or eight feet in length and one or two in breadth (and that by only endeavouring to draw the car out of the perpendicular), is to expect, by means of a boat's oar, to impel a ſhip of burthen. Oars are doubtleſs the moſt likely means to effect this purpoſe, if they were of dimenſions proportionate to the effects they are wiſhed to produce [†]. The addition of ſails, where any variation from the wind is deſired, will prove injurious till we have attained a method (perhaps only to be accompliſhed by oars) of keeping the ſame point of the balloon continually in a given direction. Yet I doubt not but theſe alſo might prove of great ſervice in quick diſpatches by water; as, for inſtance, where it is required to paſs a fortreſs or fleet for the ſuccour of a beſieged town, or convey diſpatches thereto: a ſmall balloon, of ten or twelve feet diameter, provided with ſails to expoſe a large ſurface to the wind, being attached by a long rope to a boat, would outſtrip the quickeſt veſſel, and might alſo be made to deviate from the courſe of the wind; as the water would form a counter-reſiſting medium, the want of which in air-balloons occaſions the difficulty of ſteering them. A ſail-balloon ſimilar to the above might alſo be advantageouſly attached to a land-carriage; namely, by increaſing the capacity of the balloon ſo that its power of aſcenſion being nearly equal to the weight of the appended carriage, the latter would be drawn along by the impulſion of the wind againſt the balloon and ſails, while the friction over the ground, by the ſmall overplus weight, may be reaſonably expected to afford a reſiſtance ſufficient to guide the machine, and allow of a deviation in the carriage of at leaſt eight points from the courſe of the wind *.

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[*] 'Tis a matter of ſurpriſe, that the various hints for directing balloons appear to lie dormant with their projectors, who ſeem indiſpoſed to make any attempts to carry their plans into execution: thus the inventions of profeſſor Danzel (Philoſophical Magazine, vol. iv.), alſo of Martin, and the propoſals for performing the ſame by means of eagles trained for the purpoſe; or by a reverſed parachute to retard the direct progreſs of the balloon, whereby leſs power will be neceſſary to impel it in a lateral direction; all theſe plans remain obſolete and unpractiſed from the time of their ſuggeſtion.

[†] According to the preſent mode of rowing balloons from the car, whatever deviation is made may be compared to the lee-way of a ſhip at ſea; the power being applied ſo as merely to prevent at a certain inſtant the car from following the balloon, which will therefore be impelled, during that ſhort interval, in a barely diagonal direction amounting perhaps, in the end of the voyage, to ſcarcely half a point from the wind. For oars to produce their full effect they ſhould be not leſs than twenty feet long, and three in width at their extremities; while the rowers ſhould be ſeated one on each ſide of the machine in appropriate cars attached to the body of the balloon, and nearly on a line with its equator: this might eaſily be practiſed with a balloon capable of carrying four perſons and ballaſt; the greater weight in the car below the machine, containing two travellers with ballaſt, being ſufficient to keep the whole apparatus in a ſteady poſition, and that more eſpecially if the ſame be ſuſpended at a conſiderable diſtance beneath the balloon. The oars recommended (being made of only varniſhed linen ſtretched over netting fixed to arms of pliable wood) might be eaſily worked, if a projecting ſtaff of wood were attached to each lateral oar, ſo as to form at its extremity a fulcrum whereon to reſt the oar at eight or ten feet diſtance from the rower.

To whatever degree of perfection aërial navigation may attain, the limits to which a traveller might ſoar will for ever be confined to but a ſmall diſtance, even ſuppoſing that man could exiſt in any ſtation of the aërial regions however elevated. The denſity of the atmoſphere decreaſing in a geometrical proportion, it will be found that, if a ſphere of ſheet copper †, of half a pound to the ſquare foot, were conſtructed of equal diameter with our earth (- 7920 miles), and totally exhauſted of its incloſed air, ſuch a globe would attain its equilibrium at 70·047346 miles diſtance from the ſurface of the earth; nor would it attain a greater elevation, although its power of aſcenſion at the inſtant of departure would be equal to 28716916379672707771712 pounds.

The application of aëroſtatic machines to the advancement of our knowledge of the various phænomena in meteorology, ſtands prominent, as the, perhaps, only means of maturing our acquaintance with cauſes yet known only by their effects. Their uſe will alſo be indicated in many urgent caſes where other means of conveyance might fall ſhort. At the ſame time I conclude with remarking, that the hitherto unſucceſsful

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* A land carriage to be moved by the wind is not a new contrivance: a machine of this kind, rigged in all reſpects like a ſloop, uſed occaſionally to be experimented upon on Barham Downs, near Canterbury: the carriage being of a light conſtruction, and containing two perſons, was found capable of running a mile in three minutes and a half, upon level ground and with a fair wind. Whether it could ſurmount the occaſional irregularities of a common road, or deviate in any meaſure from the wind, I do not now recollect.

† If metallic aëroſtats were ever to be conſtructed agreeably to the ſcheme of the jeſuit Francis Lana, means might be found to exhauſt them: but to prevent their being cruſhed together by the preſſure of the atmoſphere would be more difficult to accompliſh!

ful attempts to render aërial navigation of ſervice to mankind, ought to be no argument for cauſing it to be diſcountenanced by men of ſenſe, or prohibited by civil authority; as experiments in any art, however multiplied, if not well adviſed and conducted, cannot be expected to fulfil the erroneous expectations of their projectors.

To aſſiſt the artiſt in conſtructing his aëroſtatic apparatus, it may not be amiſs to ſubjoin the following data:—The diameter is to the circumference of a ſphere as 1 to 3$\frac{1}{7}$, or as 1 to 3·1416. The circumference multiplied by the diameter gives the ſurface of the ſphere in ſquare dimenſions, and 1-6th of the ſurface multiplied by the diameter, or the cube of the diameter multiplied by 0·5236, gives the capacity or ſolid contents in cubic meaſure. By the weight of a ſquare foot of the envelope may be found the weight of the whole bag, allowing for the ſeams; and to find the power of aſcenſion, ſubtract the weight of the bag and incloſed air from the weight of an equal bulk of atmoſpheric air *, and the quotient will give the power which the balloon will exert to riſe, which (during its aſcent) will decreaſe till the equilibrium between the two is reſtored; and this point may be nearly aſcertained by recollecting that the denſity of the atmoſphere decreaſes in the geometrical ratio, and is reduced to half an atmoſphere at three miles and a half from the earth's ſurface †. If a balloon with its annexed apparatus weighs therefore, at the inſtant of aſcent, but half an equal bulk of common air, it will riſe to three miles and a half perpendicular height; but, as the denſity of the inflammable air decreaſes alſo in the ratio of the diminiſhing preſſure of the ſurround-

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* A cubic foot of water weighs 1000 ounces avoirdupoiſe: and as the denſity of air to water is as 1 to 800, this will be 1¼ ounce for every cubic foot of atmoſpheric air: the weight of inflammable air varies according to its purity, ſo as to be even 17 times lighter than common air; but in the groſs way, for aeroſtatic purpoſes we muſt not expect to obtain it more than eight or ten times lighter than atmoſpheric air.

† Agreeably to the annexed table:

At miles			
3½	the air is	2	times rarer.
7		4	
14		16	
21		64	
28		256	
35		1 024	
42		4·096	
49		16·384	
56		65·536	
63		262 144	
70		1·048·576	

ing air, ſo if the balloon were originally but a quarter full (by which means room would be left for this ſubſequent expanſion of the gas), the machine would mount proportionally higher than the three miles and a half, by as much more as the relative denſity of the gas is become leſs. Thus, if, as is now allowed, we reckon the volume of all elaſtic fluids to be inverſely as the preſſure, and that the balloon was at firſt but a quarter full (yet of half its weight of power of aſcenſion), it would not ſtop till arrived at ſeven miles height, and the machine would be at its full diſtenſion, the denſity being here leſſened four times what it was at the moment of aſcenſion. But if the machine (whoſe power of aſcenſion is equal to half its whole weight) was at firſt half full, the gas would be expanded to twice its volume at three miles and a half; the balloon would then be full, and all further riſe would be prevented by the air eſcaping at the neck by all the ſubſequent rarefaction; and if this was prevented by tying the neck, the machine would burſt. But we are prevented from knowing to what preciſe height a balloon will riſe, from the yet unaccounted-for circumſtance of the temperature of the contained air always much exceeding that of the ſurrounding medium: and as there is not that coincidence in the expanſions of different gaſes by temperature, as is the caſe with reſpect to barometrical preſſure; ſo, till we are better acquainted with the cauſe of this diverſity of temperature, and the preciſe expanſion of the gas by the varying degrees of the ſame, we muſt reſt ſatisfied with a rough calculation in ſuppoſing the temperature of the internal and external air to correſpond *.

The annexed table (extracted from Mr. Cavallo's treatiſe) ſhows the proportion between the ſurfaces and capacities of ſpheres of various diameters, remembering that the ſpecific

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* From the mode of inflating aëroſtatic machines, and the diſſolving power of all elaſtic fluids, the contained gas will always hold in ſolution more or leſs water in an extremely divided ſtate. In the elevated and cold regions of the atmoſphere this vaporous fluid will be condenſed in ſmall drops in the inner concavity of the balloon; but the before-combined yet now free caloric (to which the fluid owed its vaporous ſtate, and which, in every condenſation of a fluid, or change of ſtate of its aggregation from aëriform elaſticity to a fluid or ſolid ſtate, is given out,) will be prevented from eſcaping to the external air by the non-conducting quality of the envelope: it will therefore ſerve to augment the temperature of the hydrogen gas; while in a warmer zone (or when a large and white cloud is ſo ſituated as to become a mirror to the balloon, ſtrongly reflecting the rays of the ſun upon the ſame,) the contrary effects will enſue—the water will reaſſume the vaporous ſtate, and the temperature of the gas be proportionally reduced, as far as relates to its contained water.

quantity of the number in the firſt column, whether reckoned as inches, feet, or yards, muſt be the ſame in each lateral column; as ſquare inches, feet, or yards, in ſurface—and cubical inches, feet, or yards, in capacity *.

Diam.	Surface.	Capacity	Diam.	Surface.	Capacity.
1	3·141	0·523	26	2124	9203
1½	7·068	1·767	27	2290	103[illegible]6
2	12·567	4·188	28	2463	11494
2½	19·635	8·181	29	2642	12770
3	28·274	14·137	30	2827	14137
4	50·265	33·51	31	3019	15598
5	78·54	65·45	32	3217	17157
6	113·097	113·097	33	3421	18817
7	153·938	179·594	34	3632	20580
8	201·062	268·083	35	3848	22449
9	254·469	381·704	36	4072	24429
10	314·159	523·6	37	4301	26522
11	380·1	696·9	38	4536	28731
12	452·5	904·8	39	4778	31060
13	530·9	115[illegible]·3	40	5026	33510
14	615·8	1436·7	45	6362	47713
15	706·9	1767·1	50	7854	65450
16	804·2	2145	55	9503	87114
17	907·9	2572	60	11310	113098
18	1017·9	3054	65	13273	143794
19	1134·1	3591	70	15394	179595
20	1256·6	4189	75	17671	220804
21	1385·4	4849	80	20106	268083
22	1520·5	5575	85	22698	321556
23	1661·9	6371	90	25447	381704
24	1809·6	7238	95	28353	448922
25	1963·5	8181	100	31416	523599

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* I have omitted to particularize the method of cutting the pieces compoſing a balloon, which will vary according to the width of the ſtuff; as the ſmalleſt ingenuity will enable any one to deſcribe a pattern, only recollecting the length in circumference of the machine required, together with its propoſed ſhape. Alſo the heights of the barometer are omitted (as requiring corrections for temperature too nice for any but philoſophical amateurs, who may, with propriety, be ſuppoſed to be already maſters of this eſſential branch of the ſcience) for the preſent purpoſe—only recollecting that, without regard to the difference of temperature, an elevation of 16 fathoms, or 96 feet, is uſually allowed for the fall of every tenth of an inch of the mercury.

POSTSCRIPT.

So long as the vibration of pendulums is performed in a medium of varying denſity, we muſt not look for an accurate time-piece for aſcertaining the longitude, &c.; unleſs a ſelf-correcting mercurial pendulum could be contrived, adapted to counteract the ſmalleſt variations effected by the ambient air. The errors of a time-piece are but half corrected by the fabrication of pendulums adapted to obviate the expanſion of metals by increaſe of temperature, if the works themſelves ſtill remain conſtructed of ſuch expanſible materials. A correct time-piece, therefore, will be that of which not only the works and pendulum are conſtructed of the leaſt expanſible materials, but the pendulum itſelf ſhall vibrate in a medium of unalterable denſity; a deſideratum only to be obtained by cauſing the vibrations to be performed *in vacuo*, or by a ſelf-correcting pendulum, as above alluded to. I know of no ſubſtance ſo well adapted to compoſe the works of ſuch a machine as ivory, or the horn of the narwhal or ſea unicorn (nearly entirely compoſed of enamel, and uſed in the fabrication of artificial teeth), eſpecially if we had a means of increaſing their hardneſs ſo as to vie with the metals (a ſubject on which I propoſe to make ſome experiments): but for the pendulum itſelf, the common gridiron pendulum is allowed to be the moſt accurate in uſe, provided its vibrations were not obſtructed by unequal reſiſtance from the air: but for a mercurial pendulum to move in open ſpace, glaſs will be indicated as the leaſt expanſible and moſt proper ſubſtance for this purpoſe. I add, that the moſt perfect time-piece hitherto conſtructed, has been ſo only in as far as the works have been moſt exactly accurate, and the expanſion of the pendulum been counteracted (as in the gridiron pendulum) by oppoſing expanſion to expanſion. The exact concordance of ſuch a machine, on its return to the firſt place of obſervation, will have depended on the nature of the voyage performed in the interim. As, if ſuch voyage has been performed acroſs the equator and back (as to the Eaſt Indies), the aggregate number of anomalies in the pendulum in the voyage out, will in the return be nearly balanced by the number of anomalies in the voyage home, ſo that the whole difference ſhall at laſt, perhaps, not amount to 100″. But if (ſome weeks after the ſailing of the veſſel) we had it in our power to know at a certain inſtant the preciſe moment indicated by an equally accurate chronometer left at the place of departure, we ſhould find a greater variation between the two, owing to the difference in the denſities of the media in which the pendulum at home and that on board the ſhip have in the interim vibrated. I ſay then,

1. That the concordance (although exact) of two chronometers, after a voyage on the part of one of them, is no proof of their accuracy.

2. That this concordance will depend on the nature of the voyage that has been performed in the interim. And,

3. That we muſt not expect an exact inſtrument till the expanſions by heat, and the varying denſity of the atmoſphere, are counteracted in the ſame machine.

For although the difference of an inch barometrical preſſure may not cauſe an obſtruction in the arc of vibration equal to the two millionth part of the whole, yet this ſmall difference (which, perhaps, may have prevented its hitherto due obſervance,) will, in the ſpace of ſome months, amount to ſomething conſiderable, eſpecially as a variation of not only one, but two or even three inches increaſe or diminution of preſſure will frequently enſue in leſs than 24 hours.

Kennington,
January 17, 1803.

VI. *A Sketch of the Geography of Cochin China; ſome Particulars relative to the Manners, Cuſtoms, and Hiſtory of the Inhabitants; and a few Conſiderations on the Importance of forming an Eſtabliſhment in that Country* *.

COCHIN CHINA, called by the natives Anam, extends from about the 20th degree of north latitude to Pulo Condore, which lies in 8° 40′. It is bounded by the kingdom of Tonquin on the north, from which it is ſeparated by the river Sungen; by the kingdom of Laos, and by a range of mountains, which divides it from Cambodia, on the weſt; and by that part of the Eaſtern Ocean generally called the China Sea, on the ſouth and eaſt.

The kingdom is divided into twelve provinces, all lying upon the ſea-coaſt, and ſucceeding each other from north to ſouth in the following order:

Ding-oie, Cong-bing, Ding-cat, Hue (or the Court), in he poſſeſſion of the Tonquineſe. *Cham, Cong-nai, Quinion,* in the poſſeſſion of Ignaack. *Phu-yen, Bing-khang, Nab-tong,* Bing-thoam (or *Champa*), dubious whether ſubdued by Ignaack, or ſtill in the poſſeſſion of the king. *Donai,* in the poſſeſſion of the king.

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* From the *Aſiatic Annual Regiſter for* 1801.

The breadth of the country bears no proportion to its length. Few of the provinces extend further than a degree from eaſt to weſt, ſome leſs than twenty miles: 'Donai, which is properly a province of Cambodia, is much larger.

The whole country is interſected by rivers, which, although not large enough to admit of veſſels of great burthen, yet are exceedingly well calculated for promoting inland commerce.

The climate is healthy, the violent heat of the ſummer months being tempered by regular breezes from the ſea. September, October, and November, are the ſeaſon of the rains; the low lands are then ſuddenly overflowed by immenſe torrents of water which fall from the mountains. The inundations happen generally once a fortnight, and laſt for three or four days. In December, January, and February, there are frequently rains brought by cold northerly winds, which diſtinguiſh this country with a winter different from any other in the eaſt. The inundations have the ſame effect here as the overflowings of the Nile in Egypt, and render the country one of the moſt fruitful in the world. In many parts the land produces three crops of grain in the year. All the fruits of India are found here in the greateſt perfection, with many of thoſe of China.

No country in the eaſt produces richer or a greater variety of articles proper for carrying on an advantageous commerce; cinnamon, pepper, cardemoms, ſilk, cotton, ſugar, Agula wood, Japan wood, ivory, &c. Gold is taken almoſt pure from the mines; and, before the troubles, great quantities were brought from the hills in duſt, and bartered by the rude inhabitants of them for rice, cloths, and iron. It was from them alſo the Agula and Calambae woods were procured, with quantities of wax, honey, and ivory.

The animals of Cochin China are bullocks, goats, ſwine, buffaloes, elephants, camels, and horſes. In the woods are found the wild boar, tiger, and rhinoceros, with plenty of deer; the poultry is excellent, and the fiſh caught on the coaſt abundant and delicious. The fleſh of the elephant is accounted a great dainty by the Cochin Chineſe. The breeding of bullocks is little attended to; their fleſh is not eſteemed as food, and they are made no uſe of in tilling the land, which is performed by buffaloes. They are totally unacquainted with the art of milking their cattle.

The aborigines of Cochin China are called Moys, and are the people which inhabit the chain of mountains which ſeparate it from Cambodia. To theſe ſtrong-holds they were driven when the preſent poſſeſſors invaded the country. They are a ſavage race of people, very black, and reſemble in their features the Caffrees.

It was about the year 1280 of the Chriſtian æra that the firſt Tartar prince became poſſeſſed of the throne of China. This revolution afforded an opportunity to the weſtern provinces to throw off their dependence; and they were formed into a kingdom, under a prince whoſe deſcendant now reigns in Tonquin, and is called Knaw-Whang. About the beginning of the fifteenth century a large body of people from theſe provinces, being diſaffected to the government, joined under a leader of abilities; they ſoon became maſters of the coaſt of Cochin China as far as Cape Aurilla, which lies in latitude 12° 30′ north. The Moys, the original inhabitants, retired to the hills bordering their country to the weſtward; where they have ever ſince remained. The emigrants, under their conductor, founded the kingdom of Cochin China. The continual wars they were engaged in with the Tonquineſe, induced them to build a wall on the ſouthern extremity of the province of Ding-noi, to prevent their irruptions. Every communication by ſea was ſtrictly forbidden. In the year 1764 the country of Cochin China was in a flouriſhing condition, and governed by a prince of abilities: ſoon after his ſon ſucceeded to the throne, and anarchy and confuſion enſued.

The Cochin Chineſe bear evident marks of their being derived from the ſame ſtock as the Chineſe. They reſemble them in their features, and moſt of their manners and cuſtoms. Their religion is the ſame: their oral language, though different, ſeems formed upon the ſame principles; and they uſe the ſame characters in writing. They are a courteous, affable, inoffenſive race, rather inclined to indolence. The ladies are by far the moſt active ſex; they uſually do all the buſineſs, while their lazy lords ſit upon their haunches, ſmoking, chewing beetle, or ſipping tea. Contrary to the cuſtom of China, the ladies are not ſhut up; and, if unmarried, a temporary connection with ſtrangers who arrive in the country is deemed no diſhonour. Merchants often employ them as their factors and brokers, and, 'tis ſaid, the firmeſt reliance may be placed on their fidelity.

The habit of the men and women is cut after the ſame faſhion. It is a looſe robe, buttoning with a ſmall robe round the neck, and folding over the breaſt like a banyan gown, with large long ſleeves which cover the hands. People of rank, and eſpecially the ladies, wear ſeveral of theſe gowns one over the other; the undermoſt one reaches to the ground, the ſucceeding ones are each ſhorter than the other, ſo that the diſplay of the different colours makes a gaudy appearance as they walk along.

Such are the few particulars relative to Cochin China. It now remains to ſhow how a connection with Cochin China may prove beneficial to this country. The drain of ſpecie from the company's ſettlements in India is become a matter of ſuch ſerious import, that any plan which may be offered to remedy ſo growing an evil, I have no doubt, will be deemed worthy of obſervation. I am ſanguine in my expectations, that a ſettlement in Cochin China would conduce to that deſirable end, as well as be productive of many other advantages.

Our two little veſſels brought from Cochin China to the amount of 60,000 rupees in gold and ſilver bullion. The Rumbold, the year before, alſo brought bullion to a conſiderable amount, on account of ſales of Bengal and Madras cloths, opium, iron, copper, lead, hardware, and glaſs.

The ſituation of Cochin China is excellently well adapted to commerce. Its vicinity to China, Tonquin, Japan, Cambodia, Siam, the Malay coaſt, the Philippines, Borneo, the Moluccas, &c. renders the intercourſe with all theſe countries ſhort and eaſy. The commodious harbours formed on the coaſt, particularly that of Turon, afford a ſafe retreat for ſhips of any burthen during the moſt tempeſtuous ſeaſons of the year.

The nations of Europe, having hitherto found it impoſſible to provide cargoes ſufficiently valuable to barter for the commodities of China, are obliged to make up the deficiency by ſending thither immenſe quantities of bullion; by which means it has, for a number of years paſt, drained the eaſtern and weſtern worlds of their ſpecie. The number of junks annually reſorting to Cochin China, plainly proves how much the productions of it are in demand amongſt the Chineſe. Theſe productions, had we a ſettlement and a confirmed influence in the country, might with eaſe be brought to centre with us, purchaſed with the ſtaples of India and of Europe. Turon would become the emporium for them, where our ſhips bound to Canton, from whence it is only five days ſail, might call and receive them. It would prove a ſaving of ſo much ſpecie to Great Britain or India as the value of the commodities amounted to in China. In a few years, there is every reaſon to believe a very conſiderable inveſtment might be provided.

Our trade to China has ever been burthened with enormous impoſts and exactions: theſe, under various pretences, are annually increaſing, and in proceſs of time may become inſupportable. It is an opinion, latterly grown current, that the Chineſe are deſirous of totally excluding all Europeans from their country. May we not hazard a conjecture, that the vexations they oblige them to ſuffer are the premeditated ſchemes of this politic people to effect it? Were ſuch an event to happen, the want of a ſettlement to the eaſtward would be ſeverely felt; the Chineſe would export their own commodities, and Java or the Philippines, as the neareſt ports, would become the marts for them. As there is no reaſon to ſuppoſe that our inability to procure them from the firſt land would hinder their conſumption, we muſt buy them either from the Dutch or from the Spaniards. A ſettlement in Cochin China will give us a ſuperior advantage to either, both as its ſituation is nearer, and the Chineſe are more accuſtomed to reſort thither: at all events there is reaſon to ſuppoſe it will enable us to procure the commodities of China at a much more reaſonable rate than now purchaſed by our factors at Canton, and certainly on leſs humiliating terms to the nation at large. Colonies of Chineſe have from time to time emigrated from the parent country, and fixed their abode in different parts of Cochin China. Theſe have their correſpondence in every ſea-port of the empire. Through their means, teas, china ware, and the various other articles, the objects of our commerce with China, might be imported in junks to our own ſettlements, equally good in quality, and cheaper, as the Chineſe are exempted from the exorbitant duties levied on foreigners. Some of the beſt workmen might be encouraged to ſettle in Cochin China, and, under direction, manufactories carried to as great a degree of perfection as in China itſelf.

The intercourſe between Japan and Cochin China might be renewed, and we might participate in a trade for many years monopolized by the Dutch. An advantageous trade might be carried on with the Philippine iſlands, and Madras and Bengal goods introduced amongſt them, by means of the junks, for the conſumption of Spaniſh America. The Siameſe and Cambodians would bring the produce of their reſpective countries, and barter or ſell them for ſuch articles as they wanted from Cochin China. Amongſt them it is probable a ſale might be found for quantities of Bengal cloths. The lower claſs of people in Cochin China are, for the moſt part, clothed in cangas, a coarſe cotton cloth brought from China; but the preference, which I had an opportunity of obſerving, they gave to Bengal cloths, on account of their being wider and cheaper, would ſoon induce them to adopt the uſe of them. The demand for opium, already in ſome meaſure become a neceſſary of life to the Chineſe, would increaſe in proportion to the facility of procuring it. The importation of it, no longer confined to Canton, but carried by the junks to every ſea-port in the country, would ſpread the demand for this drug to the remoteſt parts of the empire.

But what inſpires the moſt flattering hopes from an eſtabliſhment in this country is its rich gold mines, celebrated for ages as producing the richeſt ore, ſo pure, that the ſimple action of fire is ſaid to be ſufficient to refine it. I omitted no opportunity of making inquiries reſpecting this valuable article, and was told that mines were formed in different parts of the northern provinces, and particularly in Hué, where the ore lay ſo near the ſurface of the earth that it was dug up with little labour. Under the direction of a ſkilful metallurgiſt, what might not be expected from ſuch a ſource!

Great as the commercial advantages are, the political ones reſulting from a ſettlement in Cochin China would be ſcarce inferior. Turon Bay would not only afford a ſecure retreat to our Indiamen in caſe of loſing their paſſage to China, but from thence we might alſo intercept the fleets of any hoſtile power, either going to or returning from that country. We ſhould become formidable neighbours to the Dutch and to the Spaniards, and, in the event of a war with either of them, attack, with advantage, their moſt valuable ſettlements.

Should the company be induced to form a ſettlement in Cochin China, it may be effected on principles ſtrictly juſt, and at a ſmall expenſe. Several of the royal family, beſides the mandarins who were in Bengal, with many officers of the late government, urged me to uſe my endeavours with the government of Bengal to induce it to afford them aſſiſtance, promiſing a powerful ſupport whenever we ſhould heartily join in their cauſe. To reſtore their lawful ſovereign to the throne, would be now a meaſure ſo popular, that the ſincerity of their offers cannot be doubted. To relieve an unhappy people, groaning under the weight of the moſt cruel oppreſſion, would be an act worthy of the Britiſh nation. Fifty European infantry, half that number of artillery, and two hundred ſepoys, would be ſufficient for this and every other purpoſe. The natives of Cochin China are infinitely below the inhabitants of Hinduſtan in military knowledge; I have, however, no doubt that a body of them, well diſciplined and regularly paid, would prove as faithful to us, and contribute as much to the ſecurity of any poſſeſſions which we might acquire to the eaſtward, as the ſepoys do to our territories in

India. In caſe of any diſtant expeditions, they would be found ſuperior, being entirely free from all religious prejudices, and having no objection to the ſea.

While Cochin China remains in its preſent diſtracted ſtate, a favourable opening is preſented to the firſt European nation that may attempt to obtain a footing in the country. Should the company, therefore, entertain a deſign of forming an eſtabliſhment in Cochin China, no time ſhould be loſt in carrying it into execution.

VII. *Report preſented to the Claſs of the Exact Sciences of the Academy of Turin, 15th Auguſt 1802, in regard to the Galvanic Experiments made by* C. VASSALI-EANDI, GIULIO, *and* ROSSI, *on the 10th and 14th of the ſame Month, on the Head and Trunk of three Men a ſhort Time after their Decapitation. By* C. GIULIO.

THE Firſt Conſul, in a letter to Chaptal, in which he announced to that miniſter the two prizes he had founded to encourage philoſophers to make new reſearches in regard to Galvaniſm, ſays, "Galvaniſm, in my opinion, will lead to great diſcoveries." This obſervation was juſt and profound: great diſcoveries have already been made; Galvani and Volta have immortalized their names, and ſeveral celebrated philoſophers and phyſiologiſts have rendered themſelves illuſtrious in this branch of ſcience, ſo abundant in aſtoniſhing phænomena: yet it is only in its infancy, and there can be no doubt that many important diſcoveries ſtill remain to be made.

Vaſſali, Roſſi, and myſelf, have for ſeveral years been employed in reſearches on this ſubject. While the firſt examined the Galvanic fluid in every point of view, for the purpoſe of illuſtrating its nature by means of a great number of ingenious experiments, performed with that care and exactneſs which are peculiar to him, Roſſi and myſelf attempted to explain the action of the Galvanic fluid on the different organs of the animal economy.

Sometimes I was obliged to interrupt my reſearches by unfortunate circumſtances, and at others by my adminiſtrative functions: but I have now reſumed them; and though ſucceſs has not yet crowned my efforts by any brilliant diſcovery, we truſt, and with confidence, that we ſhall be able to add ſome valuable facts to the hiſtory of the animal economy; to rectify errors; to confirm facts already received; and to extend the domain of an inexhauſtible agent fertile in wonders.

Volta had announced that the involuntary organs, ſuch as the heart, the ſtomach, the inteſtines, the bladder and veſſels, are inſenſible to the Galvanic action[*]: but we have fully refuted this great phyſiological error. Unfortunately, however, the Latin memoir containing the deciſive experiments which we made on cold-blooded and warm-blooded animals in 1792, preſented to the Academy ſoon after, and which, according to Sue, in his Hiſtory of Galvaniſm[†], "are curious, and contain very intereſting obſervations," did not appear till 1801, when it was printed in the laſt volume of the Tranſactions of the Academy.

In that interval Grapengieſſer found, as we had done, that Galvaniſm, by means of zinc and ſilver[‡], has an influence on the periſtaltic motion. Humboldt aſcertained the Galvanic action on the hearts of frogs, lizards, toads, and fiſhes. Smuch obſerved the excitability of the heart by the Galvanic fluid; and Fowler changed the pulſations of the heart without the immediate application to it of armatures, and only by adapting them in warm-blooded animals to the recurrent nerve by means of the ſympathetic[§].

It is chiefly in regard to the experiments of theſe learned Germans that the hiſtorian of Galvaniſm ſtates[||], that the involuntary vermicular motion of the inteſtines, according to the acknowledgment of all phyſiologiſts, obeys metallic irritation; whence it follows, ſays he, that the Italian philoſophers have advanced an error when they ſaid that Galvaniſm exerciſes no action but on the muſcles, which depend on the will. As an accurate and impartial hiſtorian, how can Sue accuſe the Italian philoſophers indiſcriminately of ſuch an error, ſince he had our memoir before him when employed on the ſecond volume of his Hiſtory of Galvaniſm, and ſince he gave a ſhort account of my experiments in his firſt volume? Nay, I gave an account of my experiments in a ſmall work publiſhed in Italian in 1792. But as Italian works are not much read in France, and were leſs ſo at that period, I ſhould not have reproached C. Sue with this act of injuſtice, and his incorrectneſs in regard to the Italian philoſophers, had not my Latin memoir been known to him, as it had appeared in the Tranſactions of the Academy.

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[*] Mezzini, Volta, Valli, Klein, Pfaff, Berhends, have denied that the heart could be moved by the Galvanic fluid. *Hiſt. du Galvaniſme*, part i. p. 145. Bichat could obtain no contractions either in the heart of man or that of the dog. See *Récherches Phyſiologiques ſur la Vie et la Mort.*

[†] Towards the end of the firſt part.

[‡] See *Hiſtoire du Galvaniſme*, vol. ii. p. 81.

[§] Ibid, vol. ii. p. 84. [||] Vol. ii. p. 83.

Though we made a great many experiments before we attempted to combat a philoſopher ſo juſtly celebrated as Volta, and to eſtabliſh the influence of Galvaniſm on the involuntary organs; and though Grapengieſſer, Humboldt, Smuch, Fowler, &c. aſcertained this influence in certain cold-blooded and even warm-blooded animals; an object of ſo much importance to phyſiology required to be extended and confirmed, eſpecially in man, by new experiments. We have been the more ſenſible of the neceſſity of eſtabliſhing this fact in an inconteſtable manner, either in regard to the involuntary organs in general, or more particularly the heart, as the celebrated Aldini, profeſſor of Bologna, in an Italian work replete with new facts and valuable experiments made on the bodies of decapitated criminals, has been obliged to acknowledge that he was not able to obtain any contraction in that organ by means of the electro-motor of Volta, which is ſo powerful.

We ſhall give an account, in particular memoirs, of the experiments we have already made, and of thoſe which we propoſe to execute. In regard to the ſtomach, the large and the ſmall inteſtines, and the bladder, we ſhall ſay only, in a general manner, that by armature of the different nervous branches we obtained contractions analogous to thoſe deſcribed in regard to animals. The Galvanic action on the heart and arteries is the object of the preſent paper, as it is of the utmoſt importance to phyſiology, and deſerves, under every point of view, to excite our attention and occupy our reflections.

Our experiments on the different parts of the head and trunk of the decapitated criminals were begun, on the 10th of Auguſt, in a hall of the large hoſpital of St. John, and reſumed and continued yeſterday in the anatomical theatre of the univerſity, before a great number of ſpectators.

We tried the influence of Galvaniſm on the heart in three different ways:

1ſt, In arming the ſpinal marrow by means of a cylinder of lead introduced into the canal of the cervical vertebræ, and then conveying one extremity of a ſilver arc over the ſurface of the heart, and the other to the arming of the ſpinal marrow. The heart of the firſt individual ſubjected to our experiments immediately exhibited very viſible and very ſtrong contractions. Theſe experiments, as ſeen, were made without any intervention of the pile, and without any armature applied to the heart. It is very remarkable, that when the former is touched firſt, and then the arming and ſpinal marrow, the contractions of the heart which follow are more inſtantaneous, and ſtronger, than when the arming of the ſpinal marrow is firſt touched, and then the heart. In a memoir on Galvaniſm, read in the laſt public ſitting of the academy, I gave an account of a great number of experiments, made eſpecially on frogs, which exhibited a ſimilar phænomenon. In theſe animals I obſerved, a great number of times, that when the arming of the crural nerves was touched firſt, and then the muſcles of the thigh, there were no contractions, or the contractions were exceedingly weak; and, on the other hand, that when the muſcles of the thighs were firſt touched, and then the arming of the crural nerves, as long as the leaſt vitality remained in the organs the contractions of the muſcles were conſtant and violent. In the memoir already mentioned I have endeavoured to account for this phænomenon, to which I ſhall recur when, by a great number of trials, we ſhall have aſcertained that it is as general in men as I found it in frogs and other cold-blooded animals.

The ſecond manner in which we tried the influence of Galvaniſm on the heart was by arming the nervi vagi and the large ſympathetic. The object of theſe experiments will be readily comprehended by anatomiſts acquainted with the details of neurology. In theſe, as well as in the firſt and other experiments where we armed the cardiac nerves themſelves, we obtained contractions in the heart. In this, as in the former caſe, the contractions obtained when the heart was firſt touched, and then the arming of the nerves, were much ſtronger than when the arming of the nerves was touched firſt, and then the heart. In this method we even obſerved that the Galvanic experiments ſometimes failed.

The third kind of experiments on the heart were performed by means of the pile. The pile we employed on the 10th of Auguſt, for the experiments on the firſt decapitated criminal, was compoſed of fifty plates of ſilver and as many of zinc, with paſteboard moiſtened with a ſtrong ſolution of muriate of ſoda. The ſilver was mixed with a tenth part of copper. This is the proportion which we found moſt favourable to the intenſity of the ſigns of Galvaniſm:

	Metre.
The diameter of the ſilver plates was -	0·036
Their thickneſs - - -	0·0015

The dimenſions of the pieces of paſteboard were the ſame.

The

	Metre.
The diameter of the zinc plates was -	0·042
Their thickneſs - - -	0·0035

The pile employed for the experiments on the 15th of Auguſt was compoſed of fifty plates of pure ſilver, and twice that number of plates of zinc and pieces of paſteboard; the latter moiſtened in a ſolution of muriate of ſoda.

	Metre.
The diameter of the ſilver plates was -	0·038
Their thickneſs - - -	0·001

The dimenſions of the pieces of paſteboard were the ſame.

	Metre.
The diameter of the zinc plates was -	0·04
Their thickneſs - - -	0·001

By making the negative extremity of the pile to communicate, by means of reſpective conductors, with the ſpinal marrow, or merely with the muſcles of the back or breaſt, laid bare, and the poſitive extremity immediately with the heart, inſtantaneous and violent contractions were obtained; and the contractions were produced alſo when the heart was made to communicate with the negative extremity of the pile, and the ſpinal marrow with the poſitive extremity.

We ſhall obſerve, in regard to contractions of the heart, that of all its parts the apex is the moſt ſuſceptible of motion, and the moſt ſenſible to the Galvanic influence: we muſt obſerve alſo, that the contractions produced by communication with the pile were not only ſtrong, but that they continued a long time even after all the communication was removed.

A very remarkable circumſtance is, that the heart, which of all the muſcles retains longeſt, in general, its contractility in regard to mechanical ſtimulants, is the firſt to become inſenſible to the Galvanic influence. The muſcles of the arms, and thoſe of the back and breaſt, continue to be excitable by Galvaniſm for whole hours; and the heart had loſt its excitability about forty minutes after death.

The experiments made yeſterday in the anatomical theatre exhibited nearly the ſame reſults in regard to the heart as thoſe already mentioned. The great arteries, ſuch as the aorta and ſome of its branches, being injected with water raiſed nearly to the ſame temperament as that of the blood in the living individual, when ſubjected to the Galvanic action exhibited contractions. But it is probable that they will appear ſtronger when trials of this kind ſhall be made on bodies endowed with a higher degree of vitality than thoſe of yeſterday, and when the interval between the period of decapitation and that of the experiments ſhall be leſs. With this view, indeed, we have provided a hall much nearer to the place of execution; for the reſults which we obtained in the man decapitated on the 10th of Auguſt, in which caſe the experiments were begun five minutes after the decapitation, were all comparatively more ſtriking, and ſtronger, than thoſe obtained in the experiments of yeſterday, which were begun more than twenty minutes after decapitation, and which were performed, as appears, on bodies endowed with a much weaker degree of vitality.

In the experiments made on the arteries, we armed the nervous plexus, which envelop the trunks of the cœliac and meſenteric arteries, ſeveral branches of which are even interwoven around the aorta: a communication was eſtabliſhed between the poſitive or negative extremity of the pile and the aortic artery itſelf. It was by theſe means that we obtained riſible contractions.

If the effects of Galvaniſm on arterial contractions are conſtant, as I preſume, all thoſe diſcuſſions which have been agitated ſo long, and with ſo much violence, in regard to the irritability of the arteries, which does not manifeſt itſelf by the action of different mechanical and chemical ſtimulants, will at length be terminated in a poſitive and irrefragable manner; all doubts will at length be removed; and we ſhall be indebted to the Galvanic fluid, which is the moſt energetic of all agents applied to the animal fibre, for having fixed the opinions of phyſiologiſts on a point of ſo much importance to the animal economy.

Whence comes it that Aldini, even with the help of the moſt powerful electro-motors, was not able to obtain contractions in the heart of man, which we ſo evidently obtained by the ſame means which always withſtood his efforts? How happens it that we obtained contractions by means much weaker?

The firſt experiments of Aldini on the human heart were begun an hour and a half after death [*]. The trunk had been expoſed a long time to the open air, the temperature of which was no more than + 2. It is probable that the cold, and the long interval between the period of death and that of the experiment, had already annihilated the irritability of the heart [†]. In the fifty-third experiment, the heart of another executed

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[*] Saggio di Sperienze ſul Galvaniſmo di Gioani Aldini; Bolonia 1802, p. 14, eſp. 28.

[†] If the celebrated Bichat failed in his experiments on the human heart, as well as Aldini, it was, perhaps, owing to the ſame cauſes. The temperature

Nor is its deficiency of gravity ſufficient to exclude it from the claſs of material ſubſtances. Such nicety of arrangement might, with equal propriety, lead us to deny the materiality of light, the gravity of which has never yet been proved; for, beſides the experiments of Mr. Michell, which failed in aſcertaining this property of light, we have ſeveral chemical facts tending to the ſame concluſion. Thus Mr. Cavendiſh, after firing a mixture of hydrogenous and oxygenous gaſes in a cloſe veſſel, a proceſs during which much light is always emitted, found not the ſmalleſt diminution of weight.

To have completed this defence of the material nature of heat, it would have been proper to have pointed out the circumſtances in which the phænomena of heat differ from the known and acknowledged phænomena of motion. At preſent, however, I have not leiſure to purſue the ſubject at much length; and though ſeveral points of diſagreement would doubtleſs be found, I ſhall mention only one of the moſt marked and deciſive.

Motion is an attribute of matter, independently of which it cannot poſſibly ſubſiſt. If, therefore, the phænomena of heat can be ſhown to take place where matter is not preſent, we ſhall derive from the fact a concluſive argument againſt that theory of heat which aſſigns motion as its cauſe. Now, in the experiment of Count Rumford, before alluded to, heat paſſed through a torricellian vacuum, in which, it need hardly be obſerved, nothing could be preſent to tranſport or propagate motion. This experiment, in my opinion, decidedly proves that heat can ſubſiſt independently of other matter, and conſequently of motion; in other words, *that heat is a diſtinct and peculiar body*[*].

[*] The argument at p. 49, which is the baſis of my objections to the commonly employed mode of aſcertaining ſpecific caloric, I fear, is not ſo fully and clearly ſtated as the abſtruſe nature of the ſubject requires.

Aſſuming two bodies, A and B, to be at the point of privation of temperature, or to poſſeſs no free caloric whatſoever, the quantity of combined caloric in each, according to Dr. Crawford's theory, is *directly* proportional to the quantities of heat neceſſary to produce equal elevations of temperature in the two bodies. Thus, if to attain a given temperature, A require caloric as 20, and B only as 10, the combined caloric of A, before this addition, is inferred to have borne to that of B the ratio of 2 to 1. But it might, with equal or perhaps greater probability, have been aſſumed that the combined caloric of A and B is *inverſely* proportional to the quantities of heat required to produce a given temperature; that A, for example, to attain a certain temperature, has abſorbed more caloric than B, becauſe in A leſs caloric exiſted previouſly in a ſtate of chemical union.

the frontal muſcles, thoſe of the eye-lids, the face, the lower jaw, and the tongue; when they beheld the convulſions of the muſcles of the arms, the breaſt, and of the back, which raiſed the trunk ſome inches from the table; the contractions of the pectoral muſcles, and the exterior and interior intercoſtal muſcles, which diminiſhed the intervals between all the ribs, and made them approach each other with violence, raiſing the inferior ones towards the ſuperior, and the latter towards the firſt rib and the clavicle; the contractions of the arms, which, when the uncovered biceps muſcle was touched, as well as its tendon, were ſo ſpeedy and violent, that complete flexion of the fore-arm on the arm took place, and that the hand raiſed weights of ſome pounds fifty minutes after decapitation. Similar experiments may be ſeen in the work of Aldini: our object in this report was merely to ſpeak of the Galvanic influence on the heart and arteries of man, which had not yet been obſerved.

Theſe new and important reſults, which we obtained in regard to the heart and arteries of man, will be confirmed by other trials. We ſhall repeat our experiments as ſoon as an opportunity occurs, and we ſhall take care to give you an early account of the moſt remarkable obſervations we ſhall make.

VIII. *A Review of ſome Experiments which have been ſuppoſed to diſprove the Materiality of Heat.* *By* William Henry [*].

THE following remarks on the ſubject of heat were written ſoon after the publication of Count Rumford's Inquiry concerning the Source of the Heat evolved by Friction; and of the intereſting Eſſays of Mr. Davy, which appeared in Dr. Beddoes's Weſt Country Contributions. They were tranſmitted to Dr. Beddoes, for publication, about the cloſe of the year 1799; but circumſtances, with which I am unacquainted, have, I believe, induced the doctor to decline the continuation of his periodical work. Theſe circumſtances I deem it neceſſary to ſtate; becauſe, had the eſſay been written nearer the period of its publication, it would probably have aſſumed a very different form. At preſent, I have not leiſure to review the ſubject, or to attempt any material alteration; and ſtill leſs to examine whether I have been anticipated by any of the authors whoſe eſſays have been publiſhed during the two laſt years.

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[*] From the *Mancheſter Society's Tranſactions*, vol. v. part 2.

A Review

A Review of ſome Experiments which have been ſuppoſed to diſprove the Materiality of Heat.

It has long been a queſtion among philoſophers, whether the ſenſation of heat, and the claſs of phænomena ariſing from the ſame cauſe, be produced by a peculiar kind of matter, or by motion of the particles of bodies in general. The former of theſe opinions, though far from being univerſally admitted, is now moſt generally received; and the peculiar body, to which the phænomena of heat are referred, has been denominated by M. Lavoiſier caloric. Againſt the doctrine of the French ſchool, ſome forcible arguments have lately been advanced by Count Rumford and by Mr. Davy, both of whom have adopted that theory reſpecting heat, which aſſigns as its cauſe a motion among the particles of bodies.

The method of reaſoning employed by Mr. Davy in proving the immateriality of the cauſe of heat, is the *reductio ad abſurdum*, i. e. the oppugned theory is aſſumed as true, together with its applications; and facts are adduced directly contradictory of the aſſumed principles: I ſhall take the liberty of offering a ſtatement of the argument, rather different from that of Mr. Davy, though, I truſt, without miſrepreſentation, or any material omiſſion.

Let heat be conſidered as matter; and let it be granted, that the temperature of bodies depends on the preſence of uncombined caloric. Now, if the temperature of a body be increaſed, the free caloric, occaſioning that elevation, muſt proceed from one of two ſources: either, 1ſtly, It may be communicated by ſurrounding ſubſtances; or, 2dly, It may proceed from an internal ſource, *i. e.* from a diſengagement of what before exiſted in the body, latent or combined. But the temperature of bodies is uniformly increaſed by friction and percuſſion, and neceſſarily in one of the foregoing modes.

I. Mr. Davy found, by experiment, that a thin metallic plate was heated, by friction in the exhauſted receiver of an air-pump, even when the apparatus was inſulated, from bodies capable of ſupplying caloric, by being placed on ice. This experiment he conſiders as demonſtrating, that the evolved caloric could not be communicated by ſurrounding bodies.

To the inference deduced from this experiment it may be objected, that the mode of inſulation was by no means perfect. Admitting the vacuum, produced by the air-pump, to have been complete, ſtill the ſupply of caloric could not thus be entirely cut off, ſince it has been ſhown by Count Rumford that caloric paſſes even through a torricellian vacuum. If, therefore, friction produce in bodies ſome change, which enables them to attract caloric from ſurrounding ſubſtances, this attraction may be equally efficient in an exhauſted receiver as in one containing an atmoſphere of mean denſity. It would be an intereſting ſubject of experiment to determine the influence of atmoſpheres of various denſities as conductors of caloric; for, ſince effects are proportionate to their cauſes, and it is aſcertained that common air conducts caloric (better than it is conveyed through a vacuum) as 1000 is to 702, it may be expected that the ratio will hold in all intermediate degrees.

In Count Rumford's maſterly experiment, the metal ſubmitted to friction was encompaſſed by water, and air was carefully excluded from the ſurfaces in motion: yet the water became hot, and was kept boiling a conſiderable time. In this caſe, the only obvious ſource of caloric, from without, was through the borer employed in producing the friction; if it be true, as the Count has obſerved, that the water could not, at the ſame inſtant, be in the act of giving out and receiving heat. The ſame objection to the communication of heat, from an external ſource, exiſts alſo in thus explaining Mr. Davy's experiment: but I cannot admit that the argument is demonſtrative in proving the evolved caloric not to be derived from external ſubſtances; for no abſurdity is implied in ſuppoſing that a body may be receiving caloric in one ſtate, and giving it out in another. We have an example of the ſimultaneous admiſſion and extrication of a ſubtile fluid, the materiality of which is admitted by Mr. Davy, in an excited electric, which, at the very ſame inſtant, receives the electric fluid from without, and transfers it to the neighbouring conductors. In an ignited body, alſo, the two proceſſes of abſorption and irradiation of light are, perhaps, taking place at the ſame moment.

II. Another cauſe of the increaſe of temperature in bodies, is the liberation of their combined caloric; and if this be a ſource of temperature, the abſolute quantity of caloric in a body muſt be diminiſhed by friction. That no ſuch diminution really takes place, we have the evidence of two experiments,—the one of Mr. Davy, the other of Count Rumford. Mr. Davy, by rubbing together two pieces of ice, converted them into water. Now water, *ex hypotheſi*, contains more caloric than the ice from which it was formed; and, on the ſame hypotheſis, the abſolute quantity of caloric in ice is diminiſhed by friction and liquefaction; which is abſurd.

Count Rumford alſo aſcertained that the ſpecific heat of iron was not diminiſhed when converted by a borer into turnings, and conſequently when it had been the ſource of much temperature. In explanation of theſe facts we may be allowed to aſſume the communication of caloric from ſurrounding bodies, till this communication has been demonſtrated to be impoſſible. But even were the impoſſibility eſtabliſhed, it would yet remain to be proved, that the evolved caloric does not proceed from an internal ſource; and this can only be done by an accurate compariſon of the quantity of caloric in bodies before and after friction. Now, in inſtituting this compariſon, it is implied that we poſſeſs means of determining the abſolute quantity of caloric in bodies, and that we can compare quantities of caloric with as much certainty as we can obtain from an appreciation by weight or by meaſure. Such perfection, however, does not, I apprehend, belong to the preſent ſtate of our knowledge reſpecting heat; for I have always been diſtruſtful of that part of the doctrine which aſſigns the ratio of heat latent in bodies. The grounds of this diſtruſt I ſhall ſtate pretty fully; for, if it can be proved that we have no accurate conceptions of quantity, as appertaining to heat, all arguments againſt its materiality, derived from ſuppoſed determinations of its quantity, muſt be inconcluſive.

The only clear conceptions which the mind has of quantity, are derived either from a compariſon of the magnitude or of the gravity of bodies. In the inſtance of caloric, both theſe modes of menſuration fail us. We cannot eſtimate the bulk of a ſubſtance which eludes our graſp and our viſion; nor have we yet ſucceeded in comparing its gravity with that of the groſſer kinds of matter, which it ſurpaſſes in tenuity beyond all compariſon. Our notions of the quantity of caloric are derived, not from ſuch ſimple judgments, but from complicated proceſſes of reaſoning, in the ſteps of which, errors fatal to the whole may perhaps ſometimes appear.

Whatever be the nature of caloric, whether it be a body *ſui generis*, or a quality of other bodies, its effects are peculiar and appropriate; and, like all other effects, bear a proportion to the energy of their cauſe. Expanſion, for example, it is proved by experiment, keeps pace with the actual increments of heat; and on this principle is founded the thermometer, the great agent in the acquirement of all our ideas reſpecting heat, both abſolute and relative. The competency of this inſtrument, however, to afford information of the quantity of caloric, is limited by the following circumſtances:

1ſt, The mercury of the thermometer indicates only the quantity of heat which it has itſelf acquired, and by no means that contained in ſurrounding bodies. 2dly, The ſcale of expanſion is wholly arbitrary, commencing far from the abſolute privation of heat, and falling far ſhort of its maximum. 3dly, The caloric latent in bodies, or chemically combined with them, has no effect on the thermometer. 4thly, The experiments of Dr. Crawford, though ſufficient to ſhow that the expanſion of the mercury of the thermometer bears a ratio to the actual increments of heat in any temperature between the boiling and freezing points of water, by no means prove that this proportion holds univerſally.

Equal weights of heterogeneous bodies, it is preſumed, contain unequal quantities of caloric; and the ratio of theſe quantities is approximated in the following manner:

Equal weights of the ſame body, at different temperatures, give, on admixture, the arithmetical mean; but equal weights of different bodies, at different temperatures, afford a temperature which varies conſiderably from the mean. Thus a pound of water at 100°, and a pound at 200°, give the temperature of 150°; but a pound of water at 200°, and a pound of mercury at 100°, afford, not the mean, but a temperature conſiderably higher. Hence it follows, that a pound of mercury has not the power of fixing and retaining ſo much caloric as a pound of water; and the fixation of more heat by the water than by the mercury, is aſcribed to the ſuperior energy of a power inherent in both, and termed capacity for caloric.

From an extenſive ſeries of experiments Dr. Crawford infers, that the capacities of bodies are permanent ſo long as they retain their form. Thus, the capacity of water has to that of mercury the ratio of 28 to 1, at any temperature between 32° and 212°. The difference of capacities of bodies, it is inferred, therefore, would continue the ſame down to the abſolute privation of temperature. Imagine, then, two bodies at this point of privation: they may ſtill contain unequal quantities of combined caloric; for, when chemically combined, caloric does not produce temperature. On Dr. Crawford's hypotheſis theſe comparative quantities of combined caloric in the two bodies may be learned by obſerving the ratio of temperature produced by the addition to each of ſimilar quantities of heat. This ſuppoſition, however, is manifeſtly gratuitous; and the contrary might be maintained with equal or greater probability; for it may be ſuppoſed that at this aſſumed negation of temperature one body renders latent more caloric than another, becauſe it actually contains leſs, as certain dry ſalts attract more water from the atmoſphere than others containing much water of cryſtallization. The commonly employed mode of aſcertaining the ſpecific caloric of bodies is founded, therefore, on an aſſumption which is deficient in the character of a datum, and which itſelf requires proof.

If theſe objections be valid, they will apply alſo to ſhow the fallacy of the theorem for finding the abſolute zero of bodies. By this term ſome philoſophers appear to underſtand the point of abſolute privation of caloric, both free and combined. I apprehend, however, that in ſtrict propriety it can only be uſed to ſignify the negation of *uncombined* caloric, or, as Dr. Crawford expreſſes himſelf, the point of abſolute cold. As applied, however, to water, it is evident that the whole quantity of heat is underſtood. In aſcertaining the zero, ſay theſe calculators, the capacity of ice to that of water is as 9 to 10. It is plain, therefore, that when water freezes it muſt give out 1-10th of its whole heat; and this tenth part is found to anſwer to 146° of Fahr. Conſequently its whole heat is ten times 146, or 1460; and hence the natural zero is 1460 — 32, or 1428°. Now of this eſtimate it is a datum, that the capacities of ice and water have preciſely the above ratio. But if the general formula for aſcertaining the ſpecific caloric of bodies be founded on erroneous principles, it cannot ſerve as the ground-work of any ſolid concluſions.

The materiality of caloric may, I apprehend, be maintained, without admitting that we have made any ſteps towards determining its quantity in bodies; and the arguments of Count Rumford and Mr. Davy are not demonſtrative, becauſe they aſſume that this part of the doctrine of caloric cannot be relinquiſhed without abandoning it *in toto.* I may be permitted, therefore, to ſtate my reaſons for believing caloric to be matter; which would have been unneceſſary had the contrary been proved with all the force of mathematical demonſtration.

Avoiding all metaphyſical reaſoning on the nature of matter, and aſſuming the generally received definition as ſufficiently characterizing it, I ſhall examine how far this general character of matter applies to the individual—caloric. Caloric occupies ſpace, or is extended, becauſe it enlarges the dimenſions of other bodies; and, for the ſame reaſon, it is impenetrable, ſince if it could exiſt at the ſame time in the ſame place with other bodies, their volume would never be enlarged by the addition of heat. Of form or figure, as only a mode of extenſion, it is unneceſſary to prove that caloric is poſſeſſed; and, indeed, there is perhaps only one general quality of matter that will not be allowed it, viz. attraction. That caloric is influenced by the attraction of gravitation, or by coheſive attraction, has never yet been proved: yet the various experiments of Buffon, Whitehurſt, Fordyce, Pictet, &c. cannot be alleged as proofs that it is actually devoid of this property; ſince they only decide, that the ſmall quantities which can be artificially collected, are not to be ſet in the balance againſt the groſſer kinds of matter. One kind of attraction, *that* which has lately been termed chemical affinity, may, I think, after a full ſurvey of phænomena, be fairly predicated of caloric; and if its poſſeſſion of this quality be rendered probable, we ſhall thence derive a powerful argument in favour of its materiality.

That chemical affinity has a conſiderable ſhare in producing the phænomena of heat, appears probable from the following conſiderations:

1. All the characters diſtinguiſhing caloric when ſeparate, ceaſe to be apparent when it has contributed to a change of form in other bodies; and the properties of the ſubſtances ſo changed are alſo materially altered. Now this is the only unequivocal mark of chemical union that we can apply in any inſtance; and chemical union implies the exiſtence and efficiency of chemical affinity.

2. The relation of caloric to different ſubſtances appears to obſerve that peculiar law, which, in other inſtances, is termed elective affinity. If a compound of two or more principles, a metallic oxide for inſtance, be expoſed in a high temperature, the caloric forms a permanent union with the one, but not with the other. In certain inſtances, caloric is evolved, when two ſubſtances, attracting each other more powerfully than they attract caloric, produce, on admixture, an elevation of temperature. In other inſtances, caloric is abſorbed when it is attracted by the new compound, more ſtrongly than by the ſeparate components. Such facts warrant the deduction, that caloric is ſubject to the laws of chemical affinity. But the preciſe order of its affinities remains to be decided by future experiments.

3. Caloric ſeems alſo, on ſome occaſions, to bear a part in the operation of double elective affinities. In this way it produces decompoſitions, which, by ſingle affinity, it is incapable of effecting. Thus a moſt intenſe fire does not expel entirely the carbonic acid from alkalies: but, when the affinity of an acid for an alkali concurs with that of carbonic acid for caloric, a decompoſition enſues. Again: water may be ſubmitted to the higheſt temperature without imparting a gaſeous form to the hydrogen which it contains; but the conſpiring affinity of a metal for oxygen occaſions the production of hydrogenous gas. On this principle many

chemical facts are reſolved into the law of *double affinity*, which are at preſent explained by that of ſingle elective attraction.

4. Caloric acts ſometimes as an intermedium in combining bodies which, without its aid, are not ſuſceptible of combination. Thus carbon and oxygen do not evince any tendency to combination at the ordinary temperature of the atmoſphere; but caloric brings them into union, and conſtitutes itſelf part of the reſulting compound. This, and a variety of other inſtances, have a ſtriking reſemblance to what is called *intermediate affinity*.

In the theory of Dr. Crawford, no influence is allowed to chemical affinity over the phænomena of heat; and, indeed, *that* philoſopher expreſſes a decided opinion that elementary heat is not capable of uniting chemically with bodies. Hence it appears, that the difference between the terms *affinity* and *capacity* is not merely a verbal one, but that they are actually expreſſive of different powers or cauſes; and the queſtion, therefore, which of theſe terms ſhall be adopted in the deſcription of facts, is one involving the determination of cauſes.

The term capacity for heat is employed, by Dr. Crawford and others, to denote, in the abſtract, that power by which different kinds of matter acquire different quantities of caloric. But in the various applications that are made of this theory, a more preciſe meaning is often affixed to it; and the term is applied in much the ſame ſenſe which it has in common language. When thus underſtood, a difference of capacity neceſſarily implies a difference in the extent of the ſpaces between the minute particles of bodies; and that theſe differences occaſion the varieties obſerved in the acquirement of heat by different bodies. On this theory there is no active principle or power inherent in bodies, and more active in ſome than in others,—no tendency in the matter of heat to attach itſelf, in preference, to any one ſubſtance. The aſſigned cauſe of the phænomena of heat is not, I apprehend, adequate to produce the effects aſcribed to it.

On the theory of capacities, a change of form is, in certain inſtances, antecedent to the abſorption of caloric. Thus, when ether is converted into gas, on removing the preſſure of the atmoſphere, according to this hypotheſis, the capacity of the ether is increaſed by its volatilization; and the change of form is prior to, and the cauſe of, the abſorption of caloric. The order of events, then, in the volatilization of ether is firſt an alteration of form; next, a change of capacity; and, laſtly, an abſorption of caloric. On this hypotheſis, ether may exiſt in the ſtate of gas without containing a greater abſolute quantity of caloric than in a liquid form. But ſuch an interpretation of phænomena is directly contradictory to an eſtabliſhed principle, admitted even by thoſe who prefer the doctrine of capacities, viz. that all bodies, during their converſion from a fluid to a vaporous ſtate, abſorb caloric. It is at variance alſo with obſerved facts: for, if a thermometer be immerſed in a portion of ether, confined under the receiver of an air-pump, the temperature of the ether will be found to ſink gradually during the exhauſtion of the air; and the evaporation becomes proportionally ſlower, till, at laſt, it is ſcarcely perceptible. We may therefore infer, that, at a certain point of diminiſhed temperature, the volatilization of ether would entirely ceaſe, if the ſupply of caloric from ſurrounding bodies could be completely intercepted. But, on the theory of capacities, the evaporation ſhould proceed as rapidly at the cloſe as at the commencement of the proceſs; or, in other words, evaporation ſhould be wholly independent of temperature, which every one knows is contrary to fact.

It may be conſidered, therefore, as extremely probable, that the tendency of ether to aſſume a gaſeous form depends on its chemical affinity for caloric. But, it may be aſked, how is this affinity counteracted by an increaſed preſſure, and augmented by a diminiſhed one?

A circumſtance abſolutely eſſential to the formation of gaſes is, that free ſpace ſhall be allowed for their expanſion.. Mechanical preſſure acts as a counteracting force to this expanſion, and either prevents it completely or partially, according to the degree of its application. But from this fact no argument can be drawn againſt the exiſtence of chemical affinity as an attribute of caloric. Two oppoſite forces in phyſics may be ſo balanced, that neither ſhall produce its appropriate effect. Thus, a body impelled in contrary directions may remain at reſt; yet the operation of the oppoſing forces, in this caſe, cannot be denied. Even in chemiſtry, we have unequivocal examples in which the action of the affinities is ſuppreſſed by more powerful cauſes. Thus, bodies that have a ſtrong chemical affinity are kept perfectly diſtinct even when placed in contact by the affinity of aggregation. The only inference, then, that can fairly be deduced from the effects of preſſure in preventing the formation of gaſes, is, that it is a power ſometimes ſuperior in energy to that of chemical affinity.

Since, therefore, caloric is characterized by all the properties, except gravity, that enter into the definition of matter, we may venture to conſider it as a diſtinct and peculiar body.

Nor is its deficiency of gravity ſufficient to exclude it from the claſs of material ſubſtances. Such nicety of arrangement might, with equal propriety, lead us to deny the materiality of light, the gravity of which has never yet been proved; for, beſides the experiments of Mr. Michell, which failed in aſcertaining this property of light, we have ſeveral chemical facts tending to the ſame concluſion. Thus Mr. Cavendiſh, after firing a mixture of hydrogenous and oxygenous gaſes in a cloſe veſſel, a proceſs during which much light is always emitted, found not the ſmalleſt diminution of weight.

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* The argument at p. 49, which is the baſis of my objections to the commonly employed mode of aſcertaining ſpecific caloric, I fear, is not ſo fully and clearly ſtated as the abſtruſe nature of the ſubject requires.

Aſſuming two bodies, A and B, to be at the point of privation of temperature, or to poſſeſs no free caloric whatſoever, the quantity of combined caloric in each, according to Dr. Crawford's theory, is *directly* proportional to the quantities of heat neceſſary to produce equal elevations of temperature in the two bodies. Thus, if to attain a given temperature, A require caloric as 20, and B only as 10, the combined caloric of A, before this addition, is inferred to have borne to that of B the ratio of 2 to 1. But it might, with equal or perhaps greater probability, have been aſſumed that the combined caloric of A and B is *inverſely* proportional to the quantities of heat required to produce a given temperature; that A, for example, to attain a certain temperature, has abſorbed more caloric than B, becauſe in A leſs caloric exiſted previouſly in a ſtate of chemical union.

IX. *Report made to the Philoſophical and Mathematical Claſs of the French National Inſtitute in the Sitting of Auguſt* 18, *entitled A Tour to Upper Egypt above the Cataracts of Sienne, with Obſervations on the different Kinds of Senna uſed in Commerce. By* C. DESSESSARTZ *and* VENTENAT.

C. NECTOUX, one of the ſcientific men whom the government had made choice of to form part of the commiſſion of the ſciences and arts which accompanied general Bonaparte to Egypt, being appointed to obſerve the agricultural ſyſtem of the country, by examining its plants as well as other objects of natural hiſtory, embraced that opportunity of acquiring ſome more certain information in regard to the ſenna which Alexandria ſupplies to all Europe, and particularly to France.

The magazines of Alexandria and Cairo were the firſt ſources which he minutely inſpected. All the bales of ſenna were opened to him, and he found not only the two kinds already known, but alſo a third plant added to them, the leaves of which have a great reſemblance to thoſe of the real ſenna. But fearing, and with reaſon, that the information he ſhould thus obtain would be as imperfect and incomplete as that before publiſhed, he reſolved to examine the different ſpecies in the places where they are cultivated, and in the different ſtates of growth, that the deſcription he intended to give might be as full and accurate as poſſible.

With this view he viſited and examined, with great care, the environs of Alexandria, Roſetta, Damietta, and Cairo; but without ſucceſs: he did not find a ſingle plant of ſenna in the whole ſpace incloſed in the Delta. The ſenna, therefore, is called the ſenna of Alexandria merely becauſe that city is the general entrepôt from which it is tranſported to Europe; and ſenna *de la palthe*, becauſe ſuch entrepôts are called *palthe*, which ſignifies a farm: the managers are called *palthiers*.

By the information obtained from the different palthiers, among whom he mentions, with gratitude, C. Roſetty, and from the inhabitants of the country, he learned that ſenna plants were to be found in the valleys of Sienne. He therefore proceeded thither, and had the ſatisfaction of meeting with ſome, and of collecting ſeveral loaded with flowers and fruit. Encouraged by this ſucceſs, nothing was able to reſtrain the ardour of his curioſity; neither the violence of the heat, nor the difficulty of a long journey through parched

Count Rumford alſo aſcertained that the ſpecific heat of iron was not diminiſhed when converted by a borer into turnings, and conſequently when it had been the ſource of much temperature. In explanation of theſe facts we may be allowed to aſſume the communication of caloric from ſurrounding bodies, till this communication has been demonſtrated to be impoſſible. But even were the impoſſibility eſtabliſhed, it would yet remain to be proved, that the evolved caloric does not proceed from an internal ſource; and this can only be done by an accurate compariſon of the quantity of caloric in bodies before and after friction. Now, in inſtituting this compariſon, it is implied that we poſſeſs means of determining the abſolute quantity of caloric in bodies, and that we can compare quantities of caloric with as much certainty as we can obtain from an appreciation by weight or by meaſure. Such perfection, however, does not, I apprehend, belong to the preſent ſtate of our knowledge reſpecting heat; for I have always been diſtruſtful of that part of the doctrine which aſſigns the ratio of heat latent in bodies. The grounds of this diſtruſt I ſhall ſtate pretty fully; for, if it can be proved that we have no accurate conceptions of quantity, as appertaining to heat, all arguments againſt its materiality, derived from ſuppoſed determinations of its quantity, muſt be inconcluſive.

The only clear conceptions which the mind has of quantity, are derived either from a compariſon of the magnitude or of the gravity of bodies. In the inſtance of caloric, both theſe modes of menſuration fail us. We cannot eſtimate the bulk of a ſubſtance which eludes our graſp and our viſion; nor have we yet ſucceeded in comparing its gravity with that of the groſſer kinds of matter, which it ſurpaſſes in tenuity beyond all compariſon. Our notions of the quantity of caloric are derived, not from ſuch ſimple judgments, but from complicated proceſſes of reaſoning, in the ſteps of which, errors fatal to the whole may perhaps ſometimes appear.

Whatever be the nature of caloric, whether it be a body *ſui generis*, or a quality of other bodies, its effects are peculiar and appropriate; and, like all other effects, bear a proportion to the energy of their cauſe. Expanſion, for example, it is proved by experiment, keeps pace with the actual increments of heat; and on this principle is founded the thermometer, the great agent in the acquirement of all our ideas reſpecting heat, both abſolute and relative. The competency of this inſtrument, however, to afford information of the quantity of caloric, is limited by the following circumſtances:

1ſt, The mercury of the thermometer indicates only the quantity of heat which it has itſelf acquired, and by no means that contained in ſurrounding bodies. 2dly, The ſcale of expanſion is wholly arbitrary, commencing far from the abſolute privation of heat, and falling far ſhort of its maximum. 3dly, The caloric latent in bodies, or chemically combined with them, has no effect on the thermometer. 4thly, The experiments of Dr. Crawford, though ſufficient to ſhow that the expanſion of the mercury of the thermometer bears a ratio to the actual increments of heat in any temperature between the boiling and freezing points of water, by no means prove that this proportion holds univerſally.

Equal weights of heterogeneous bodies, it is preſumed, contain unequal quantities of caloric; and the ratio of theſe quantities is approximated in the following manner:

Equal weights of the ſame body, at different temperatures, give, on admixture, the arithmetical mean; but equal weights of different bodies, at different temperatures, afford a temperature which varies conſiderably from the mean. Thus a pound of water at 100°, and a pound at 200°, give the temperature of 150°; but a pound of water at 200°, and a pound of mercury at 100°, afford, not the mean, but a temperature conſiderably higher. Hence it follows, that a pound of mercury has not the power of fixing and retaining ſo much caloric as a pound of water; and the fixation of more heat by the water than by the mercury, is aſcribed to the ſuperior energy of a power inherent in both, and termed capacity for caloric.

From an extenſive ſeries of experiments Dr. Crawford infers, that the capacities of bodies are permanent ſo long as they retain their form. Thus, the capacity of water has to that of mercury the ratio of 28 to 1, at any temperature between 32° and 212°. The difference of capacities of bodies, it is inferred, therefore, would continue the ſame down to the abſolute privation of temperature. Imagine, then, two bodies at this point of privation: they may ſtill contain unequal quantities of combined caloric; for, when chemically combined, caloric does not produce temperature. On Dr. Crawford's hypotheſis theſe comparative quantities of combined caloric in the two bodies may be learned by obſerving the ratio of temperature produced by the addition to each of ſimilar quantities of heat. This ſuppoſition, however, is manifeſtly gratuitous; and the contrary might be maintained with equal or greater probability; for it may be ſuppoſed that at this aſſumed negation of temperature one body renders latent more caloric than another, becauſe it actually contains leſs, as certain dry ſalts attract more water from the atmoſphere than others containing

containing much water of cryſtallization. The commonly employed mode of aſcertaining the ſpecific caloric of bodies is founded, therefore, on an aſſumption which is deficient in the character of a datum, and which itſelf requires proof.

If theſe objections be valid, they will apply alſo to ſhow the fallacy of the theorem for finding the abſolute zero of bodies. By this term ſome philoſophers appear to underſtand the point of abſolute privation of caloric, both free and combined. I apprehend, however, that in ſtrict propriety it can only be uſed to ſignify the negation of *uncombined* caloric, or, as Dr. Crawford expreſſes himſelf, the point of abſolute cold. As applied, however, to water, it is evident that the whole quantity of heat is underſtood. In aſcertaining the zero, ſay theſe calculators, the capacity of ice to that of water is as 9 to 10. It is plain, therefore, that when water freezes it muſt give out 1-10th of its whole heat; and this tenth part is found to anſwer to 146° of Fahr. Conſequently its whole heat is ten times 146, or 1460; and hence the natural zero is 1460 — 32, or 1428°. Now of this eſtimate it is a datum, that the capacities of ice and water have preciſely the above ratio. But if the general formula for aſcertaining the ſpecific caloric of bodies be founded on erroneous principles, it cannot ſerve as the ground-work of any ſolid concluſions.

The materiality of caloric may, I apprehend, be maintained, without admitting that we have made any ſteps towards determining its quantity in bodies; and the arguments of Count Rumford and Mr. Davy are not demonſtrative, becauſe they aſſume that this part of the doctrine of caloric cannot be relinquiſhed without abandoning it *in toto*. I may be permitted, therefore, to ſtate my reaſons for believing caloric to be matter; which would have been unneceſſary had the contrary been proved with all the force of mathematical demonſtration.

Avoiding all metaphyſical reaſoning on the nature of matter, and aſſuming the generally received definition as ſufficiently characterizing it, I ſhall examine how far this general character of matter applies to the individual—caloric. Caloric occupies ſpace, or is extended, becauſe it enlarges the dimenſions of other bodies; and, for the ſame reaſon, it is impenetrable, ſince if it could exiſt at the ſame time in the ſame place with other bodies, their volume would never be enlarged by the addition of heat. Of form or figure, as only a mode of exteuſion, it is unneceſſary to prove that caloric is poſſeſſed; and, indeed, there is perhaps only one general quality of matter that will not be allowed it, viz. attraction. That caloric is influenced by the attraction of gravitation, or by

X. *Biographical Account of* MATTHEW BOULTON, *Eſq.**

IF genius and indefatigable induſtry, directed by the pureſt patriotiſm, have any claim to the notice of our readers, an authentic account of this gentleman cannot but be highly acceptable to them. When we contemplate the enlarged extent of his views, the wide and rapid circulation of his improvements and diſcoveries in the moſt important branches of art, and the numerous and honourable connections which he has formed in every part of the civilized world, we ſhall be obliged to admit that few men poſſeſs greater claims to the attention and gratitude of their country.

Matthew Boulton, ſon of Matthew Boulton, by Chriſtian, daughter of Mr. Peers, of Cheſter, was born at Birmingham the 14th of September 1728. He received the chief part of his education at a private grammar-ſchool kept by the Rev. Mr. Anſted, who officiated at St. John's Chapel, Deritend.

So early, we believe, as the year 1745, Mr. Boulton, having loſt his father, who left him in flouriſhing circumſtances, diſtinguiſhed himſelf by the invention of a new and moſt ingenious method of inlaying ſteel. Buckles, watch-chains, and a great variety of other articles wrought at his manufactory, were exported in large quantities to France, where they were eagerly purchaſed by the Engliſh, who affected to have no taſte for the productions of their own country.

The confinement of a populous town was but ill ſuited to ſuch an eſtabliſhment as ſoon became neceſſary for Mr. Boulton's further experiments. Accordingly, in the year 1762, he purchaſed thoſe extenſive tracts of common, at that time a barren heath, with only a ſmall houſe and mill, on which the Soho manufactory now ſtands. He laid the foundation of his preſent extenſive works at the expenſe of nine thouſand pounds. To this ſpot his liberal patronage ſoon attracted great numbers of ingenious men from all parts; and by their aid he ſo eminently ſucceeded in imitating the or moulu, that the moſt ſplendid apartments in this and in many foreign countries received their ornaments from Soho. Here, too, the works of the greateſt maſters in oil colours were mechanically taken off, with ſuch eaſe and exactneſs that the original could ſcarcely be diſtinguiſhed from the copy. This mode of copying was invented, we believe, by Mr. Eggington,

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* From Public Characters of 1800-1801.

ton, whoſe performances in ſtained glaſs have ſince introduced his name to the public.

The utmoſt power of the water-mill, which Mr. Boulton had hitherto employed, fell infinitely ſhort, even with the aid of horſes, of that immenſe force which was ſoon found neceſſary to the completion of his deſigns. Recourſe was therefore had, about the year 1767, to that *chef-d'œuvre* of human ingenuity, the ſteam engine. In ſpeaking of that wonderful machine, we ſhall adopt the animated language of a late excellent Review:—The ſteam engine, approaching to the nature of a perpetuum mobile, or rather an *animal*, is incapable of laſſitude or ſenſation, produces coals, works metals, moves machines, and is certainly the nobleſt *drudge* that was ever employed by the hand of art. Thus we "put a hook in the noſe of the Leviathan:" thus we "play with him as a child, and take him for a ſervant for ever*;" thus "we ſubdue nature, and derive aid and comfort from the elements of earthquakes†."

The firſt engine that Mr. Boulton conſtructed was on M. Savary's plan, of which the reader will find one of the moſt ſatisfactory accounts in Profeſſor Bradley's "New Improvements of Planting and Gardening‡," &c. But the machine was yet, as it were, in its infancy, and by no means anſwered Mr. Boulton's expectations. In the year 1769 Mr. James Watt, of Glaſgow, obtained a patent for ſuch a prodigious improvement of it, that Mr. Boulton immediately ſought his acquaintance, and induced him to ſettle at Soho. At this place, the facility of its application to a variety of concerns, wherein great force was requiſite, ſoon manifeſted its ſuperior utility and vaſt advantages to the public: Parliament, therefore, in 1775, cheerfully granted a prolongation of Mr. Watt's patent for twenty-five years. A partnerſhip now commenced between Meſſrs. Boulton and Watt; and a manufactory of ſteam-engines, on their improved plan, was eſtabliſhed at Soho, which ſtill ſupplies the chief mines and manufactories throughout the kingdom.

Aided by ſuch talents, and commanding ſuch unlimited mechanical powers, Mr. Boulton's views ſoon expanded, and Soho began to exhibit ſymptoms of the extraordinary advantages it had acquired. The art of coining had long ſtood in need of ſimplification and arrangement; and to this art Mr. Boulton no ſooner turned his attention, than, about the year 1788, he erected a coining-mill on an improved plan, and ſtruck a gold medal of the full weight of a guinea, and of the ſame form as that of his new copper coinage lately put into circulation. The ſuperior advantages of that form are obvious. The impreſſion is far leſs liable to friction; and by means of a ſteel gauge of equal diameter, money coined on that principle may be examined by meaſure as well as by weight, the rim being exactly circular. Moreover, the intrinſic is ſo nearly equal to the current value of every piece, that, without a ſteam-engine and adequate apparatus, every attempt to counterfeit the Soho coinage muſt be made with loſs. The fabrication of baſe money ſeems likely, by theſe means, to be ſpeedily checked, and, it is to be hoped, entirely defeated. The reaſon why Mr. Boulton has not yet been employed by government in the coinage of gold and ſilver, we have not been able to learn.

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* Job, xli. 2—4.

† Analytical Review, Feb. 1792, p. 220.

‡ Seventh edit. p. 315.

The mill at Soho works eight machines, *each of which receives, ſtamps, and delivers out, by the aid of only a little boy, from ſeventy to ninety pieces of copper in one minute.* Either of them is ſtopped without the ſmalleſt interruption to the motion of the others. In adjoining apartments all the preparatory proceſſes are carried on with equal facility and diſpatch; ſuch as rolling the copper into ſheets, dividing them into blanks, and ſhaking them into bags clean and ready for the die. Without any perſonal communication between the different claſſes of workmen, &c. the blanks are conveyed to the room where they are ſhaken, and from thence to the coining-room, in boxes moving with immenſe velocity on an inclined plane, and accompanied by a ticket of their weight.

The Sierra Leone company have employed Mr. Boulton's mint in the coinage of ſilver, and the Eaſt India company in that of copper. Two complete mints have likewiſe been lately ſent to Peterſburgh.

Since the demiſe of the late empreſs Catharine, Mr. Boulton preſented her ſucceſſor, the late emperor Paul I., with ſome of the moſt curious articles of his manufactory, and in return received a polite letter of thanks and approbation, together with a ſplendid collection of medals, minerals from Siberia, and ſpecimens of all the modern money of Ruſſia. Among the medals, which for elegance of deſign and beauty of execution have never yet been equalled in this or any other country, is a maſſy one of gold, impreſſed with a ſtriking likeneſs, it is ſaid, of that monarch. Our readers will be ſurpriſed, when they are told that this unrivalled piece was ſtruck from a die engraved by the preſent empreſs dowager,

ager, who has from her youth taken great delight in the art of engraving on ſteel.

With the view of ſtill further improving and facilitating the manufactory of ſteam-engines, Meſſrs. Boulton and Watt have lately, in conjunction with their ſons, eſtabliſhed a foundery at Smethwick, a ſhort diſtance fromSoho. Here that powerful agent is employed, as it were, to multiply itſelf, and its various parts are fabricated and adapted together with the ſame regularity, neatneſs, and expedition, which diſtinguiſh all the operations of their manufactory. Thoſe engines are afterwards diſtributed to all parts of the kingdom by the Birmingham canal, which communicates with a wet dock belonging to the foundery.

To ſuch amazing perfection has the ſteam-engine at length been brought, that the conſumption of one buſhel of Newcaſtle coals will raiſe nearly ſix thouſand hogſheads of water ten feet high, and will do the work of ten horſes for one hour. This remarkable abridgement of human labour, and proportionate diminution of expenſe, are, in a great meaſure, the reſult of trials made under the auſpices of Mr. Boulton. But for a more complete account of theſe machines, their power, &c. we muſt refer the reader to Dr. Darwin's Botanic Garden*.

It could ſcarcely be expected that envy would view with indifference ſuch ſingular merit, and ſuch unexampled ſucceſs. The inventions and improvements of Meſſrs. Boulton and Watt were firſt imitated, and then either decried or diſputed. Reaſon laboured in vain to ſilence the clamours of injuſtice, and to defeat the ſtratagems of fraud. At length, in the year 1792, a ſolemn deciſion of parliament, and, about the ſame time, the concurrent opinion of the court of king's bench, forbad any further encroachment.

Whoever contemplates the merit and utility of a long life devoted to ſuch valuable purſuits, as we have here briefly and very imperfectly deſcribed, and recollects without emotion, that the ſpot whereon ſo much has been done, and is ſtill doing; where hundreds of women and children eaſily earn a comfortable ſubſiſtence †; where population is rapidly increaſing, and the means of national proſperity increaſing in proportion, was lately a bleak, ſwampy, and ſterile waſte, muſt want underſtanding to comprehend, or ſympathy to appreciate, the happineſs of his fellow-creatures.

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* Fourth edit. note xi. page 287.

† We have been unable to aſcertain the number of hands employed by Mr. Boulton at this time, which muſt frequently vary according to the changes that neceſſarily take place in the demand for different articles; but we know, that when Mr. Boulton junior came of age, in 1791, ſeven hundred workmen ſat down to an entertainment given by his father.

Mr. Boulton is now in his ſeventy-third year, and he appears to poſſeſs the hilarity of youth. Extraordinary exertions, often both of body and mind, ſeem not to have impaired a conſtitution which muſt have been naturally robuſt. He is fond of muſic, and takes great delight in the company of young people. One ſon, a young man of conſiderable accompliſhment and great promiſe in his father's line, and one daughter, both of them unmarried, have ſurvived their mother. Mr. Boulton is fellow of the Royal Societies of London and Edinburgh, and of the Free Economical Society of Peterſburgh, as well as of many other foreign inſtitutions.

XI. *On the Effects of Elder in preſerving Plants from Inſects and Flies.* *By* M. CHRISTOPHE GULLET [*].

COMMON elder has appeared to me uſeful, 1ſt, for preventing cabbage plants from being devoured or damaged by caterpillars: 2d, to prevent blights, and their effects on fruit- and other trees: 3d, to preſerve corn from yellow flies and other inſects: 4th, to ſecure turnips from the ravage of flies, &c.

1ſt, The ſtrong and fetid odour of a bunch of elder leaves induced me to think that different kinds of butterflies might be incommoded by it in proportion to their delicacy. I therefore took ſome young twigs of elder, at the period when butterflies began to appear, and whipped well with them ſome cabbage plants, but in ſuch a manner as not to damage them. Since that time, during two ſummers, though the butterflies hovered round the plants, I never ſaw one of them ſettle on them; and I do not think that a ſingle butterfly was hatched on the cabbages treated in this manner, though a neighbouring board was dirtied by them in the uſual manner.

2d, After a ſhort reflection on the effects here mentioned, and on blights, which, in my opinion, are chiefly occaſioned by ſmall flies and ſmall inſects whoſe organs are ſtill more delicate than thoſe of the former, I was induced to whip in the ſame manner with elder twigs, as high as I could reach, the branches of a plum-tree which grew in an eſpalier. The whipped leaves remained green and in a good condition, while from at leaſt ſix inches above to the top of the tree the reſt of the leaves were blighted, wrinkled, and full of worms. It is here to be obſerved that the tree was in full flower when I whipped it, therefore much too late for this operation, which ought to have been performed once or twice before flowering. But I am of opinion, that if trees were beſprinkled with a ſtrong infuſion of elder every eight or fifteen days, the ſucceſs would be certain, and that there would be no danger of injuring either the flowers or the fruit.

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[*] From the *Bibliothèque Phyſico-Economique*, Ann. 6.

3d, What the farmers call the yellows in corn, and which they conſider as a kind of blight, is the effect, as every one knows, of a ſmall yellow fly with blue wings nearly of the ſize of a gnat. It lays its eggs in the ear of wheat, and produces a worm almoſt inviſible to the naked eye, but which, when ſeen by a magnifying glaſs, is a large yellow larva, having the ſhining colour of amber. This fly is ſo productive, that I have counted upwards of forty worms in the chaff of one ear of wheat, which was a number ſufficient to deſtroy it entirely. I therefore propoſed to make my experiment as ſoon as poſſible; but the heat and drought of the ſeaſon having advanced the wheat more than uſual, it was in flower before I could attempt it. Next morning, however, at break of day, two ſervants having drawn bundles of elder over the ears of wheat on each ſide of the furrow, backwards and forwards, in places where the wheat was not ſo far advanced, I hoped that the fetid effluvia of the elder would prevent the flies from remaining on the ears that were covered with them: and, indeed, I was not entirely diſappointed; for, on examining my wheat ſome time after, I found that the part which had been beaten with elder was much leſs damaged than that which had not been treated in the ſame manner. I have no doubt, that, had I employed this precaution ſooner, the corn would have been completely preſerved. Should this be the caſe, the proceſs is ſimple; and I flatter myſelf that fine crops of corn may be ſaved by theſe means from this ſmall inſect, which is ſo deſtructive to them. One of theſe yellow flies laid on my thumb at leaſt eight or ten eggs, of an oblong form, in the ſmall interval of time which I employed in walking over two or three furrows, holding it by the wings, and which I could not obſerve without the aſſiſtance of a magnifying glaſs.

4th, It often happens that whole crops of turnips are deſtroyed while young, in conſequence of being pricked by certain inſects. I have great reaſon to think that this evil may be prevented in an effectual manner, by cauſing a perſon to draw a bunch of elder, ſufficiently large to cover about the breadth of a foot, over the young turnips, going backwards and forwards. What confirms me in this idea is, that, having drawn a bunch of elder over a bed of young cauliflowers which had begun to be pricked, they afterwards remained untouched by theſe inſects.

Another fact which tends to ſupport this idea is, that when my neighbourhood, about eight or nine years ago, was ſo infeſted with caterpillars that they devoured all the vegetables, leaving ſcarcely a green leaf untouched, they ſpared the elder-trees amidſt this general devaſtation, and never moleſted them. In reflecting on theſe circumſtances, I am of opinion that the elder might be introduced with advantage into our gardens, as the means of preſerving fruit-trees and various plants from the rapacity of inſects.

The dwarf elder appears to me to exhale a much more fetid ſmell than the common elder, and therefore ought to be preferred in making experiments on this ſubject.

XII. *Reply to a Charge of Plagiariſm, brought by Mr.* HENRY CLARKE *againſt* W. DICKSON, *LL. D. Communicated in a Letter from Dr.* DICKSON *to Mr.* TILLOCH.

I HAVE partly drawn up, and hoped before this to have laid before the Public, a Defenſe of Sir Iſaac Newton, and ſome other diſtinguiſhed Britiſh mathematicians, againſt the miſrepreſentations of Mr. Henry Clarke, and of the writer of the mathematical articles of the Monthly Review. But a diſtreſſing complaint, which is alleviated but not yet removed, has, as you know, long rendered me almoſt, and at times altogether, incapable of the neceſſary application to more ſerious concerns, which will demand my whole attention for ſome time to come.

I certainly owe the gentlemen ſome return for their Animadverſions * on one of my notes on C. Carnot's excellent piece on the Infiniteſimal Calculus; and it will be eaſy to repay them in their own coin, and with intereſt. In that inſtance, it is true, and perhaps in others, I unfortunately

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* See Animadverſions on Dickſon's Tranſlation of C. Carnot's Reflections on the Theory of the Infiniteſimal Calculus, by Henry Clarke, late *Prælect. Coll. Mancun.* &c. ſold by T. Hurſt, No. 32, Paternoſter Row.—Alſo the Monthly Review Enlarged, vol. xxxviii. p. 440, 441. N. Series.

miſtook

miſtook the ſenſe of my very acute and ingenious author; for which I am ready to make *him* and my readers any reaſonable apology*. But it is alſo true, as Mr. Clarke himſelf confeſſes, that I "have endeavoured to do juſtice to my author," and, "on the whole, have properly appreciated his merit." Yet it is no leſs apparent, that both Mr. Clarke and the Monthly Critic, not contented, as the Critical Reviewer has been, with correcting my error, have, *without the ſmalleſt provocation*, treated me with the greateſt harſhneſs and contempt. Their motives will appear hereafter, from the internal evidence of their own writings, compared with my notes on C. Carnot; and from ſtrong collateral circumſtances. Among other things, it will appear, that Mr. Clarke, as well as the Monthly Cenſor, is in ſome degree, a critic by trade; that both are in the habit of emitting their deciſions, with an air of deſpotic authority, which ill becomes men whoſe abilities, conſiderable as they are, cannot hide their infirmities, far leſs juſtify their inſolence; and that, in many inſtances, they ought rather to implore pardon, than to provoke recrimination. Mr. Clarke has already been taxed with "*nonſenſe*" in correcting my error†; and the "injurious aſſertions" of the Monthly Critic have lately obliged the juſtly celebrated Dr. Hutton, of Woolwich, to drag the "concealed tyrant" out of his hole, and to expoſe him publicly *by name*.

I ſhall be juſtified by the evidence before me, to conſider my cenſors (eſpecially the Monthly one) in certain *other* lights than thoſe of ſelf-conceited and unjuſt critics, and illogical, new-fangled, mathematicians.—But my chief object at preſent is, to repel a charge of plagiariſm, brought againſt me by Mr. Clarke, in theſe words:

"P. S. The Tranſlator informs the reader, in his note, p. 47, that the general method there ſhown of finding the fluxions of quantities, is "*in ſome meaſure new.*" But if the reader will look into *Clarke's Rationale of Circulating Numbers, with uſeful Remarks on various Parts of the Mathematics*, publiſhed about twenty years ago, he will find the very ſame proceſs adopted, and the application thereof copiouſly exemplified!"

Such is the charge which Mr. Clarke, no doubt to make it conſpicuous, has placed in a Poſtſcript after "the end" of his Animadverſions on my Tranſlation of Carnot; and has finiſhed it with a (!) by way of a ſting to its tail.

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* My error cannot embarraſs the reader. He has only to omit my note, p. 26.

† In the Critical Review, vol. xxxiv. New Arr. p. 359.

In the firſt place, I ſolemnly declare that this charge is falſe and groundleſs in every poſſible reſpect.

Secondly, I deny that I have there "ſhown a general method of finding the fluxions of quantities." I have only inſerted in a *note*, "*a ſpecimen*"—"*a ſlight and imperfect general outline*"—*of a very eaſy, and in ſome meaſure new*, manner of treating the fundamental proceſſes of fluxions, which I long ago mentioned to yourſelf; and which, with ſome thoughts on Prime and Ultimate Ratios, I intended to offer you for publication," &c.[*] The ſubſtance of the paper here alluded to, which you may recollect my mentioning, I ſubmitted to a certain Gentleman whoſe education does honour to the Univerſity of Cambridge. But the paper itſelf, owing to my indiſpoſition and avocations, is not yet finiſhed; and, owing to the treatment I have received, is likely never to be printed, any more than ſome analogous pieces of more conſequence, which I intended for the preſs. This, Mr. Clarke will ſay, perhaps very truly, is no loſs to the public. But unleſs he can divine the contents of a piece, which has no public exiſtence, his charge of plagiariſm muſt become impotent, or can only affect it's fabricator.

Thirdly, Whether even the "ſpecimen" of the method which I *promiſed*, be, or be not, "the very ſame" with Mr. Clarke's, is *a matter of fact*, of which thoſe perſons who compare them can judge; and ſuch I leave to determine, Whether they know any book from which that "ſpecimen" differs ſo widely as it does from that of Mr. Clarke.

I pretended not to a method entirely, or even in a *great* meaſure, new. My principal object was *facility*, which is but too much neglected by great mathematicians. I thought, however, that my firſt way of finding the fluxion of a fraction was my own, as I ſtill think the ſecond is. Certainly, I never received either of them from any man, or from any book. The firſt is as follows:

"The fluxion of a fraction may be found by conſidering it as the product of the numerator and denominator, giving the latter a negative index. Thus $\frac{x}{y}$ is equivalent to $y^{-1} \times x$; and, as the fluxion of xy is $y\dot{x} + x\dot{y}$, ſo the fluxion of $\frac{x}{y}$, or of its equivalent $y^{-1} \times x$, muſt be

$$\left(y^{-1} \times \dot{x}\right) + \left(x \times -y^{-2}\dot{y}\right) = \frac{\dot{x}}{y} - \frac{x\dot{y}}{y^2} = \frac{y^2\dot{x} - yx\dot{y}}{y^3} = \frac{y\dot{x} - x\dot{y}}{y^2}.$$

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[*] See my Tranſlation of Carnot, notes p. 48 and 49.

This proceſs, I ſay, I took to be my own. But ſince Mr. Clarke has rudely forced me to look into his obſcure book, and a great number of better ones which I never before conſulted, I find it in the *table*, in which, as he ſays, he has "copiouſly exemplified" his method; whether intelligibly or not, to beginners, whom he profeſſes to inſtruct, it is not my preſent buſineſs to inquire.

In Mr. Clarke's firſt column, marked "Variable Quantities," ſtands "$\frac{x}{y} = xy^{-1}$", and in his ſecond column marked "Fluxions, $x \times -y^{-2}\dot{y} + y^{-1}\dot{x} = \frac{y\dot{x} - x\dot{y}}{y^2}$."

Mr. Jones, I find, ſtates the ſame proceſs, thus,

"F, $\frac{x}{y}\left(x \times \frac{1}{y}\right)$ is $\left(\dot{x} \times \frac{1}{y} - \frac{\dot{y}}{y^2} \times x\right) \frac{\dot{x}y - x\dot{y}}{yy}$."*

And Mr. Holliday thus: "What is the fluxion of $\frac{x}{y}$, or $xy-1$?—1ſt, $\dot{x} \times y = y\dot{x}$; and 2d, $\dot{y} \times x = x\dot{y}$: then $\frac{y\dot{x} - x\dot{y}}{yy}$ the fluxion. For $\frac{\dot{x}}{y}$ or $\dot{x} \times \frac{1}{y}$ is $= \dot{x} \times \frac{1}{y} - \frac{\dot{y}}{yy} \times x = \frac{y\dot{x} - x\dot{y}}{yy}$."†

Now, have not I as good a right to ſay (what however, I am very far from ſaying) that Mr. Clarke copied from Jones or Holliday, as he has to affirm, that I copied from him? And, is it not a preſumption in my favour, that I have been more explicit than any of them; although I only offer a "ſpecimen" in a *note*, and they profeſs, as Mr. Clarke expreſſes it, to render the principles of Fluxions "clear to the loweſt capacity ‡?"

Be this as it may, in the ſame note, I mention a ſecond, and ſtill eaſier, way of doing the ſame thing, in theſe words, "The fluxion of a fraction, $\frac{x}{y}$, may alſo be found, but not ſo elegantly, by actually dividing $x + \dot{x}$ by $y + \dot{y}$." Of this, which indeed has little to recommend it but it's obvious facility, not being able to find a trace in any author, I may fairly claim it as my own; and, if ſo, may now affirm (what I have not before affirmed) that even my "ſlight and imperfect ſpecimen," and, *a fortiori*, the *promiſed* method itſelf, is "in ſome meaſure new"—the point to be proved.

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* Synopſis Palmariorum Matheſeos, printed in 1706, p. 229.

† Introduction to Fluxions, printed in 1777, p. 96.

‡ See Mr. Clarke's Preface, p. xii. Jones and Holliday make the ſame profeſſion in their title-pages and prefaces; and the latter appears, in general, to have fulfilled it.

I remain, Dear Sir,
Truly and reſpectfully yours,
WILLIAM DICKSON.

P. S. My language may be thought ſomewhat ſevere. But thoſe who read the critiques of which I complain, muſt acknowledge, that it is more than juſtified by the provocation. To ſuch perſonal provocation, I have hitherto been an *utter ſtranger*; though, you know, that for ſix years, I laboured to promote, by all the fair, honeſt and legal means in my power, the Abolition of the nefarious African Slave-trade. I do not even except the unprovoked piece of invective which firſt contributed to "drag me into that controverſy [*]"—one of the moſt angry, and voluminous, which ever agitated this nation.——In what I have farther to write, I ſhall endeavour to preſerve better humour, and to return "dry rubs," rather than dry blows,

"To words far bitterer than wormwood,
"Which would, in Job or Grizzel, ſtir mood."

The ends of ſelf-defence (not to mention here a certain more important end) will be abundantly anſwered by a defence of the illuſtrious dead, whoſe aſhes, as is known to you and others, I have long wiſhed to ſee protected by ſome abler hand, from the profanation of the Monthly Reviewer, in the licentious exerciſe of his *fonction*.——What I mean to publiſh, ſhall be *concluſive* on my part. Controverſy, though I ſuffered no diſcredit in the important one juſt mentioned, has no charms for me; nor ſhall I contend, farther than is abſolutely neceſſary, with men who, for aught that appears, watch for opportunities, and will not be nice about the means, of exalting themſelves, at the expenſe of others. W. D.

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[*] See my Letters on Slavery, p. 4.

XIII. *A general View of the Coal Mines worked in France, of their different Products, and the Means of circulating them. By* C. Lefebvre, *Member of the Council of Mines, of the Philomatic Society, &c. &c.**

I HAD propoſed for ſeveral years to exhibit a view of the reſources of France in regard to combuſtible foſſils. I was deſirous of making known, at the ſame time, the principal places of conſumption, the grand metallurgic eſtabliſhments, where theſe ſubſtances are, or might be, employed; and to add to this labour an indication of the localities proper for the creation of new manufactories, according to the union preſented by nature of mineral ſubſtances, capable of being worked, and of abundance of fuel.

It appeared to me alſo, that, to fill up this ſketch, it would be neceſſary to include pretty certain data, in regard to the nature and quantity of the reſources which the foreſts and woods might furniſh for conſumption in different parts of the French territory, and particularly in regard to thoſe moſt intereſting to be conſidered under the view of large manufactories †.

The information collected by the council of mines for ſeven or eight years, furniſhed a variety of details and intereſting facts in regard to the mineral ſubſtances known and dug up in our territory. The notes reſpecting coal mines in particular were the moſt numerous, becauſe the adminiſtration of mines, feeling the neceſſity of ſupplying the diminution of the products of our foreſts, and the importance of enſuring conſtant activity to our manufactories, by means of coals, had paid the moſt ſerious attention to this object.

A great many indications, however, required further confirmation, and whole diſtricts of France, which were ſuppoſed to be highly intereſting, were ſtill imperfectly known in regard to mineralogy. The council had not been able for ſeveral years to ſend out many engineers to explore the country; many of the departments earneſtly requeſted that reſearches might be made in this manner; and the diſtribution of theſe functionaries in the places where their preſence was ſo neceſſary, could not fail to be one of the firſt meaſures of a government whoſe attention was ſo zealouſly occupied with the proſperity of the ſtate *.

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* From *Journal des Mines*, No. 71.

† A good map of the communications, both by land and by water, pointing out the foreſts, coal mines, the turf moſſes, and the large manufactories, would be of great value to commerce. It would certainly contribute to give more activity to internal commerce. A map of this kind is wanted, and the conſtruction of it appears to be an object worthy the attention of the preſent government.

The council of mines ſketched out, in the year 4, a part of this operation on the navigation chart by Dupain-Triel. It contained marks which pointed out the poſition of the coal mines, turf moſſes, forges, great founderies, ſalt works, and ſome other large manufactories, reſpecting which accurate information had been obtained; but this labour is now very imperfect, becauſe the new departments are not included in this chart, which, on account of the new information obtained ſince that period, ought to be added.

As I had the ſtrongeſt reaſon to hope that the reſults of the reſearches and obſervations of the engineers of mines would furniſh the moſt valuable materials for the propoſed view of our mineral reſources, I reſolved to wait until their reſidence in the different departments ſhould have enriched the adminiſtration of mines with new information proper for rendering more perfect my propoſed labour.

I was beſides perſuaded that, during that period, the ſolicitude and enlightened alacrity of the magiſtrates who compoſe the general adminiſtration of foreſts, would give more extent and correctneſs to our knowledge reſpecting the conſiſtence and products of our woods; that, perhaps, we ſhould even have a map of the foreſts; and that the work which I projected might ſtill be completed in this very important department.

I confined myſelf, therefore, to the collecting of notes; to verifying as far as poſſible, and to extending the firſt data in the poſſeſſion of the adminiſtration of mines, and to making reſearches in regard to the exiſting means of circulation, and thoſe which it would be proper to create, in order to take as much advantage as poſſible of the materials which were the object of my labour.

But at the moment when peace, reſtored to Europe, inſpired a deſire of renewing our commercial intercourſe with the neighbouring nations, we endeavoured to examine what really were our wants, and what advantages would ariſe to us from the reciprocal exchanges which might take place.

Our attention was directed in particular to mineral ſubſtances: we knew that before the war they had been a conſiderable object of importation; coals in particular had been

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* In conſequence of meaſures taken this year (an. 10) by Chaptal, the miniſter of the interior, ſixty departments are to be viſited and carefully examined by engineers, who will concur with the præfects in the improvements to be made in thoſe parts, and who will give an account of them to government. This is all that could be done with the preſent number of engineers who can be ſpared for that purpoſe.

brought in great abundance into our territory, though the uſe of that kind of fuel was leſs frequent at that period than at preſent, and much leſs, no doubt, than it will become in conſequence of the deterioration of the wood. It was, therefore, of more importance than ever to appreciate properly our new ſituation in this reſpect, after our increaſe of territory, the fruit of the conqueſts of our armies. It was neceſſary to determine whether we ought to admit among us the products of foreign coal mines; conſidering on one hand the quantity of money exported,, and the decreaſe or even ſtagnation of induſtry which might enſue in our national mines; and on the other, whatever was proper for the ſecurity and improvement of our manufactures.

All the data proper for the ſolution of this queſtion were ſubjected to government. The different opinions pro and contra were defended with that warmth which is always excited by objects of great national importance, among men ſtrongly attached to their country, and deeply affected by every thing which ought to have an influence on their deſtiny.

Whatever may be the determination of government, we muſt rely with confidence on the wiſdom of its views, and be aſſured, that it will not decide without being fully informed on the ſubject.

In publiſhing at preſent this view of the reſources preſented by our coal mines, I have no intention of entering into a diſcuſſion of the queſtion already mentioned: but ſeveral perſons having urged me to make known, as far as poſſible, our riches in this department, I thought that by complying with their requeſt I ſhould render my labour important in ſeveral points of view; firſt, as ſupplying information uſeful to ſtateſmen, who might be deſirous of details on this ſubject, and to the different conſumers and to the workers of mines.

I have followed the alphabetical order of the departments; each of them has been reviewed; and from the information obtained I have enumerated all the known mines now worked, thoſe ſuſceptible of being worked, and thoſe indications not yet verified which ſeem to deſerve an attentive examination.

I have exhibited as far as poſſible the maſs of the productions of the coal mines in each department where they are worked, and the mean price of the coals, both at the mines and in the principal places of conſumption.

I muſt however obſerve, that there are ſome departments where the mines are worked in ſo irregular a manner, that it was impoſſible for me to obtain data ſufficiently ſatisfactory, either in regard to the quantity of coals extracted, or to their value. In that caſe I have only made known the diſtricts where the coals are found.

To the means of circulation which exiſt, or thoſe which ought to be eſtabliſhed, I have paid particular attention. I have indicated theſe means as far as poſſible; and under this point of view in particular I hope my labour may be uſeful, becauſe, by making known the means of circulation, the public attention may be directed to the tranſportation and uſe of our combuſtible foſſils, in places where it has hitherto been ſuppoſed that they could not be employed with advantage. With this view I have added to this work a chart, where the means of circulation may be traced out, by correſponding numbers placed in the margin, and by lines drawn or dotted with red, which mark the exiſting modes of circulation, and thoſe projected, or which it may appear of advantage to eſtabliſh.

The departments in which no foſſil combuſtibles are dug up have nevertheleſs been preſented in their order; and I have announced whence theſe ſubſtances may be extracted, and the means by which they may be tranſported.

I have exhibited alſo a general view of the various conſiderations in regard to public advantage and economy which reſult from the preſent ſtate, as far as it is known, of our reſources of mineral fuel.

Department of Ain.

This department exhibits no ſtrata of coals worked at preſent. At Surjoux, in the canton of Seyſſel, on the banks of the Rhine, aſphaltes or mineral bitumen is found. It is extracted from beds of coarſe granitic freeſtone, analogous to that which covers certain ſtrata of coal, or which is found in alternate ſtrata with it. Several banks of this bituminous freeſtone are found all round the commune of Surjoux. They have very little conſiſtence, on account of the abundance of liquid bituminous matter with which they are mixed.

C. Suretan, an inhabitant of Seyſſel, who has a grant of a part of thoſe diſtricts, extracts and prepares this mineral bitumen. The products extracted amount annually to about ten thouſand myriagrammes. This ſubſtance may be employed for daubing over cordage and wood, to defend them from water and from the attack of worms. It is uſeful alſo for greaſing the axle-trees of carriages, and different parts of machines, to facilitate their rolling.

The means of circulation of theſe productions are the

Rhone deſcending towards Lyons, the South of France, and particularly the ſea-ports in that quarter, where the uſe of this bitumen may be attended with economical advantage.

Department of Aiſne.

This country contains no coal mines (1) * worked at preſent: a ſubſtance known under the name of *coal earth* is commonly found in it, forming beds of greater or leſs thickneſs, and generally placed at no great depth beneath the cultivated lands. It is a ſort of exceedingly pyritous turf. It burns badly, and cannot be employed for the ſame purpoſes as real coal: it would be very bad for heating apartments, or for working iron. A great deal of this ſubſtance, however, is dug up; it is employed by the farmers, who ſpread it out, either in its natural ſtate, or after it has been left to waſte in the open air, on the fields, which it renders fitter for vegetation.

The abundance of pyrites diſperſed throughout the ſtrata of turf render them ſuſceptible, perhaps, of yielding, by proper treatment, ſulphate of iron (green copperas). The coal pits of the northern departments will eaſily furniſh coal for the conſumption of that of Aiſne, when the junction canal between the Scheldt and the river Oiſe ſhall be terminated.

Department of Allier.

There are coal mines worked at Noyant, (2) in the commune of the ſame name, ſituated ſix leagues ſouth-weſt of Moulins, on the edge of the road from that town to Montluçon. At the diſtance of three leagues further, proceeding towards the latter place, there is another coal mine now worked; namely, that of Fins, in the commune of Chatillon; and a league and a half further, but on the other ſide of the road, a third, that of des Gabliers, in the commune of Tronget.

The coal hitherto extracted at Noyant is that proper for uſing under boilers, and for other purpoſes of a ſimilar kind. That furniſhed by the mines of Fins and Gabliers is for the moſt part of an excellent quality, and proper for forges. The annual product of theſe mines is about a million of myriagrammes; but they are far from being carried to that degree of activity of which they ſeem to be ſuſceptible. It is announced that meaſures are about to be adopted in regard to the mine of Fins, calculated to obtain from it all the advantages which the depth of the ſtrata and the valuable quality of the coals ſeem to promiſe. The product of theſe mines is conſumed in the neighbouring glaſs-houſes, and the coals are tranſported by water down the Allier, on the Loire, the canal of Briare, and the Seine.

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* Theſe numbers refer to the map.

A plan has been long in agitation to employ the ſmall river of Quenne to facilitate the tranſportation of theſe coals as far as the Allier. If this project be ſuſceptible of execution, there can be no doubt that this mode of communication with the river Allier will be of great importance; for the expenſe of land-carriage as far as Moulins already doubles the price of the coal. It varies in theſe mines from ſix to ten per cent. the myriagramme: it has already riſen at Moulins from twelve to twenty per cent.; and when delivered at Paris from twenty-four to forty per cent.

This department exhibits alſo, in the commune of Commentry, conſiderable ſtrata of coal of excellent quality, and might be worked with advantage, if a conſumption be created for them: this at preſent is abſolutely wanting, and for this reaſon theſe mines are very little worked. The engineers of mines who have viſited this country have pointed out ſeveral new ſtrata not before obſerved.

The mines of Plaveret and de Bouijc furniſhed in the year 3, a hundred thouſand myriagrammes, which is far below the quantity that might eaſily be extracted. The price at the mine is about four cents per myriagramme.

If the Cher were rendered navigable from Montluçon to Vierzon, where it is ſo at preſent, the mines in the neighbourhood of Commentry would ſoon give riſe to enterpriſes which would enliven that country. It is probable that they would have a very happy influence on the activity of the numerous forges ſituated on the borders of the Cher, and at a ſmall diſtance from it, by the application of a fuel ſo ſtrong as coal to a part of the operations of theſe forges.

There are indications of coal in ſeveral parts of this country which deſerve to be examined, if there were eaſier means of diſpoſing of them, or if conſumption could be found for them on the ſpot. Such are the indications found by C. Rambourg, near the foreſt of Tronçais, in the canton of Meaulne; and thoſe in the commune of Vallon, in the ſame canton, diſcovered by Thiebault of Allier.

Department of the Lower Alps.

Some coal mines (3) are worked in the neighbourhood of Manoſque and Forcalquier. They are worked very irregularly. The products are not well known. The quality of the coals is very indifferent. They are ſold for about 20 cents per myriagramme, delivered at the mine. There are very few means of diſpoſing of them.

Department of the Upper Alps.

Coal mines are found (4) in the commune of Saint Martin de Querieres, and in the neighbourhood: the ſame obſervations may be applied to theſe coal mines as have already been made in regard to thoſe of the Lower Alps. They have however a more certain and an eaſier ſale, in conſequence of the neighbourhood of the town of Briançon, where the conſumption is very conſiderable, as wood is exceedingly ſcarce in that canton.

Department of the Maritime Alps.

A grant was paſſed in the year 9, of the coal mine (5) in the neighbourhod of Roquebrune. The grantee has lately announced that the firſt attempts were not ſucceſsful. It, however, appears that ſome of this fuel is dug up in ſeveral places in the neighbourhood of Monaco. Theſe mines might acquire a certain degree of importance, if the coals were of a good quality, and if they were exported through the port of Monaco, or if manufactories of iron articles were eſtabliſhed in that department, or in the environs of the mines.

If the iron ore of the mines of Elba was treated in this department, or in that of Var, the iron obtained might give riſe to manufactories of this kind; but theſe conſiderations require further information, which will no doubt excite the attention of government.

Department of Ardeche.

Coals are found (6) in ſeveral cantons of this department, and particularly in the environs of Jaujac, Privas, Aubenas, Vallon, and Saint Marcel d'Ardeche. Theſe mines, in general, are badly worked. They are intereſting, however, in different points of view; firſt, on account of the conſumption occaſioned by the numerous manufactories of the country; and in the ſecond place of the facility of carriage down the Rhone.

No data ſufficiently exact have yet been obtained in regard to the products of theſe mines: they are certainly conſiderable, and would become much more productive and profitable, if the works were better conducted. The common price of coals in that country is about 8 cents per myriagramme.

An engineer of mines, if ſettled in this department, might introduce improvements in the working of theſe coal mines, which are exceedingly deſirable. He could employ himſelf alſo on other objects uſeful to the induſtry of the inhabitants.

[To be continued.]

XIV. On

XIV. *On Canals.* *By* THOMAS TELFORD, *Eſq.**

INLAND navigation has now been ſo long and extenſively practiſed in Great Britain, that the benefits ariſing from it are generally felt and acknowledged; and it is difficult to ſay, whether the improvements in agriculture, manufactures, or commerce, are moſt indebted to the numerous canals which now interſect the country in all directions, or to other cauſes.

It is not my intention at preſent to trace the riſe and progreſs of this great national improvement, nor to enter deeply into the general principles of it: what I chiefly aim at in this ſlight ſketch is, to draw the attention of the people who are concerned in laying out new canals, to ſome circumſtances which appear to me to be very material to the intereſts of canal companies and to the country at large.

In conſidering the ſubject, I ſhall beg leave to ſtate, that canals are chiefly for the purpoſe of,

1ſt, Carrying fuel and raw materials to ſome manufacturing towns or diſtricts, and for exporting manufactured goods.

2dly, Carrying fuel for domeſtic purpoſes; manure for the purpoſes of agriculture; tranſporting the produce of the diſtricts through which the canal paſſes to the different markets; and promoting agricultural purpoſes in general.

3dly, Carrying the produce of mines to the ſea-ſhores.

Theſe appear to be the leading points by which our views ought to be guided in planning new canals. The ſupplying the diſtrict through which the canal paſſes with groceries and merchant goods is a very ſecondary conſideration; experience has taught this leſſon to the perſons who are concerned in the canals already made; and it is proper this ſource of diſappointment ſhould be made generally known, in order to prevent future miſapplication of talents and capital.

In planning a new canal, it ſhould be carefully conſidered under which of the foregoing heads the uſes of the canal ought to be claſſed. Caſes will ſometimes, no doubt, occur, where the ſeveral purpoſes above mentioned will in ſome meaſure be combined; but they will rarely be ſo equally balanced, as to create a difficulty in diſcovering the predominant features. The general purpoſes of the canal being judiciouſly determined, all the following ſteps ſhould be taken with a view to promote the principal end.

If the canal is for the purpoſe of carrying fuel and raw materials to ſome manufacturing town or diſtrict, and exporting the manufactured goods from thence, or for carrying the produce of mines to the ſea-ſhores,—the line of canal ſhould be made in as ſhort a direction as the nature of the country will admit of, even if this line ſhould be atttended with ſome extraordinary expenſe; and that mode of conveyance ſhould be adopted which is leaſt liable to be interrupted, becauſe in thoſe caſes much depends upon a conſtant and regular ſupply.

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* Communicated by the Author.

If the canal is chiefly for the purpoſes of agriculture, thoſe lines are to be ſought for which will accommodate the country moſt perfectly at the leaſt poſſible expenſe. This will not, in general, be effected by ſhort and direct, but, on the contrary, by circuitous and level lines, ſuch as will viſit many parts of the country, carry fuel and manure into every quarter, and take off the produce of the land for back carriage. Merchandize, although a very inferior conſideration, will, by the ſame means, be extenſively and equally diſtributed. When the carriage of thoſe heavy but neceſſary articles is rendered cheap, and the ſupply convenient, the conſumption will increaſe. The very uſe of lime as a manure is in general ſufficient to create a conſiderable revenue upon a canal, when the coal and limeſtone can be carried along the canal at a moderate expenſe. As improvements in agriculture muſt increaſe the wealth of the inhabitants, more articles are required to ſupply their multiplied wants; the poſſeſſion of capital prompts them to enter upon new employments, and the demands of all create an addition of buſineſs upon the canal.

Many other advantages attend circuitous and level canals. If there are upland countries above the level of the canal, ſome cheap and convenient ſituations may be ſelected where the waſte water of the uplands can be collected into reſervoirs to be formed for this purpoſe. From thoſe reſervoirs the water may be taken into the canals. At various points of the canal, where there are falls immediately on the lower ſide, mills may be erected for grinding corn, or for the purpoſes of manufactures: in many parts, two or three overſhot wheels, of ſufficient diameter, may be placed below each other: when the water has performed thoſe uſeful offices, it will fall into the brooks, and prove a plentiful and regular ſupply to other works in the courſe of each ſtream.

Improvements in agriculture may alſo be greatly promoted by uſing a part of the waſte water for the purpoſe of irrigation. For a canal carried over a country upon a high level would prove an extenſive top drain. Not only the land adjoining the banks of the canal, but all which lies below the level, may enjoy the uſe of the water for irrigation; and the brooks and rivers will be the tail drains of the country.

Collecting the waſte water in winter, and in heavy rains, will prove advantageous in every reſpect: it will be the means of preventing high and rapid floods; it will be converting to uſeful purpoſes that which flows off to idle waſte, and, not unfrequently, to poſitive miſchief.

The uſeful purpoſes to which brooks and rivers are now applied, will not be injured, but improved; as there will be a more plentiful ſupply in ſummer, and a better regulated one during the winter months.

Parliament would not heſitate to grant canal companies the power of making reſervoirs, and receiving a reaſonable recompenſe for the water ſupplied from the canal for the purpoſes of agriculture and manufactures, becauſe it would injure no perſon, and accommodate all: it will be furniſhing additional powers to Britiſh induſtry, and creating permanent wealth to the nation.

I underſtand that this mode of managing water has long been practiſed in Italy, eſpecially in the Milaneſe, where a conſiderable revenue is derived by the waſte water of canals; and the regulations reſpecting it have long engaged the attention of the legiſlature of that country.

If I am right in the foregoing ſtatements, it will be eaſy to apply them to different diſtricts of country, and, by a careful and impartial judgment, to decide upon the character of each canal.

It is probably not ſufficiently known what quantity of rain water falls in the courſe of a year in various parts of the kingdom; it therefore may not be improper to add the following ſtatement, which will be ſome guide with regard to this ſubject; and if along with this we take the accounts of perſons accuſtomed to make obſervations, a tolerably correct eſtimate may be formed of the average quantity of water which can be collected in each diſtrict: it will, in general, be found to exceed the expectations of perſons who have not paid attention to this important ſubject.

Rain falls—At London, being the average of the following Years: 1774, 5, 6, 7, 8, 9, 1780; 1789, 90, 91, 92	Inch. 21¼
Upminſter, in Eſſex, average of 1700, 1, 2, 3, 4, 5	19⅛
Lincolnſhire, in medium ſeaſon - - - -	18
Ditto extreme wet - - - - -	24
Liverpool - - - - - -	34½
Townley, in Lancaſhire - - -	42½
Kendal, in Weſtmoreland - - -	61¼
Dumfries, in Scotland - -	36¼
Glaſgow, ditto - - - - -	31

XV. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

IN the meeting of Jan. 27th was read a paper by Charles Greville, eſq. giving an account of three ſpecimens, in different muſeums in France, of ſtones which have fallen to the earth from the clouds, all ſimilar in their characteriſtics to thoſe deſcribed by H. Howard, eſq. and alſo of a maſs of native iron which had fallen in Perſia in the year of the Hegira 1030, according to the annals of the empire written by the emperor, and of which he is ſtated to have made ſome ſabres and daggers: but until other iron was mixed with that of the maſs deſcribed the iron was not malleable.

February 5. A paper by Everard Home, eſq. on the nature of the tongue, was read. His experiments on different diſeaſed tongues prove that that member is endowed with leſs irritability than any other organ of the body; and that a diſeaſed part may be removed with great ſafety by means of ligatures.

On the 10th a letter from profeſſor Bygge of Copenhagen was read, giving an account of the tranſit of Mercury in November laſt.

And on the ſame night was begun a paper by Dr. Herſchel on the ſame ſubject, and alſo on the viſible effects of ſudden changes of temperature on the interior of teleſcopes; giving ſome obſervations on the proper ſituations to be choſen for obſervatories.

On the 17th the reading of this paper was concluded.

ANTIQUARIAN SOCIETY.

It appears by a memoir preſented to the Antiquarian Society on the 3d of February by Dr. Raine, that the inſcription on the pillar near Alexandria, ſo long conſidered illegible, has been decyphered. The diſcovery was made by captain Leake of the artillery, and captain Squire of the engineers. The impreſſion was taken in ſulphur; and it ſeems that it was only when the ſun ſhone on that ſide of the pillar where the inſcription is, that any traces of it could be diſtinguiſhed. By the inſcription it appears that the pillar was erected in honour of the emperor Diocletian.

VACCINE INSTITUTION.

Intelligence has been received at the Vaccine Inſtitution through one of its members, that cow-pock inoculation has been introduced at Bombay, where it is in great eſtimation.

At the Vaccine Inſtitution dinner on Monday Feb. 7th, many viſitors voluntarily entered their names with liberal ſubſcriptions. After dinner among other toaſts were given, The promulgator of the vaccine inoculation, Dr. Jenner; Dr. Pearſon, with due acknowledgments of his ſervices in eſtabliſhing the new inoculation by the evidence of practice.

Dr. Pearſon then took the opportunity, after thanking the company for their honourable teſtimony, to inform them of many particulars concerning the conduct and objects of the inſtitution, which could not be noticed in the report, and gave a moſt pleaſing view of the advantages which individuals had already derived from the cow-pock: but he anticipated the momentous conſequences of univerſal inoculation throughout the united kingdom in two reſpects.

1ſt, In preſerving from 30,000 to 46,000 lives annually; and, 2dly, In a few years *exterminating the ſmall-pox.* To accompliſh theſe views, he contended that laws for the inoculation of every ſubject within a certain period after birth were eſſentially neceſſary, as well as immediate prohibition of the inoculation of the ſmall-pox; that the latter meaſure alone would be quite inadequate; and he maintained that it was not more an infringement on the liberty of the ſubject, to render the cow-pock inoculation univerſal, than to prohibit, as already propoſed, the ſmall-pox inoculation. He obſerved that the number loſt by inoculation for the ſmall-pox was not an object of national importance, as it amounted to no more than ſix to nine per thouſand; but when it was conſidered, that one could hardly ſay there was more danger from the inoculation of the cow-pock than from a puncture with a clean lancet, and that the ſymptoms were uſually very much ſlighter than in the ſmall-pox inoculation, and that by univerſal cow-pock inoculation, the infection of the ſmall-pox muſt be extinguiſhed, which end was obvious by even the univerſal inoculation of the ſmall-pox; it remained only to inſtitute the cow-pock inoculation as propoſed, univerſally.

Dr. Pearſon combated the objections againſt increaſing the population, by ſhowing that the reſources of agriculture and animal food were not underſtood. That the augmentation of the population in France required, as a meaſure of ſafety, attention to the means of increaſing this kind of national wealth;—that the mere waſte in feeding of animals, and in preparing food for man, if taken care of, would be ſufficient to ſupport a conſiderably greater than the preſent population.

Dr. Pearſon illuſtrated his argument in favour of vaccine inoculation by obſerving, that going through the ſmall-pox in the caſual way, and by inoculation of it, and by exciting the cow-pock in place of the ſmall-pox, was like croſſing a river in three different ways.

1ſt, Swimming or wading over; in which 97 out of a thouſand periſhed.

2d, Going over in boats; by which mode 6 to 9 per thouſand were loſt, and many others were injured in their health.

3d, By means of a bridge; during which paſſage no one periſhes (except from thoſe caſualties which might happen in other ſituations), and by which generally the health was not affected, and when it was ſo it was as frequently amended as made worſe.

ROYAL JENNERIAN SOCIETY.

It gives us pleaſure to announce, that a new Vaccine Inſtitution under the above title has juſt been eſtabliſhed, which we have reaſon to hope, from the extenſive patronage it has received, will eventually extirpate the ſmall-pox, in paving the way to which ſo much had already been done by the Vaccine Inſtitution.

His Majeſty is Patron.
Her Majeſty, Patroneſs.

The Vice-Patrons are,

His Royal Highneſs the Prince of Wales.
His Royal Highneſs the Duke of York.
His Royal Highneſs the Duke of Clarence.
His Royal Highneſs the Duke of Cumberland.

Vice-Patroneſſes.

Her Royal Highneſs the Princeſs of Wales.
Her Royal Highneſs the Ducheſs of York.
Her Royal Highneſs Princeſs Auguſta Sophia.
Her Royal Highneſs Princeſs Elizabeth.
Her Royal Highneſs Princeſs Mary.
Her Royal Highneſs Princeſs Sophia.
Her Royal Highneſs Princeſs Amelia.

ROYAL SOCIETY OF GOTTINGEN.

The Triple Inſcription.

In the ſitting of September 4th 1802, profeſſor Heyne read a memoir on the Egyptian monument with three inſcriptions, of which the following is the ſubſtance:

The inſcription contains a decree of the aſſembly of the prieſts of Memphis, by which they confer on king Ptolemy Epiphanes new divine honours, out of gratitude for the ſervices rendered by him to religion, to its miniſters, to the people, and to the ſtate in general. To determine the time and occaſion of this decree, is the firſt point in queſtion, and therefore is the firſt object treated of in the memoir. Ptolemy Philopator left at his death (Olymp. 144. 1. 2. 6 years before the birth of Chriſt) a ſon about five years of age. The moſt contemptible perſons, who, under the preceding government, had miniſtered to the pleaſures of Philopator, Agathocles, Agathoclea, and her mother Œnanthe, ſeized on the government and the guardianſhip of the young prince: they were followed by Tlepolemus, and afterwards by Ariſtomenes. In the year 200 before Chriſt the Egyptians intruſted to the Romans the tutelage of the young king, to protect the empire from the projects of king Philip, and of Antiochus of Syria. This meaſure produced internal troubles and revolts. When Thoas and the Ætolians had been cruſhed, it was thought moſt proper for the public repoſe to declare the prince, who was then fourteen years of age, major, and to ſuffer him to govern his affairs himſelf. The ceremony of the coronation, which is known under the name of *Anacleteria*, Olymp. 145. 4. 197 years before the birth of Jeſus Chriſt, and nine years after the death of Philopator, was celebrated at Memphis. The ninth year is expreſsly named in the decree. All the Egyptian prieſts were invited to this ceremony. It appears that during the laſt years of the king's minority attempts had been made to gain over the people and the prieſts. Government had granted to the prieſts in particular a great number of privileges and immunities, and had alſo been at great expenſe for their worſhip and temples. Out of gratitude, the aſſembly of prieſts decreed, that in crowning the king (the day even is indicated in the text of the decree, the 4th of the Macedonian month Xantichus, the 18th of the Egyptian month Mechir, in the firſt half of our March, in the ſecond half of the Olympic year) they ſhould offer their homages to the king, according to the manner uſual at that time, by giving him titles, and paying him honours, borrowed from the divine worſhip. The decree contains an account of all the ſervices which the king had rendered to the empire and the Egyptian prieſts, and then the new titles of honour decreed to him.

The titles found in the decree are: the lord of kings, the glorious, the pious towards the gods, the conqueror of his enemies, the repairer of the pleaſures of life, the lord of the cycles of thirty years (Κυριος των τριακοντα ετηριων). He is ſtyled alſo the deſcendant of the gods, (εκγονος Θεων Φιλοπατορων) a title by which is underſtood his father Ptolemy Philopator, and perhaps his mother. The titles properly ſo called, which are repeated ſeveral times with the name, are: the enjoyer of long life, αιωνοβιος, the favourite of Phtha, the viſible god, επιφανης, (and not illuſtrious, as is uſually tranſlated) the beneficent, ευχαριϛος, according to the meaning given to this word at that period.

The titles and names of the prieſts, which are at the head of the decree of the aſſembly, are: εφ' ἱερεως Αετοῦ, τȣ̃ Αετοῦ, Αλεξανδρου και Θεων Σωτηρων, και Θεων Αδελφων, και Θεων Ευεργετων, και Θεων Φιλοπατορων, και Θεου Επιφανοῦς ευχαριϛου. It is evident that theſe were prieſts of Alexander, of Ptolemy Soter, of Philadelphus Euergetes, of Philopator, of Epiphanes, and therefore of the royal family raiſed to the rank of deities. The name Αετος, eagle, gives reaſon to believe, that the caſe was the ſame here as with other kinds of prieſthood mentioned in antiquity, that the family name was entirely abandoned, and could no longer be named; and that the rank was indicated by a general name. Here a ſymbolic name, viz. the eagle, ſeems to have indicated the high prieſt. A prieſteſs of Berenice, no doubt the ſpouſe of Soter, has the ſecond rank; her name of honour is αλλοφορος. The third place, under the title of a *Canephora*, is given to the prieſteſs of Arſinoe, the wife of Philadelphus. The fourth is occupied by the prieſteſs of Arſinoe, the ſiſter and wife of Philopator. Then comes αρχιερεις, προφηται, και ὁι εις το αδυτον εις πορευεμενοι, προς τον στολισμον των Θεων, (hence it appears that the ſtatues were dreſſed or ornamented) και πτεροφοραι και ἱερογραμματεις, και ὁι αλλοι ἱερεις, &c. reſpecting which Jablonſki will give ſome explanations, as well as the learned who make further reſearches in regard to the explanation of the inſcription. In regard to the word πτεροφοραι, one of the members remembered a paſſage in Clemens of Alexandria, where is found ἱερογραμματευς εφων πτερα επι της κεφαλης*. This claſs therefore was diſtinguiſhed by a feather on the head, as a mark of honour, which is found alſo on the Egyptian monuments †.

An account of the benefits conferred by the prince on the prieſts affords ſeveral notices reſpecting the different kinds of taxes; but it would require too much room to detail the whole of them. They conſiſt partly in pecuniary impoſitions, and partly in contributions in kind. We ſhall however mention thoſe benefits which are connected with hiſtory. Among the ſervices rendered by the king, is the ſiege and capture of Lycopolis. The inſurgents of the laſt years had thrown themſelves into this place, and had fortified it; ſo that the ſiege was attended with difficulties, eſpecially in the *eighth* year (of the government). The Nile had increaſed in an extraordinary degree, and the king was obliged to oppoſe dykes to the water, in order that the ſiege might be continued with advantage. Polybius ſpeaks of the ſiege of Lycopolis *; but he places it in the 4th year of the 148th Olympiad, 185 years before the birth of Jeſus Chriſt, and therefore 12 years later. Vaillant alſo places it in this year. The inſcription evidently contradicts this opinion; but we need only examine with attention the words of Polybius, who ſpeaks of the ſiege of Lycopolis as an event that took place ſome years before.

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* See Beckmann's Hiſtory of Inventions, part iv. p. 294.

† See *Admiranda Urbis Romæ*, No. 16. ed. 1693.

The honours decreed to the king conſiſt partly in a confirmation of the antient honourable titles, and partly in the addition of a new one, that of protector (or avenger) of Egypt. Πτολεμαιου τȣ· επαμυνοντας τῇ Αιγυπτῳ, which was to be every where inſerted in the ſacerdotal rite, and then a ſtatue was to be erected in all the temples. The ſtatue was to be placed in ſuch a manner, that the ſtatue of the deity of the temple ſhould preſent to it victorious arms. The prieſts were to adore the ſtatue three times a day, in the ſacred proceſſions, when ſmall golden temples (ναοι) were carried about, with ſmall images of the gods. The image of the king was to be carried about in a ſimilar ſacellum.

Some other circumſtances, which require further explanation, the author has reſerved for another opportunity. Unfortunately the inſcription has ſuffered in the lower part. It concludes with theſe words: (σ) τερεου λιθου τοις τε ἱεροις και εγχωριοις και ελληνικοις γραμμασιν και στησαι εν εκαστωι των τε πρωτων και δευτερων (ἱερων).

On the Perſepolitan Inſcriptions.

In the ſame ſitting a memoir was read of M. George Grotefend, entitled: *Prævia de cuneatis quas vocant inſcriptionibus Perſepolitanis legendis et explicandis relatio*; the contents of which are the more aſtoniſhing, as the author is not an orientaliſt, and employed himſelf accidentally in decyphering this writing, hitherto ſo obſcure. The author, who has long exerciſed himſelf in decyphering, in conſequence of a trifling diſpute with one of his friends, laid a bet that he would decypher one of the Perſepolitan inſcriptions. He ſucceeded beyond his expectation, and in a few weeks was able to explain the greater part of the inſcriptions, and to communicate details reſpecting the manner in which he proceeded, and concerning his reſults.

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* Excerpt. lib. xxiii. 16.

The author firſt ſpeaks of the *cuneiform writing* in general, and lays down the following principles: 1ſt, The wedge-formed characters are really letters. On the monuments of Perſepolis there are three ſorts of them, which, for the moſt part, correſpond with each other, as has been remarked by Niebuhr and Munter, and which may be called the firſt, ſecond, and third kinds of writing. The whole three may be clearly obſerved on the vaſe of Caylus *, provided two ſigns only be corrected from ſimilar inſcriptions found in Niebuhr and Le Bruyn. 2d. The wedge-formed figures are alphabetic letters, and not ſyllables or ſigns. In the firſt ſort of letters the end of a word is indicated by a wedge placed obliquely, and in the ſecond, by a perpendicular wedge. If the groupes of figures were ſyllables, we ought to have here words of ten ſyllables, for very often there are ſo many ſigns between two ſeparations of words. In each of theſe kinds of writing there are diſtinguiſhed about forty figures, a number which would be too ſmall for writing in ſigns. 3d. All the wedge-formed inſcriptions proceed from left to right, in a horizontal direction, and not vertically or in buſtrophedon characters, as may be ſeen by comparing the inſcriptions B, D, C, in Niebuhr. It is thence ſeen that this principle, expreſſed perhaps too generally, can be applied only to the inſcriptions of Perſepolis.

In the ſecond ſection, which relates to inſcriptions of the firſt ſort in particular, the author obſerves, that this writing has need of marks proper for the long and ſhort vowels, as in the antient Perſian writing called the Zendic: hence the quantity of forty letters which Niebuhr has already collected. In a word, all the inſcriptions which the author has been hitherto able to explain, relate to Darius the ſon of Hyſtaſpes, and to Xerxes; a circumſtance to which new reſearches have already given a high degree of probability.

In the third ſection the author deſcribes the manner in which he proceeded, and gives his explanations. The ſuppoſitions already mentioned, as well as the analogy of the inſcription of the Saſſanides, gave reaſon to expect in particular the name of king, and titles particularly for Darius and

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* Rem. d'Antiq. v. pl. 30.

Xerxes. The words on which accents are found ought to be titles. By different combinations the author obtained the words *Kbſcbercbe* and *Darbeuſcb*; by help of the words found, read the other words of the inſcription on the urn of Caylus, and thoſe of B and G of Niebuhr, which he explains according to the Zendic. The dictionaries and grammatical remarks of Anquetil were of great ſervice to him in this labour.

The author has given, as the reſults of his reſearches, an explanation of the inſcriptions on the urn already mentioned, and thoſe of Niebuhr, pl. 24, B, G. He read thus the B of Niebuhr: *Dârbeûſcb Kbſcbebiôb egbre. Kbſcbebiôb Kbſcbêbiobetcbâo. Kbſcbêbiôb. Dabutcbao. Goſcblaſpbæbe, búa akbeotcboſcbocb, áb* ôoo, Môro, *ezutcbuſcb*, that is to ſay, *Darius rex fortis, rex regum, rex dabarum (filius) Hyſtaſpys, ſtirps mundi rectoris, in conſtellatione maſculâ* τοῦ *Moro*, ·του *Ized*. Moro is one of the twenty-eight conſtellations; the author refers the word governor of the world to Giemſhid, from whom the Perſian kings, the Achemenides, derived their origin.

He read thus the inſcription marked G of Niebuhr: *Kbſcbercbe Kbſêbébiôb egbre. Kbſcbebiôb. Kbſcbêbiobetcbâo. Darbeuſcb Kbſcbebiobêbê. bûn. akbeotcboſcbôb*, that is to ſay, *Xerxes rex fortis, rex regum* (filius) *Darii, regis ſtirps omnium rectoris*. It is in this manner that the author explains the inſcription found in Le Bruyn, p. 273, No. 133, on the mantle of the king: he even propoſes corrections. He thinks alſo that he can reſtore and correct the inſcription on the window, (ſee in the ſame place No. 134.) What has been here ſaid may enable the reader to form ſome opinion of his labour.

However unexpected this diſcovery may be, it ſeems to deſerve ſome attention: we have reaſon indeed to be on our guard againſt diſcoveries of this kind, becauſe one may be eaſily deceived by poſſible combinations, and by certain ſuppoſitions which may originate in chance; eſpecially when the language affords no certain means of judging of their correctneſs, and when, on the other hand, the language itſelf muſt in ſome meaſure be diſcovered. As the author has not made known his alphabet, and as he has not mentioned whether it be complete for all the inſcriptions of the firſt ſort of writing, and how far he has made reſearches in regard to the other kinds; no opinion can yet be formed of the certainty and extent of the diſcovery. It appears that the author hitherto has employed himſelf chiefly with the inſcriptions of Perſepolis, and we muſt confine to theſe inſcriptions ſome principles too generally expreſſed; namely, that each ſort of writing exhibits forty ſigns, that all the wedge-formed writing has a horizontal direction, and that they are all alphabetic. There is ſomething alſo to be rectified in the explanation: *rex dabarum*, for example, as a title of Darius, is not very probable: in regard to *rector mundi*, it might be referred perhaps with more reaſon to Ormuzd. But what may give us a prepoſſeſſion in favour of the author's hypotheſis is, that it is founded on what the reſearches and obſervations hitherto made, give as the moſt probable reſult. From theſe indeed we can admit that the buildings of Perſepolis belong to the time of the ſucceſſors of Cyrus. We may even go further, and conclude that the greater part of the monuments were finiſhed under Darius and the following kings; becauſe the ſhort duration of the reign of Cambyſes would not have been ſufficient for the execution of ſuch works.

As the inſcriptions are as old as the monuments, they muſt be referred to the time of theſe princes. In a word, it is ſeen that the wedge-formed writing on thoſe monuments, and in general where it has a horizontal direction, proceeds from left to right. This circumſtance, which was obſerved by Niebuhr, is inconteſtably proved by a ſimilar inſcription on ſtone found in the French national muſeum at Paris, and which C. Millin has given in the firſt number of *Monumens Antiques*, plates 8 and 9. The artiſt in this inſcription has placed inſulated wedges, or groupes, or whole letters, for which there was no room, above the lines, or in the ſecond column, and always to the right, where the end of the line ought conſequently to be. The author's explanation agrees with theſe obſervations. He reads from left to right, and finds in the inſcriptions alluſions to Darius, Xerxes, and certain points reſpecting the worſhip of the magi. The legends which the author has found are conſiſtent and probable, when conſidered hiſtorically. They have alſo the greateſt analogy with the inſcriptions of the Saſſanides, a dynaſty deſcended from the antient Perſian kings, and who endeavoured to re-eſtabliſh the empire and its religion. As theſe princes on antient coins and monuments are ſtyled kings of the kings of Izan, of divine origin, &c. ſimilar titles, but more ſimple, are found in theſe inſcriptions. Time alone can ſhow whether the continuation of the author's reſearches will juſtify theſe explanations.

If the learned ſhould ſucceed in decyphering the wedge-formed writing, their diſcoveries would throw great light on many points of the Aſiatic antiquities, becauſe more monuments of this writing are daily diſcovered. They may be divided into three claſſes. 1ſt, The Babylonian, among which are reckoned the bricks of the walls of antient Babylon,

lon, and perhaps ſome other monuments, if the reſearches of Dr. Lichtenſtein do not aſſign to them another period. This writing has a peculiar character, which may be diſtinguiſhed by the name of the *nail-writing**. 2d, The Perſian: the monuments of Perſepolis, ſome gems and vaſes, the ſtone of which C. Millin ſpeaks, and ſeveral others. The writing of the latter has more reſemblance to the point of a dart. It might be called *dart-writing*. 3d, The Egypto-Perſian: to this kind, in all probability, belong the cylindric amulets of hæmatites with wedge-formed letters and figures; and, in particular, a fragment of a ſtone found near Suez, which exhibits wedge-formed letters; and a Perſian head, having on it a hawk's wing. An engraving of it may be ſeen in Denon, from a drawing by general Dugua. This variety of monuments, which is daily becoming more numerous, proves how widely diffuſed the uſe of ſuch letters was at a certain period.

XVI. *Intelligence and Miſcellaneous Articles.*

ANTIQUITIES.

SOME curious and valuable remains of antiquity were lately diſcovered in Iſtria, Dalmatia, and Albania, and have been ſent to Vienna by M. de Carnea Stephaneo, his imperial majeſty's commiſſary in theſe provinces. The articles found are:

1ſt, A torſo of Parian marble, found among the ruins of Salona, in Dalmatia, a league north-eaſt from Spalatro. It is ſeven feet and a half in height, and repreſents a naked warrior ſeated on the trunk of a tree, on which is placed his war dreſs. This ſtatue is in the moſt ſublime Grecian ſtyle. The expreſſion and beauty of the form ſhow that it is the work of one of the firſt maſters of antiquity. The belt exhibits a peculiarity not generally met with in ſtatues of this kind. It is much to be regretted that it is mutilated nearly in the ſame manner as the torſo of the Vatican at Rome.

2d, A Minerva, of Corinthian braſs, found in Dalmatia, near Xa-Oſtrogs, three leagues and a half from the mouth of the Narenta. It is eight feet in height, and perfectly entire. This Minerva is in the Greek coſtume, with a helmet, an ægis, and a Meduſa's head on the breaſt: ſhe holds a lance in the left hand, which is ſomewhat elevated, and a cup in the right. The hair is arranged in the ſame manner as is ſtill uſual among the Illyrian women in the interior part of Dalmatia, and among thoſe of Kamtſchatka and on the banks of the Anadir.

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* For a ſpecimen of this kind of writing, ſee our xith volume.

3d, A Madona with infant Jeſus aſleep, painted by Aloys Vivarinus, of Murano, on a plaſter ground. This painting, which was finiſhed in 1489, is five feet two inches in height and one foot eleven inches in breadth. It was executed at the time when the art of painting in oil was firſt carried from Flanders to Italy. It proves that Vivarinus, by his delicate and expreſſive pencil, had already carried to a high degree of excellence the art of painting, which was not brought to a ſtate of perfection till the time of Charles V.

4th, A naval battle painted in oil, eleven feet in height and twenty-one in breadth. It is a maſter-piece of Tintoretto. He compoſed it at the requeſt of the ſenate of Venice, who preſented it to the city of Piſano. It repreſents the battle ſaid to have taken place in 1177 between the Germans and the Venetian fleet near Cape de Salvora, in Iſtria, and which was firſt mentioned by Andrew Dandolo, in his Chronicle, two centuries after.

5th, A buſt, painted on marble, of Sanctorius, a celebrated profeſſor of Padua, who died in 1639, and who formed an epoch in the philoſophical hiſtory of medicine.

EARTHQUAKE.

Some obſervations have lately been publiſhed in the German journals in regard to the curious circumſtances which accompanied the earthquake that took place in the province of the Seven Mountains towards the end of October. It was in the low meadows lying towards the north that the phænomena were moſt ſenſible, where deep fiſſures two or three feet in breadth and thirty-five fathoms in length, which divided into ſeveral branches, were formed in a direction from weſt to eaſt. A great deal of very fine ſand of an aſh-gray colour, which emitted a ſulphureous odour, was thrown up from theſe fiſſures. The principal fiſſure, after extending throughout a long ſpace, ſeparated into ſeveral branches, which bent themſelves into ſemicircles towards the weſt, and traverſed a houſe and a ſtable, after piercing the walls at the foundation. The whole country was covered with one or more inches of ſand in the direction of the fiſſures: and perſons who were witneſſes to theſe phænomena aſſert, that at the time of the laſt ſhock the earth opened with a noiſe like that of a muſket ſhot; that a black and ſtrong ſmelling vapour iſſued from the fiſſures, as well as a great deal of water, which ſpouted up with a loud noiſe; and that theſe jets were followed by a great quantity of ſulphureous ſand. The ground alſo ſunk down in a ſenſible manner. The water which iſſued from the fiſſures was exceedingly ſulphureous, cold, and limpid.

Chemiſts obſerved, that a pound of this ſand diluted in eight pints of water produced no effervеſcence, and even gave no precipitate on being mixed with vitriolic acid. The nitrous acid gave the ſame reſult. It was only after its decompoſition, and after gall-nuts were able to produce no change in the colour, that the water became turbid, in ſuch a manner as to ſhow the preſence of the vitriolic acid. By decoction and evaporation ſigns of cryſtallization were obſerved at the ſurface; and, when the evaporation was complete, there remained about eight grains of alkaline matters, two parts of which were Glauber's ſalt, one earthy ſalt, and the other gypſum.

TRANSIT OF MERCURY,

As obſerved at Paris November 9, 1802.

Obſervers.	Interior Contact.	Exterior Contact.
C. Mechain - -	$0^h\ 6^m\ 45{\cdot}4^s$	$0^h\ 8^m\ 30{\cdot}4^s$
Lalande - - - - -	0 6 29·0	0 7 56
His nephew and -	0 6 43·0	0 8 18
Burckhardt -	0 6 49	0 8 19
Meſſier - -	0 6 49·2	

The above reſults are mean time reduced to the national obſervatory.

Mechain ſaw the planet very well terminated without any aureola, though he always followed its progreſs on the ſun with a teleſcope of three object glaſſes 4½ inches in diameter and 7 feet focus, with which he took 45 diſtances. He had obſerved in the ſame manner the tranſits in the years 1782, 1786, 1789, and 1799.

CHEMISTRY.

1. Brugnatelli has lately obſerved, that by treating paper with the nitric acid, a large quantity of ſuberic acid mixed with oxalic acid is obtained. This ſeems to prove that Fourcroy was right in claſſing cork among the immediate principles of the vegetable kingdom.

2. The ſame chemiſt in a letter ſays, that after a particular examination of wheat, he thinks he has found that vegetable gluten is nothing elſe than animal fibrous matter. It differs indeed from animal gluten or glue, by its inſolubility in cold water, and by the contraction it experiences in warm water. When treated with the nitric acid it ſwells, pro-

duces a foamy effervefcence, gives out nitrous gas, and diſſolves. This ſolution is decompoſed by water, like that of animal fibrous matter, and the white precipitate thence reſulting diſſolves in ammonia, aſſuming a dark yellow colour.

3. A diſcovery of two new principles in ſulphureous waters has lately been made by Counſellor Weſtrumb, of Hammeln, who gives the following details:

"For ſix years I have been excluſively employed in the analyſis of ſulphureous waters. I have found an eaſy and entirely new method of determining what they contain of ſulphurated hydrogen and carbonic acid gas; and I have diſcovered a principle hitherto unknown in theſe waters, namely, bitumen in combination with ſulphurated hydrogen gas and hydro-ſulphurated lime. I communicated the detail of this diſcovery to M. Wurzer. Counſellor Von Crell and Profeſſor Schaub of Caſſel ſaw at my houſe theſe new products. Theſe principles are found in the waters of Neuwdorf, Limmen, Rehburg, and Eylſe. In the laſt place, five ſulphureous ſprings, altogether different from each other, a ſpring of carbonic gaſeous water, and one of freſh water, are found in an extent of leſs than a thouſand ſquare feet. Near Buckembourg, the capital of the ſmall country of Leppe, to which Eylſe belongs, are found two other ſulphureous ſprings perfectly ſimilar, one of which we ſounded to the depth of eighty feet without finding its reſervoir. Baths of mud ſimilar to thoſe of St. Amand have been conſtructed at Eylſe: a very conſiderable quantity of hydro-ſulphurated mud, which may furniſh baths for ſeveral centuries, has been found there."

4. A new gas, ſulphurated azotic gas, has been diſcovered in the mineral waters of Aix-la-Chapelle by M. C. Gimbernat, ſub director of the muſeum of natural hiſtory at Madrid, who gives the following account of it:

"I have juſt finiſhed my labour on the analyſis of the ſulphureous waters of Aix-la-Chapelle. I have found that the excipient of the ſulphur in theſe waters is not hydrogen, as has hitherto been ſuppoſed and aſſerted, but azote. Theſe waters contain no ſulphurated hydrogen.

"Sulphurated azotic gas is a chemical body, the exiſtence of which has never been ſuſpected. Nature preſents it in abundance in the mineral waters of this country.

"Beſides ſulphurated azote, theſe waters contain a great deal of pure and free azote interpoſed, and in the ſemigaſeous ſtate."

5. We are informed by Mr. Paul of Geneva (now in London) that water holding ſulphate of lime in ſolution, and impregnated under preſſure with hydrogen gas, of which it can take up about one-fourth of its volume, after ſtanding ſtopped up for about three months, on being opened was found to contain ſulphurized hydrogen gas. This has been repeated ſeveral times with the ſame reſult. In diſtilled water not impregnated with ſulphate of lime, the hydrogen gas undergoes no change.

How is the ſulphurized hydrogen gas produced? There is plainly a decompoſition effected, not merely of the ſulphate of lime, but of the ſulphuric acid, which parts with a portion of its ſulphur.—What becomes of the oxygen? Does it join the hydrogen, and form water?

The hydrogen gas made uſe of in the above experiments was procured from the decompoſition of water, by applying it to iron expoſed to a high temperature. When obtained by means of ſulphuric acid, water, and iron, in the humid way, it has a different flavour, ſomewhat hepatic.

6. Mr. Paul has mentioned to us another circumſtance, which deſerves to be made known. Water impregnated with hydrogen gas is extremely injurious in dropſical complaints. A gentleman labouring under the dropſy was cured by drinking water impregnated with oxygen gas; but during the cure his ſervant by miſtake had purchaſed ſome hydrogen water. In leſs than 24 hours his malady returned: on finding out the cauſe, and having recourſe to the oxygenated water, the miſchief was ſpeedily remedied.

ANIMAL GALVANISM.

Some curious Galvanic experiments were made on February 18, by profeſſor Aldini, in Dr. Pearſon's lecture-rooms. They were by far more intereſting and ſatisfactory than any we have yet noticed on animals, owing to the pains taken to procure the fitteſt ſubjects for the operations. They were inſtituted in the preſence of his excellency the ambaſſador of France general Andreoſſi, lord Pelham, lord Roxburgh, lord Caſtlereagh, lord Hervey, the honourable Mr. Upton, Mr. Cholmondely, Mr. Anchora, Mr. Elliot, and ſeveral other gentlemen of rank. The profeſſor was aſſiſted ably, as on former occaſions, by Mr. Carpue, Mr. Cuthbertſon, and Mr. Hutchins.

Among other important facts, it was deciſively ſhown,

1. That a vital attraction ſubſiſts between a nerve and muſcle: for the ſuſpended ſciatic nerves of a frog, after detaching

taching the ſpine, being brought near the intercoſtal muſcles of a dog, while the aſſiſtant who held the frog did, with his other hand, touch the muſcles of the thigh of the dog, (thus forming a circle); in this ſituation, the nerves ſuſpended approached, and came into contact with the muſcle, as evidently as a ſilken thread is attracted by ſealing-wax.

2. The heart of a rabbit was excited to action in a little time after the animal was killed; but vitality diſappeared much ſooner than in the other muſcles: ſo that this organ is the *primum*, and not, as Harvey aſſerted, the *ultimum moriens*. The lungs, liver, and ſpleen could not be excited to action, even immediately after the animal was killed.

3. The moſt important fact of all was that of exciting contractions by making a circle of nerves and muſcles of different animals, without any metallic exciter or conductors.

4. The head of an ox, recently decapitated, exhibited aſtoniſhing effects: for, the tongue being drawn out by a hook fixed into it, on applying the exciters, in ſpite of the ſtrength of the aſſiſtant the tongue was retracted, ſo as to detach itſelf, by tearing itſelf, from the hook: at the ſame time a loud noiſe iſſued from the mouth by the abſorption of air, attended by violent contortions of the whole head and eyes.

After the exhibition of theſe phænomena of Galvaniſm, which, among other philoſophers, particularly occupy the attention of the Firſt Conſul, the company partook of an elegant *dejeune* and a *converſazione* in Dr. Pearſon's houſe.

XVII. *Deſcription of Mr.* PEPYS's *large Galvanic Apparatus.*

MR. PEPYS jun. has lately conſtructed the moſt powerful Galvanic apparatus that has, we believe, been yet produced, of which we ſhall endeavour to give our readers ſome idea. It conſiſts of 60 pairs of zinc and copper plates diſpoſed in two troughs conſtructed on Mr. Cruickſhank's plan, but with ſome accompanying arrangements which are extremely convenient and uſeful. That our deſcription may be the better underſtood, we refer to a view of the apparatus, Plate I.

A A. The two troughs, each conſiſting of 30 pairs of plates. The plates are cemented into troughs made of mahogany, properly varniſhed to ſtand the action of the fluids which may be introduced into the cells between the plates. The plates are each 6 inches ſquare, or contain 36 ſquare inches of ſurface on each ſide; and that they may be able to conti-

nue in uſe for a conſiderable time even with the application of ſtrong acidulous liquors, they are made of ſuch a thickneſs that each pair weighs four pounds. The troughs are furniſhed with pivots at each end, by which they reſt on the bar B of the table into which the whole apparatus is framed: on theſe pivots, which are ſo placed as to throw the centre of gravity of the troughs below them, the troughs revolve when it is neceſſary to empty the cells.

C. A japanned iron tray or receiver, capable of holding the contents of the cells of the troughs when they are emptied.

D. A range of ſix funnels made of tin, the diſtances of the tubes of which are ſo adjuſted that they enter ſix of the cells of the troughs at once.

E. A tin veſſel having ſix diſtinct cells with ſpouts, each capable of containing the exact quantity of fluid neceſſary to fill a cell of the trough.

By means of the funnel D and veſſel E, the troughs are filled with very little trouble, no more being neceſſary than to fill the veſſel E by dipping it into the fluid or acid, and then emptying it, by the ſpouts correſponding with the diviſions in the funnel D, into the latter, the pipes of which are placed in the correſponding cells of the troughs.

FF the two prime conductors, being two braſs rods with ſhoulders. They paſs through holes in the top of the table (which, to ſhow the arrangement of the troughs, appears elevated in the engraving) into the end cell of each trough.

G G two metallic conductors, each conſiſting of two parts joined by ſockets, and each having a ſocket which fits upon the prime conductors F F. By this contrivance they are movable in any direction required in the experiments.

H H two ſhallow veſſels made of tin, with tubes underneath, by means of which they can be placed on the tops of the conductors. When filled with water they are found convenient for giving the Galvanic ſhock in a very perfect manner, and for other experiments.

I, an arch of metal to unite the troughs at their further ends.

When the cells are filled, the troughs connected by the metallic arch, the top or lid ſhut down, and the prime conductors put in their places, the apparatus forms a Galvanic table free from any incumbrance, and extremely well adapted for every experiment in Galvaniſm.

On the 21ſt of February, with a number of ſcientific gentlemen, we witneſſed a trial of this apparatus. The experiments

ments made by Mr. Pepys on the deflagration of metals were the moſt brilliant and ſplendid we ever beheld.

The troughs were filled with 32 pounds of water mixed with two pounds of concentrated nitrous acid. With this charge—

Iron wires of $\frac{1}{200}$ to $\frac{1}{10}$ of an inch in diameter were deflagrated with great ſplendour. A number of the ſmall ones twiſted together produced ſomewhat like a little bruſh: a pleaſing appearance in deflagration.

Charcoal of box wood was not only deflagrated at the place of contact, but remained permanently red hot for near two inches in length.

Lead foil burnt with great vividneſs, becoming red hot, and emitting a ſmall volcano or adjutage of red ſparks along with the flame.

Tin foil burnt with great ſplendour, with ſmoke and ſparks.

Dutch leaf or braſs foil deflagrated vividly, with ſmoke and a profuſion of ſparks.

Silver leaf burnt with an intenſe vivid green light: no ſparks, but much ſmoke or fume.

Gold leaf deflagrated with bright white light and ſmoke.

Tin wire, 1-eighth of an inch in diameter, fuſed, burnt, and oxidated, with great ſplendour.

Platina wire, 1-ſixteenth of an inch in diameter, became red hot, white, and fuſed into globules at the contact.

Gunpowder, phoſphorus, and inflammable ſubſtances, are inſtantly fired by contact with conductors armed with charcoal.

The Galvanic power was capable of deflagrating charcoal, after paſſing through ſixteen perſons with wetted hands joined.

The beſt method of ſhowing theſe experiments is by introducing into an earthen plate filled with clean mercury, one of the prime conductors. The other conductor may be then mounted with the ſeveral ſubſtances to be tried. The leaves and laminæ of metal eaſily adhere to it by wetting. Wires may be twiſted round it, and will alſo ſerve to bind other ſubſtances to it.

The power of this large trough was ſo great as to keep up the deflagrations and combuſtion without intermiſſion.

XVIII. *An Eſſay on the Method of determining the Difference of Longitude between Places at Land, from the obſerved Tranſits of the Moon over their Meridians; with a Demonſtration and Example. By Mr.* GAVIN LOWE *.

> Ea eſt methodorum ſimpliciſſimarum ratio atque natura, ut poſtremæ in mentem veniant, et niſi obſtinatiore animo, ne veniant quidem.
>
> BOSCOVICH.

ALTHOUGH the method of determining the difference of longitude at ſea from the lunar obſervations has been accurately laid down by Dr. Maſkelyne and other able nautical aſtronomers, it has, however, happened that ſeveral writers on longitude and aſtronomy have, in the courſe of the laſt twenty years, given rules for finding the difference of longitude at land from the moon's tranſits, either ſo erroneous or imperfect, that the adoption thereof might do a ſerious injury both to navigation and geography: they have given examples, but no demonſtrations; or at leaſt ſuch obſcure and imperfect ones, as prove that they had not a clear conception of the matter.

It is for theſe reaſons that the following demonſtration of a rule both eaſy and accurate for finding the difference of longitude, is now propoſed. The data are the obſerved increaſe of the moon's right aſcenſion in paſſing from the firſt to the ſecond meridian, and the increaſe of the ſun's and moon's right aſcenſion in twelve hours apparent time, which may be had from the Nautical Almanac.

DEMONSTRATION.

Let the circle A B C repreſent the equator, P its pole, and A P D the firſt meridian, as that of Greenwich. Suppoſe that the centres of the ſun, the moon, and a fixed ſtar are on that meridian at the ſame moment of time as repreſented at A, and that they move from thence to the weſtward with their reſpective velocities, the earth being conſidered as at reſt. Then, after twelve hours apparent time, the ſun will be at D, the oppoſite point to A, or 180° diſtant from it; but the fixed ſtar, moving in appearance over a greater ſpace than 180° in twelve hours apparent time, will be at E; while the moon, with a motion apparently ſlower than the ſun and the ſtar, will appear after twelve hours at the point B, or on a meridian B P. But E D is the diſtance of the ſun from the ſtar after an interval of twelve hours apparent time, and EB the diſtance of the moon, or, in other words, the increaſe of their reſpective right aſcenſions: and ſince ED and EB are known from the Nautical Almanac, if we ſubtract the firſt from the laſt, we have DB, equal to the difference between the increaſe of the ſun's and moon's right aſcenſion in twelve hours apparent time. Now the difference of longitude between the two meridians AP and BP is the arc $A\beta B$, equal to $A\beta D$ leſs the arc DB; that is, equal to 180° leſs the difference between the increaſe of the ſun's and moon's right aſcenſion in twelve hours: and, ſince the increaſe of the moon's right aſcenſion from the time of its paſſing the meridian AP to the time of its paſſing BP is known from obſervation, and equal to EB, we can make the following proportion for finding the difference of longitude between any other two meridians, AP and βP, from the obſerved increaſe of the moon's right aſcenſion $\varepsilon\beta$.

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* Communicated by the Author.

As $EB : A\beta D - DB :: \varepsilon\beta : A\beta$ the difference of longitude; or, in more familiar language, as the increaſe of the moon's right aſcenſion in twelve hours apparent time is to 180° or 12^h leſs the difference between the increaſe of the ſun's and moon's right aſcenſion in that time ::, ſo is any other obſerved increaſe of the moon's right aſcenſion between two meridians : to their difference of longitude. Q. E. D.*

If the increaſe of the moon's right aſcenſion in twelve hours were uniform, or ſuch that equal parts of it would be produced in equal times, the above rule would be ſtrictly accurate; but as that increaſe ariſes from a motion continually accelerated or retarded, and ſeldom uniform but for a ſhort ſpace of time, it will therefore be neceſſary to find the mean increaſe of the moon's right aſcenſion when it is at the intermediate point between A and β, in order to determine their difference of longitude with the greateſt preciſion; and for that purpoſe, Taylor's Tables of Second Difference are very uſeful.

EXAMPLE.

April the 8th, 1800, the tranſit of the moon's firſt limb was obſerved at the royal obſervatory (A); and, allowance being made for the error of the clock, its right aſcenſion

was - - - - -	12^h	35^m	$18{\cdot}22^s$
Add the time that the moon's ſemi-diameter took to paſs the meridian -	0	1	8·38
Right aſcenſion of the moon's centre	12	36	26·6

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* Demonſtrated for the firſt time by the author in 1770.

Brought over	12^{h}	36^{m}	$26{\cdot}6^{s}$
On a meridian (β[*]) far to the weſtward the tranſit of the moon's firſt limb was obſerved the ſame day, and being reduced to the centre, its right aſcenſion was -	12	47	56·7
Increaſe of right aſcenſion between A and β - - - - -	0	11	30·1
The increaſe of the moon's right aſcenſion in twelve hours apparent time per the Nautical Almanac was - - -	0	26	3[†]
The increaſe of the ſun's in the ſame time	0	1	49·65
Difference - - - -	0	24	13·35

And 12 hours minus this difference is = $11^{h}\ 35^{m}\ 46{\cdot}65^{s}$; therefore, As $26^{m}\ 3^{s}$: $11^{h}\ 35^{m}\ 46{\cdot}65^{s}$: : $11^{m}\ 30{\cdot}1^{s}$ to $5^{h}\ 7^{m}\ 12^{s}$, the correct difference of longitude between A and β.

By reducing the three terms to ſeconds, and uſing logarithms, the operation is much ſhortened.

In a book publiſhed by Mr. Mackay on longitude about fifteen or ſixteen years ago, there is a rule given, and alſo an example, for finding the difference of longitude at land from the tranſits of the moon, but no demonſtration. The rule, when diveſted of its high-ſounding enunciation, runs thus:

As the increaſe of the moon's right aſcenſion in twelve hours apparent time : is to 180° : :, ſo is any other obſerved increaſe between two meridians : to their difference of longitude. It follows from this, that the moon as well as the ſun would, in twelve hours apparent time, paſs over an arc of 180°, although the apparent motion of the moon to the weſtward in twelve hours, or 180° of ſpace, be leſs than that of the ſun by ſix or ſeven degrees; and ſo much error would this method produce, if the two places differed about 180° in longitude.

The above example, wrought according to Mackay's rule, would come out thus:

As $26^{m}\ 3^{s}$: 12^{h} : : $11^{m}\ 30{\cdot}1^{s}$ to -	5^{h}	17^{m}	$53{\cdot}7^{s}$
But the correct difference as above is -	5	7	12
Error - -	0	10	41·7

which amounts to more than 2½°, or 150 miles, in a difference of longitude little exceeding five hours.

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[*] A place near Port-Royal, in Jamaica.

[†] By interpolation $26^{m}\ 3{\cdot}67^{s}$; but the fraction of the ſecond was not minded.

Mr. Edward Pigott adopts the very ſame rule for determining the difference of longitude between Greenwich and York, and ſtates the reſult in the Philoſophical Tranſactions for 1786, p. 417.

Mr. Vince has inſerted this rule and example in his Treatiſe of Practical Aſtronomy; but we have to regret that they were not accompanied with a ſtrict demonſtration.

The Rev. Mr. Wollaſton, in the appendix to his Faſciculus Aſtronomicus, publiſhed two or three years ago, has given a rule, without demonſtration or example, for finding the difference of longitude from the moon's tranſits, which produces the ſame error as Mackay's and Pigott's, although worded differently from theirs. Mr. Wollaſton makes the firſt term of his proportion apparent, and the third mean time; this renders the reſult erroneous. Since the motion of the ſun, moon, and planets are computed for apparent time, and given ſo in the Nautical Almanac, mean time is not at all requiſite for reſolving the difference of longitude either at ſea or at land. We ſhall therefore endeavour to apply Mr. Wollaſton's rule, according to its literal meaning, for finding the difference of longitude from the above obſervations.

The right aſcenſion of the moon's centre on the meridian of Greenwich being known, we can eaſily deduce the mean and apparent time correſponding to it; and in like manner the mean and apparent time at the diſtant meridian β*. The apparent and mean time of the tranſits of the moon's centre over the meridians of A and β, when ſtrictly computed, were as follows:

	Apparent Time.			Mean Time.		
At A - -	11^{h}	26^{m}	$47{\cdot}81^{s}$	11^{h}	28^{m}	$33{\cdot}5^{s}$
At β - -	11	37	29·5	11	39	11·4
Time later at β than at A	0	10	41·69	0	10	37·9

From the increaſe of the moon's right aſcenſion in twelve hours - -	26	3
Subtract the increaſe of the ſun's right aſcenſion in that time - -	1	49·65
The moon's retardation in twelve hours - -	24	13·35

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* Mean time, however, at β, before its longitude be known, is not a fair poſtulatum. The method above demonſtrated does not require it.
G. L.

Then, "As twice the moon's retardation in twelve hours : is to twenty-four hours : :

"So is the mean time later at β than at A : to the difference of longitude weſt from A."

After doubling 24^m 13·35^s, and alſo 12, which is totally unneceſſary, as the reſult would be the ſame if they ſtood ſingle, we ſtate the following proportion:

As 48^m 26·7^s : 24^h : : 10^m 37·9^s to--5^h 15^m 1·3^s, the difference of longitude between A and β.

But as the third term is improperly reduced to mean time, we ſhall take the apparent time above found, and then 48^m 26·7^s : 24^h : : 10^m 41·69^s to--5^h 17^m 53·7^s; the ſame as reſults from Mackay's and Pigott's rules.

We ſhall only remark, that 5^h 17^m 53·7^s is the apparent time that the moon took in paſſing from the meridian of A to the meridian of β; but from what has been demonſtrated, the apparent time at β will be equal to the difference between the increaſe of the ſun's and moon's right aſcenſion in that interval of apparent time; for DB, or 24^m 13·35^s is the difference for twelve hours, and therefore by proportion δβ, or 10^m 41·69^s will be the difference for 5^h 17^m 53·7^s; ſubtracting the former from the latter, we have 5^h 7^m 12^s, the difference of longitude as before, and a clear proof that the authors above mentioned have omitted to deduct the apparent time at the diſtant place or ſtation β, from the apparent time at Greenwich.

If it ſhould be thought eaſier to employ ſidereal time in reſolving the difference of longitude between A and β, let ED, the increaſe of the ſun's right aſcenſion in twelve hours, be added to twelve hours, and we have then the arc of ſidereal time ABDE; from which ſubtracting EB, there remains AβB, equal to the difference of longitude. By proportion we can therefore ſay, As EB, the increaſe of the moon's right aſcenſion in twelve hours; is to ABD + DE, or twelve hours + the increaſe of the ſun's right aſcenſion; ſo is any other obſerved increaſe of the moon's right aſcenſion as εβ; to the arc of ſidereal time Aβε; and if from this we ſubtract εβ, then Aβ is the difference of longitude required.

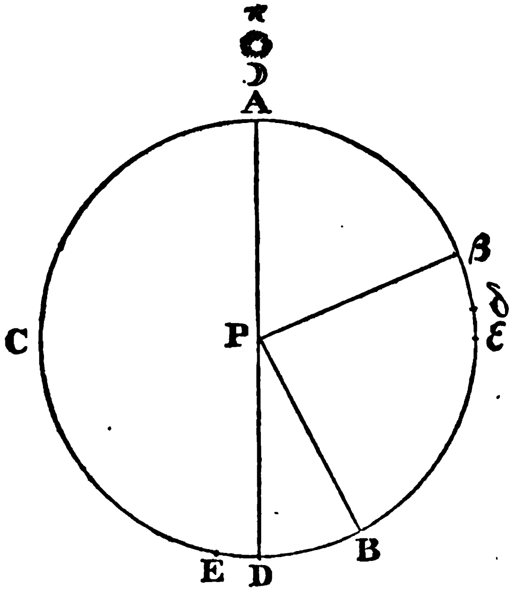
To apply this rule for finding the difference of longitude from the above obſervations and data, we ſay,

As 26^m 3^s : 12^h 1^m 49·65^s : : 11^m 30·1^s to Aβε 5^h 18^m 42·1^s
From this ſubtract εβ, the obſerved increaſe 0 11 30·1

The difference of longitude between A and β is 5 7 12

Q. E. I.

Although we can ſafely truſt to the accuracy of the obſervations at the royal obſervatory, yet, at the diſtant ſtation β, great attention muſt be paid in adjuſting the tranſit inſtrument truly to the meridian, and taking care that it be perfect in collimation before the moon's tranſit be obſerved there; and alſo, that correct time be deduced as ſoon before or after the moon's tranſit as poſſible, from obſerving the meridian paſſage of one or more of the thirty-ſix ſtars, whoſe right aſcenſions, after a long ſeries of obſervations, have been aſcertained by the Aſtronomer Royal with the greateſt preciſion.



In order to put the above method to the teſt of experiment, a number of tranſits of the moon's firſt limb were obſerved many years ago at a place 26 ſeconds of time weſt from the royal obſervatory, with a ſmall portable tranſit inſtrument made by that eminent artiſt Mr. Edward Troughton, of Fleet-ſtreet. It had a teleſcope 20 inches long, a treble object-glaſs, and the price only 20 guineas. When the obſervations at Greenwich were publiſhed, all thoſe that were made on the ſame days at both places were employed to determine the difference of longitude, and the mean reſult of four or five correſponding obſervations gave it true to two ſeconds of time.

This is only mentioned to ſhow, that with ſmall tranſit inſtruments well conſtructed, and due attention paid to their adjuſtments, obſervations may be made ſufficiently accurate for anſwering the ends of practical aſtronomy.

We expect ſoon to have the lunar tables brought to a greater degree of perfection, by applying certain equations that have been lately diſcovered, and by which thoſe purpoſes will, of courſe, be more ſucceſsfully promoted. It is alſo much to be wiſhed that the moon's tranſits were as aſſiduouſly obſerved by public and private aſtronomers in every country as they are at the royal obſervatory in this, and from time to time communicated to the public at large.

It ſometimes, but not very frequently, happens, that favourable weather will permit one aſtronomer to obſerve four tranſits of the moon over his meridian in the ſame number of ſucceeding days. If another, or indeed a hundred others, ſhould obſerve only one or two on any of thoſe days, whether at the neareſt or remoteſt ſtations from the firſt meridian; ſuch obſervations are of great value, and afford the very beſt means of finding the difference of longitude with accuracy, becauſe the errors of the lunar tables are thereby completely obviated.

Paradiſe Row, Iſlington,
February 14, 1803.

XIX. *On the Preparation of the Hyacinthus non ſcriptus, or common Blue-Bell, as a Subſtitute for Gum-Arabic.* By Mr. THOMAS WILLIS, *of Lime-Street, London**.

IN the year 1794, whilſt collecting plants in a wood for botanical ſpecimens, I obſerved that the root of the *hyacinthus non ſcriptus*, the plant commonly called blue-bells, or hare-bells, was extremely mucilaginous; and, on taſting it, I diſcovered only a very ſlight pungency. I collected a pound of the bulbs, and, after ſlicing and drying them before a fire, they yielded about four ounces of powder. I thought that, by keeping the powder ſome time, the little acridneſs might go off, as it does in the arum-root powder. I taſted it about ſix months after, and found it perfectly inſipid. I concluded it might be rendered uſeful for food or nouriſhment, but at that time purſued the matter no further.

In the ſpring of 1800, gum-arabic having been a long time very dear, and likely to continue ſo, I thought this mucilaginous root might anſwer ſome of its purpoſes for external uſe. I therefore procured ſeven pounds and a half of the bulbs, which, when ſliced and dried, produced two pounds of powder. Being ſoon afterwards in company with Mr. Charles Taylor, ſecretary to the Society of Arts, &c. I mentioned to him that I had diſcovered a root which grew in great plenty in this kingdom, yielded a very ſtrong mucilage, and which I imagined would anſwer the purpoſes of gum-arabic in ſome of the manufactories. He ſaid, if I pleaſed, he would ſend ſome of it down to Mancheſter, to be tried by the calico-printers.

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* From the *Tranſactions of the Society for the Encouragement of Arts, &c.* vol. xx., who voted a ſilver medal to the author for his communication.

Three or four ounces of the powder were given him, and ſent down there: he was informed, upon trial, that it anſwered the purpoſes of fixing the calico-printers' colours equally as well as gum-arabic; and in the ſame proportion, of an ounce and a half of the powder to four ounces of the mordant. Mr. Taylor received the ſamples of the printed cottons on which it had been uſed.

On the 15th of January 1801 I furniſhed Mr. Taylor with eight ounces more of the powder; but have not ſince heard the reſult.

As this root can be eaſily procured, and uſed at a leſs price than gum-arabic has been ſold for ſeveral years paſt, I think it may be rendered of great utility; and the Society of Arts, &c., by patronizing it, may be the means of making it a public benefit.

Care ſhould be taken, and advice given, that the woods ſhould not be left deſtitute of the roots; and it would be adviſeable to offer premiums for cultivating the roots and offſets, as they are very increaſing. By ſuch means a conſtant ſupply may be had, if the roots anſwer the intended purpoſes.

I do not preſume to offer any thing reſpecting the mode in which the ſociety may think proper to divulge the diſcovery, and promote the uſe of theſe roots; but I imagine, that if the roots are bruiſed and uſed freſh, they would anſwer the purpoſe better than when dried and powdered; and as it is now a proper time of the year for taking them up, and will continue to be ſo for two months, I wiſh that the diſcovery may be made known as ſoon as poſſible.

I have ſent you ſpecimens both of the dried roots and powder, that they may be ſeen at the ſociety's rooms by the calico-printers. What I have done have been ſcorched a little in drying; but the colour would be much better, if proper care was taken in drying them. I am, &c.

Lime-ſtreet, THOMAS WILLIS.
March 17, 1802.

From the trials made before the committee with this powder with hot and cold water, from ſamples of the printed cotton

ton produced which had been printed therewith inſtead of gum-ſenegal, and from experiments made in Mancheſter, it appears that the *hyacinthus non ſcriptus* may, in many caſes, be found a uſeful ſubſtitute for gum-arabic.

XX. *On the Edulcoration of Fiſh-Oil.* *By* ROBERT DOSSIE, *Eſq.**

Explanation of the Principles on which the Purification of Fiſh-Oil may be performed, and of the Uſes to which it is applicable.

THAT the fœtid ſmell of fiſh-oil is chiefly owing to putrefaction, it is unneceſſary to ſhow; but, though this be the principal cauſe, there is another likewiſe, which is, uſtion, or burning the oil, occaſioned by the ſtrong heat employed for the extracting it from the blubber of the larger fiſh, and which produces a ſtrong empyreumatic ſcent that is not always to be equally removed by the ſame means as the putrid ſmell, but remains ſometimes very prevalent after that is taken away.

In order to the perfect edulcoration of oils there are conſequently two kinds of fœtor or ſtink to be removed; viz. the putrid, and the empyreumatic: and the ſame means do not always equally avail againſt both.

The putrid ſmell of fiſh-oil is of two kinds: the rancid, which is peculiar to oils; and the common putrid ſmell, which is the general effect of the putrefaction of animal fluids, or of the vaſcular ſolids, when commixed with aqueous fluids.

Fiſh-oil has not only rancidity, or the firſt kind of putrid ſmells peculiar to oils, but alſo the ſecond or general kinds; as the oil, for the moſt part, is commixed with the gelatinous humour common to all animals, and ſome kinds with a proportion of the bile likewiſe; and thoſe humours putrefying combine their putrid ſcent with the rancidity of the oil, and, in caſes where great heat has been uſed, with that and the empyreuma alſo.

The reaſon of the preſence of the gelatinous fluid in fiſh-oil is this: that the blubber, which conſiſts partly of adipoſe veſicles, and partly of the membrana celluloſa, which con-

ments made by Mr. Pepys on the deflagration of metals were the moſt brilliant and ſplendid we ever beheld.

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* From the *Tranſactions of the Society for the Encouragement of Arts, &c.* vol. xx. So far back as the year 1761 the ſociety voted him a bounty of one hundred pounds for this communication, though they did not publiſh it till 1802.

The troughs were filled with 32 pounds of water mixed with two pounds of concentrated nitrous acid. With this charge—

Iron wires of $\frac{1}{200}$ to $\frac{1}{10}$ of an inch in diameter were deflagrated with great ſplendour. A number of the ſmall ones twiſted together produced ſomewhat like a little bruſh: a pleaſing appearance in deflagration.

Charcoal of box wood was not only deflagrated at the place of contact, but remained permanently red hot for near two inches in length.

Lead foil burnt with great vividneſs, becoming red hot, and emitting a ſmall volcano or adjutage of red ſparks along with the flame.

Tin foil burnt with great ſplendour, with ſmoke and ſparks.

Dutch leaf or braſs foil deflagrated vividly, with ſmoke and a profuſion of ſparks.

Silver leaf burnt with an intenſe vivid green light: no ſparks, but much ſmoke or fume.

Gold leaf deflagrated with bright white light and ſmoke.

Tin wire, 1-eighth of an inch in diameter, fuſed, burnt, and oxidated, with great ſplendour.

Platina wire, 1-ſixteenth of an inch in diameter, became red hot, white, and fuſed into globules at the contact.

Gunpowder, phoſphorus, and inflammable ſubſtances, are inſtantly fired by contact with conductors armed with charcoal.

The Galvanic power was capable of deflagrating charcoal, after paſſing through ſixteen perſons with wetted hands joined.

The beſt method of ſhowing theſe experiments is by introducing into an earthen plate filled with clean mercury, one of the prime conductors. The other conductor may be then mounted with the ſeveral ſubſtances to be tried. The leaves and laminæ of metal eaſily adhere to it by wetting. Wires may be twiſted round it, and will alſo ſerve to bind other ſubſtances to it.

The power of this large trough was ſo great as to keep up the deflagrations and combuſtion without intermiſſion.

manufacture, as the heat through which this is in ſome caſes employed cauſes this matter to contract a moſt diſagreeable empyreuma. It alſo prevents its burning in lamps, as well from its viſcidity as from the repugnance which the preſence of water gives to all oleaginous matter. It is therefore neceſſary to free the oil from this heterogeneous matter; after which it can be ſubject only to the rancid putreſcence, or that which is proper to oils as ſuch.

The ſubſtances which have been or may be applied to the removing or preventing the effects of putreſcence, are, acids, alkalies, metallic calces, neutral ſalts, ethereal and eſſential oils, vinous ſpirits, water, and air. With reſpect to acids, though they may be applied with effect to the removal or prevention of putrefaction in mixed animal and vegetable ſubſtances, yet they have not the ſame efficacy when employed in the caſe of oils; for in a ſmall proportion, without the ſubſequent aid of alkalies, they rather increaſe than diminiſh the fœtor, and in a large proportion they coagulate the oils, and change their other properties as well as their conſiſtence. Though they might therefore be employed with the aſſiſtance of alkalies, yet, requiring a more expenſive and complex proceſs, and not being moreover neceſſary, as the ſame end may be obtained by the uſe of alkalies only, they may be deemed improper for the purification of animal oils for commercial purpoſes. Alkaline ſubſtances, both ſalts and earths, are the moſt powerful inſtruments in the edulcoration of oils; but as their action on putrid oils, and the method of applying them to this end, are not the ſame in both, it is proper to conſider them diſtinctly.

Of alkaline ſalts it is the fixed kind only which are proper to be uſed for the edulcoration of oils. Fixed alkaline ſalts, in a diſſolved ſtate, being commixed with putrefying animal ſubſtances, appear to combine with the putrid matter, and, mixing with ſome of the principles, form inſtantly volatile alkaline ſalts. On the leſs putrid they ſeem to act, after their combination, by an acceleration of the putreſcent action, till they attain the degree which produces volatile ſalts. This is evident by the ſenſible putrid ferment and ſmell which appear after their commixture; but which gradually abating, the oil is rendered ſweeter, much lighter coloured, and thinner.

Their great uſe in the edulcoration of fiſh-oil ariſes therefore from their converting ſuch parts of the gelatinous fluid and bile as are highly putrefied inſtantly into volatile ſalts, and cauſing a rapid putrefaction of the other parts; by which means the oil is freed from them by their diſſipation. They do not, however, equally act on the parts of the oil on which the empyreumatic ſcent depends, unleſs by the aſſiſtance of heat; for when they are commixed with the oils without heat, in proportion as the putrid ſmell diminiſhes, that becomes more ſenſibly prevalent. The ultimate action of lixiviate ſalts on animal oils, except with reſpect to the empyreuma, ſeems to be the ſame either with or without the medium of heat; for the ſame urinous and putrid ſmell, gradual diminution of the colour, and fœtid ſcent, happens in one caſe as in the other, except with regard to the acceleration of the changes; and ſuch ſalts, where the purification is required to be made in a great degree, are a neceſſary means, as they are more effectual than any other ſubſtance that can be employed.

The uſe of lixiviate ſalts alone is not, however, the moſt expedient method that can be purſued for the edulcoration of oils, for ſeveral reaſons. If they be uſed alone, cold, in the requiſite proportions, they coagulate a conſiderable part of the oil, which will not again ſeparate from them under a very great length of time; and when they have deſtroyed the putrid ſcent, a ſtrong bitter empyreumatic ſmell remains. The ſame inconvenience, with relation to the coagulation of part of the oil, reſults when they are uſed alone with heat. The ſuper-addition of common ſalt (which reſolves the coagulum and counteracts the ſaponaceous power of the lixiviate ſalt, by which the oil and water are made to combine) is therefore neceſſary; and the expenſe ariſing from the larger proportion of lixiviate ſalt requires it to be employed if no other alkali be taken in aid, and renders the junction of alkaline earths with it extremely proper in the edulcoration of oils for commercial uſes. Lime has alſo an edulcorative power on animal oils; but it has alſo ſo ſtrong a coagulative action, that the addition of a large proportion of alkaline ſalts becomes, when it is uſed, neceſſary to reduce the concreted oil to a fluid ſtate; and therefore this ſubſtance alone is not proper for that purpoſe. The combination of lixiviate ſalt with lime, or the ſolution commonly called ſoap-lye, has an effectual edulcorative action on fœtid oils; but it makes a troubleſome coagulation of part of the oil if no common ſalt be employed, and muſt be uſed in ſuch large proportion, if no alkaline earth be added, as renders the method too expenſive.

Lime has a power of combining with and abſorbing the putrid parts of the gelatinous fluid and bile when commixed with oil, and effects, either with or without heat, a conſiderable edulcoration of fœtid oils; but it combines ſo ſtrongly with them, either cold or hot, that the ſeparation is difficult to be effected even with the addition of brine; and the oil, when a large proportion of it is uſed, can ſcarcely be at all brought from its concreted to a fluid ſtate but by an equivalent large proportion of lixiviate ſalt: the uſe of lime, therefore, alone is improper, or even in a great proportion with other ingredients. But when only a leſſer degree of edulcoration is required, a moderate quantity, conjoined with an equal or greater weight of chalk, which aſſiſts its ſeparation from the oil, may, on account of its great cheapneſs, be employed very advantageouſly: it will in this caſe admit of precipitation from the oil by the addition of brine. It may be alſo expediently uſed when lixiviate ſalt is employed with heat for the moſt perfect purification of oils; for it will in that caſe give room for the diminiſhing of the quantity of lixiviate ſalt, though the proportion be nevertheleſs ſo reſtrained as not to exceed what the proportion of lixiviate ſalt (juſt requiſite for the edulcoration) can ſeparate from the oil.

Chalk has an abſorbing power ſimilar to lime, but in a leſs degree, on the putrid ſubſtance of oil: it does not, however, combine ſo ſtrongly with the oil as to reſiſt ſeparation in the ſame manner, and is therefore very proper to be conjoined either with lixiviate ſalts or lime, as it renders a leſs quantity of either ſufficient, and indeed contributes to the ſeparation of the oil from them.

Magneſia alba, or the alkaline earth, which is the baſis of the ſal catharticus, and the ſingular earth which is the baſis of alum, both have an edulcorating power on fœtid oils, but, like lime, have too ſtrong an attraction with them to be ſeparated ſo as to admit of the reduction of the oil from the concreted ſtate to which they reduce it; and therefore, as they are not ſuperior in efficacy to lime and chalk, but much dearer or more difficult to be obtained, they may be rejected from the number of ingredients that are proper for the purifying of oils, with a view to commercial advantages.

Sea ſalt has an antiſeptic power on the mixed ſolid parts of animals; but uſed alone, or diſſolved in water, it does not appear to leſſen the putrid fœtor of oils, but, on the contrary, rather increaſes it. If after their commixture with it they are ſubjected to heat, it rather depraves than improves the oils; but though by its own immediate action on them it conduces ſo little to the edulcoration of oils, yet it is a medium for the ſeparation of water and the alkaline ſubſtances requiſite to be employed to that end. It is of great utility in the edulcorative proceſſes; for when alkaline ſalts or earths combine with the water neceſſary to their action on the oils;

as is ſtill uſual among the Illyrian women in the interior part of Dalmatia, and among thoſe of Kamtſchatka and on the banks of the Anadir.

3d, A Madona with infant Jeſus aſleep, painted by Aloys Vivarinus, of Murano, on a plaſter ground. This painting, which was finiſhed in 1489, is five feet two inches in height and one foot eleven inches in breadth. It was executed at the time when the art of painting in oil was firſt carried from Flanders to Italy. It proves that Vivarinus, by his delicate and expreſſive pencil, had already carried to a high degree of excellence the art of painting, which was not brought to a ſtate of perfection till the time of Charles V.

4th, A naval battle painted in oil, eleven feet in height and twenty-one in breadth. It is a maſter-piece of Tintoretto. He compoſed it at the requeſt of the ſenate of Venice, who preſented it to the city of Piſano. It repreſents the battle ſaid to have taken place in 1177 between the Germans and the Venetian fleet near Cape de Salvora, in Iſtria, and which was firſt mentioned by Andrew Dandolo, in his Chronicle, two centuries after.

5th, A buſt, painted on marble, of Sanctorius, a celebrated profeſſor of Padua, who died in 1639, and who formed an epoch in the philoſophical hiſtory of medicine.

EARTHQUAKE.

Some obſervations have lately been publiſhed in the German journals in regard to the curious circumſtances which accompanied the earthquake that took place in the province of the Seven Mountains towards the end of October. It was in the low meadows lying towards the north that the phænomena were moſt ſenſible, where deep fiſſures two or three feet in breadth and thirty-five fathoms in length, which divided into ſeveral branches, were formed in a direction from weſt to eaſt. A great deal of very fine ſand of an aſh-gray colour, which emitted a ſulphureous odour, was thrown up from theſe fiſſures. The principal fiſſure, after extending throughout a long ſpace, ſeparated into ſeveral branches, which bent themſelves into ſemicircles towards the weſt, and traverſed a houſe and a ſtable, after piercing the walls at the foundation. The whole country was covered with one or more inches of ſand in the direction of the fiſſures: and perſons who were witneſſes to theſe phænomena aſſert, that at the time of the laſt ſhock the earth opened with a noiſe like that of a muſket ſhot; that a black and ſtrong ſmelling vapour iſſued from the fiſſures, as well as a great deal of water, which ſpouted up with a loud noiſe; and that theſe jets were followed by a great quantity of ſulphureous ſand. The ground alſo ſunk down in a ſenſible manner. The water which iſſued from the fiſſures was exceedingly ſulphureous, cold, and limpid.

Chemiſts obſerved, that a pound of this ſand diluted in eight pints of water produced no effervefcence, and even gave no precipitate on being mixed with vitriolic acid. The nitrous acid gave the ſame reſult. It was only after its decompoſition, and after gall-nuts were able to produce no change in the colour, that the water became turbid, in ſuch a manner as to ſhow the preſence of the vitriolic acid. By decoction and evaporation ſigns of cryſtallization were obſerved at the ſurface; and, when the evaporation was complete, there remained about eight grains of alkaline matters, two parts of which were Glauber's ſalt, one earthy ſalt, and the other gypſum.

TRANSIT OF MERCURY,

As obſerved at Paris November 9, 1802.

Obſervers.	Interior Contact.			Exterior Contact.		
C. Mechain - -	0^h	6^m	$45{\cdot}4^s$	0^h	8^m	$30{\cdot}4^s$
Lalande - - - - -	0	6	29·0	0	7	56
His nephew and -	0	6	43·0	0	8	18
Burckhardt -	0	6	49	0	8	19
Meſſier - -	0	6	49·2			

The above reſults are mean time reduced to the national obſervatory.

Mechain ſaw the planet very well terminated without any aureola, though he always followed its progreſs on the ſun with a teleſcope of three object glaſſes 4½ inches in diameter and 7 feet focus, with which he took 45 diſtances. He had obſerved in the ſame manner the tranſits in the years 1782, 1786, 1789, and 1799.

CHEMISTRY.

1. Brugnatelli has lately obſerved, that by treating paper with the nitric acid, a large quantity of ſuberic acid mixed with oxalic acid is obtained. This ſeems to prove that Fourcroy was right in claſſing cork among the immediate principles of the vegetable kingdom.

2. The ſame chemiſt in a letter ſays, that after a particular examination of wheat, he thinks he has found that vegetable gluten is nothing elſe than animal fibrous matter. It differs indeed from animal gluten or glue, by its inſolubility in cold water, and by the contraction it experiences in warm water. When treated with the nitric acid it ſwells, produces

duces a foamy effervefcence, gives out nitrous gas, and diſſolves. This ſolution is decompoſed by water, like that of animal fibrous matter, and the white precipitate thence reſulting diſſolves in ammonia, aſſuming a dark yellow colour.

3. A diſcovery of two new principles in ſulphureous waters has lately been made by Counſellor Weſtrumb, of Hammeln, who gives the following details:

"For ſix years I have been excluſively employed in the analyſis of ſulphureous waters. I have found an eaſy and entirely new method of determining what they contain of ſulphurated hydrogen and carbonic acid gas; and I have diſcovered a principle hitherto unknown in theſe waters, namely, bitumen in combination with ſulphurated hydrogen gas and hydro-ſulphurated lime. I communicated the detail of this diſcovery to M. Wurzer. Counſellor Von Crell and Profeſſor Schaub of Caſſel ſaw at my houſe theſe new products. Theſe principles are found in the waters of Neuwdorf, Limmen, Rehburg, and Eylſe. In the laſt place, five ſulphureous ſprings, altogether different from each other, a ſpring of carbonic gaſeous water, and one of freſh water, are found in an extent of leſs than a thouſand ſquare feet. Near Buckembourg, the capital of the ſmall country of Leppe, to which Eylſe belongs, are found two other ſulphureous ſprings perfectly ſimilar, one of which we ſounded to the depth of eighty feet without finding its reſervoir. Baths of mud ſimilar to thoſe of St. Amand have been conſtructed at Eylſe: a very conſiderable quantity of hydro-ſulphurated mud, which may furniſh baths for ſeveral centuries, has been found there."

4. A new gas, ſulphurated azotic gas, has been diſcovered in the mineral waters of Aix-la-Chapelle by M. C. Gimbernat, ſub director of the muſeum of natural hiſtory at Madrid, who gives the following account of it:

"I have juſt finiſhed my labour on the analyſis of the ſulphureous waters of Aix-la-Chapelle. I have found that the excipient of the ſulphur in theſe waters is not hydrogen, as has hitherto been ſuppoſed and aſſerted, but azote. Theſe waters contain no ſulphurated hydrogen.

"Sulphurated azotic gas is a chemical body, the exiſtence of which has never been ſuſpected. Nature preſents it in abundance in the mineral waters of this country.

"Beſides ſulphurated azote, theſe waters contain a great deal of pure and free azote interpoſed, and in the ſemigaſeous ſtate."

5. We are informed by Mr. Paul of Geneva (now in London) that water holding ſulphate of lime in ſolution, and impregnated under preſſure with hydrogen gas, of which it can take up about one-fourth of its volume, after ſtanding ſtopped up for about three months, on being opened was found to contain ſulphurized hydrogen gas. This has been repeated ſeveral times with the ſame reſult. In diſtilled water not impregnated with ſulphate of lime, the hydrogen gas undergoes no change.

How is the ſulphurized hydrogen gas produced? There is plainly a decompoſition effected, not merely of the ſulphate of lime, but of the ſulphuric acid, which parts with a portion of its ſulphur.—What becomes of the oxygen? Does it join the hydrogen, and form water?

The hydrogen gas made uſe of in the above experiments was procured from the decompoſition of water, by applying it to iron expoſed to a high temperature. When obtained by means of ſulphuric acid, water, and iron, in the humid way, it has a different flavour, ſomewhat hepatic.

6. Mr. Paul has mentioned to us another circumſtance, which deſerves to be made known. Water impregnated with hydrogen gas is extremely injurious in dropſical complaints. A gentleman labouring under the dropſy was cured by drinking water impregnated with oxygen gas; but during the cure his ſervant by miſtake had purchaſed ſome hydrogen water. In leſs than 24 hours his malady returned: on finding out the cauſe, and having recourſe to the oxygenated water, the miſchief was ſpeedily remedied.

ANIMAL GALVANISM.

Some curious Galvanic experiments were made on February 18, by profeſſor Aldini, in Dr. Pearſon's lecture-rooms. They were by far more intereſting and ſatisfactory than any we have yet noticed on animals, owing to the pains taken to procure the fitteſt ſubjects for the operations. They were inſtituted in the preſence of his excellency the ambaſſador of France general Andreoſſi, lord Pelham, lord Roxburgh, lord Caſtlereagh, lord Hervey, the honourable Mr. Upton, Mr. Cholmondely, Mr. Anchora, Mr. Elliot, and ſeveral other gentlemen of rank. The profeſſor was aſſiſted ably, as on former occaſions, by Mr. Carpue, Mr. Cuthbertſon, and Mr. Hutchins.

Among other important facts, it was deciſively ſhown,

1. That a vital attraction ſubſiſts between a nerve and muſcle: for the ſuſpended ſciatic nerves of a frog, after detaching

taching the ſpine, being brought near the intercoſtal muſcles of a dog, while the aſſiſtant who held the frog did, with his other hand, touch the muſcles of the thigh of the dog, (thus forming a circle); in this ſituation, the nerves ſuſpended approached, and came into contact with the muſcle, as evidently as a ſilken thread is attracted by ſealing-wax.

2. The heart of a rabbit was excited to action in a little time after the animal was killed; but vitality diſappeared much ſooner than in the other muſcles: ſo that this organ is the *primum*, and not, as Harvey aſſerted, the *ultimum moriens*. The lungs, liver, and ſpleen could not be excited to action, even immediately after the animal was killed.

3. The moſt important fact of all was that of exciting contractions by making a circle of nerves and muſcles of different animals, without any metallic exciter or conductors.

4. The head of an ox, recently decapitated, exhibited aſtoniſhing effects: for, the tongue being drawn out by a hook fixed into it, on applying the exciters, in ſpite of the ſtrength of the aſſiſtant the tongue was retracted, ſo as to detach itſelf, by tearing itſelf, from the hook: at the ſame time a loud noiſe iſſued from the mouth by the abſorption of air, attended by violent contortions of the whole head and eyes.

After the exhibition of theſe phænomena of Galvaniſm, which, among other philoſophers, particularly occupy the attention of the Firſt Conſul, the company partook of an elegant *dejeune* and a *converſazione* in Dr. Pearſon's houſe.

XVII. *Deſcription of Mr.* PEPYS'S *large Galvanic Apparatus.*

MR. PEPYS jun. has lately conſtructed the moſt powerful Galvanic apparatus that has, we believe, been yet produced, of which we ſhall endeavour to give our readers ſome idea. It conſiſts of 60 pairs of zinc and copper plates diſpoſed in two troughs conſtructed on Mr. Cruickſhank's plan, but with ſome accompanying arrangements which are extremely convenient and uſeful. That our deſcription may be the better underſtood, we refer to a view of the apparatus, Plate I.

A A. The two troughs, each conſiſting of 30 pairs of plates. The plates are cemented into troughs made of mahogany, properly varniſhed to ſtand the action of the fluids which may be introduced into the cells between the plates. The plates are each 6 inches ſquare, or contain 36 ſquare inches of ſurface on each ſide; and that they may be able to conti-

nue in uſe for a conſiderable time even with the application of ſtrong acidulous liquors, they are made of ſuch a thickneſs that each pair weighs four pounds. The troughs are furniſhed with pivots at each end, by which they reſt on the bar B of the table into which the whole apparatus is framed: on theſe pivots, which are ſo placed as to throw the centre of gravity of the troughs below them, the troughs revolve when it is neceſſary to empty the cells.

C. A japanned iron tray or receiver, capable of holding the contents of the cells of the troughs when they are emptied.

D. A range of ſix funnels made of tin, the diſtances of the tubes of which are ſo adjuſted that they enter ſix of the cells of the troughs at once.

E. A tin veſſel having ſix diſtinct cells with ſpouts, each capable of containing the exact quantity of fluid neceſſary to fill a cell of the trough.

By means of the funnel D and veſſel E, the troughs are filled with very little trouble, no more being neceſſary than to fill the veſſel E by dipping it into the fluid or acid, and then emptying it, by the ſpouts correſponding with the diviſions in the funnel D, into the latter, the pipes of which are placed in the correſponding cells of the troughs.

FF the two prime conductors, being two braſs rods with ſhoulders. They paſs through holes in the top of the table (which, to ſhow the arrangement of the troughs, appears elevated in the engraving) into the end cell of each trough.

G G two metallic conductors, each conſiſting of two parts joined by ſockets, and each having a ſocket which fits upon the prime conductors F F. By this contrivance they are movable in any direction required in the experiments.

H H two ſhallow veſſels made of tin, with tubes underneath, by means of which they can be placed on the tops of the conductors. When filled with water they are found convenient for giving the Galvanic ſhock in a very perfect manner, and for other experiments.

I, an arch of metal to unite the troughs at their further ends.

When the cells are filled, the troughs connected by the metallic arch, the top or lid ſhut down, and the prime conductors put in their places, the apparatus forms a Galvanic table free from any incumbrance, and extremely well adapted for every experiment in Galvaniſm.

On the 21ſt of February, with a number of ſcientific gentlemen, we witneſſed a trial of this apparatus. The experiments

[illegible] metals were [illegible]

[illegible] mixed [illegible] With this [illegible]

[illegible] were deflagrated [illegible] ones [illegible] brush: a [illegible]

[illegible] deflagrated at the [illegible] hot for near [illegible]

[illegible] red hot, [illegible] ſparks along [illegible]

[illegible], with ſmoke and [illegible]

[illegible], with ſmoke [illegible]

[illegible] green light: no [illegible]

[illegible] light and ſmoke.

[illegible] diameter, fuſed, burnt, [illegible]

[illegible] in diameter, became [illegible] at the contact.

[illegible] ſubſtances, are [illegible] conductors armed with charcoal.

[illegible] deflagrating charcoal, [illegible] perſons with wetted hands [illegible]

[illegible] experiments is by in[illegible] with clean mercury, [illegible] The other conductor may be [illegible] to be tried. The [illegible] to it by wetting. [illegible] and will alſo ſerve to bind [illegible]

[illegible] was ſo great as to keep [illegible] without intermiſſion.

XVIII. *An Eſſay on the Method of determining the Difference of Longitude between Places at Land, from the obſerved Tranſits of the Moon over their Meridians; with a Demonſtration and Example.* By Mr. Gavin Lowe *.

> Ea eſt methodorum ſimpliciſſimarum ratio atque natura, ut poſtremæ in mentem veniant, et niſi obſtinatiore animo, ne veniant quidem.
>
> Boscovich.

ALTHOUGH the method of determining the difference of longitude at ſea from the lunar obſervations has been accurately laid down by Dr. Maſkelyne and other able nautical aſtronomers, it has, however, happened that ſeveral writers on longitude and aſtronomy have, in the courſe of the laſt twenty years, given rules for finding the difference of longitude at land from the moon's tranſits, either ſo erroneous or imperfect, that the adoption thereof might do a ſerious injury both to navigation and geography: they have given examples, but no demonſtrations; or at leaſt ſuch obſcure and imperfect ones, as prove that they had not a clear conception of the matter.

It is for theſe reaſons that the following demonſtration of a rule both eaſy and accurate for finding the difference of longitude, is now propoſed. The data are the obſerved increaſe of the moon's right aſcenſion in paſſing from the firſt to the ſecond meridian, and the increaſe of the ſun's and moon's right aſcenſion in twelve hours apparent time, which may be had from the Nautical Almanac.

DEMONSTRATION.

Let the circle A B C repreſent the equator, P its pole, and A P D the firſt meridian, as that of Greenwich. Suppoſe that the centres of the ſun, the moon, and a fixed ſtar are on that meridian at the ſame moment of time as repreſented at A, and that they move from thence to the weſtward with their reſpective velocities, the earth being conſidered as at reſt. Then, after twelve hours apparent time, the ſun will be at D, the oppoſite point to A, or 180° diſtant from it; but the fixed ſtar, moving in appearance over a greater ſpace than 180° in twelve hours apparent time, will be at E; while the moon, with a motion apparently ſlower than the ſun and the ſtar, will appear after twelve hours at the poi[illegible] or on a meridian B P. But E D is the diſtance of [illegible] from the ſtar after an interval of twelve hours apparent time, and EB the diſtance of the moon, or, in other words, the increaſe of their reſpective right aſcenſions: and ſince ED and EB are known from the Nautical Almanac, if we ſubtract the firſt from the laſt, we have DB, equal to the difference between the increaſe of the ſun's and moon's right aſcenſion in twelve hours apparent time. Now the difference of longitude between the two meridians AP and BP is the arc $A\beta B$, equal to $A\beta D$ leſs the arc DB; that is, equal to 180° leſs the difference between the increaſe of the ſun's and moon's right aſcenſion in twelve hours: and, ſince the increaſe of the moon's right aſcenſion from the time of its paſſing the meridian AP to the time of its paſſing BP is known from obſervation, and equal to EB, we can make the following proportion for finding the difference of longitude between any other two meridians, AP and βP, from the obſerved increaſe of the moon's right aſcenſion $\epsilon\beta$.

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* Communicated by the [illegible]

As $EB : A\beta D - DB :: \epsilon\beta : A\beta$ the difference of longitude; or, in more familiar language, as the increaſe of the moon's right aſcenſion in twelve hours apparent time is to 180° or 12^h leſs the difference between the increaſe of the ſun's and moon's right aſcenſion in that time ::, ſo is any other obſerved increaſe of the moon's right aſcenſion between two meridians : to their difference of longitude. Q. E. D.*

If the increaſe of the moon's right aſcenſion in twelve hours were uniform, or ſuch that equal parts of it would be produced in equal times, the above rule would be ſtrictly accurate; but as that increaſe ariſes from a motion continually accelerated or retarded, and ſeldom uniform but for a ſhort ſpace of time, it will therefore be neceſſary to find the mean increaſe of the moon's right aſcenſion when it is at the intermediate point between A and β, in order to determine their difference of longitude with the greateſt preciſion; and for that purpoſe, Taylor's Tables of Second Difference are very uſeful.

EXAMPLE.

April the 8th, 1800, the tranſit of the moon's firſt limb was obſerved at the royal obſervatory (A); and, allowance being made for the error of the clock, its right aſcenſion

was - - - -	12^h	35^m	$18{\cdot}22^s$
Add the time that the moon's ſemi-diameter took to paſs the meridian -	0	1	8·38
Right aſcenſion of the moon's centre	12	36	26·6

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* Demonſtrated for the firſt time by the author in 1770.

Brought over	12^h	36^m	$26{\cdot}6^s$
On a meridian (β[*]) far to the weſtward the tranſit of the moon's firſt limb was obſerved the ſame day, and being reduced to the centre, its right aſcenſion was -	12	47	56·7
Increaſe of right aſcenſion between A and β - - - - -	0	11	30·1
The increaſe of the moon's right aſcenſion in twelve hours apparent time per the Nautical Almanac was - - -	0	26	3[†]
The increaſe of the ſun's in the ſame time	0	1	49·65
Difference - - -	0	24	13·35

And 12 hours minus this difference is = $11^h\ 35^m\ 46{\cdot}65^s$; therefore, As $26^m\ 3^s : 11^h\ 35^m\ 46{\cdot}65^s :: 11^m\ 30{\cdot}1^s$ to $5^h\ 7^m\ 12^s$, the correct difference of longitude between A and β.

By reducing the three terms to ſeconds, and uſing logarithms, the operation is much ſhortened.

In a book publiſhed by Mr. Mackay on longitude about fifteen or ſixteen years ago, there is a rule given, and alſo an example, for finding the difference of longitude at land from the tranſits of the moon, but no demonſtration. The rule, when diveſted of its high-ſounding enunciation, runs thus:

As the increaſe of the moon's right aſcenſion in twelve hours apparent time : is to 180° ::, ſo is any other obſerved increaſe between two meridians : to their difference of longitude. It follows from this, that the moon as well as the ſun would, in twelve hours apparent time, paſs over an arc of 180°, although the apparent motion of the moon to the weſtward in twelve hours, or 180° of ſpace, be leſs than that of the ſun by ſix or ſeven degrees; and ſo much error would this method produce, if the two places differed about 180° in longitude.

The above example, wrought according to Mackay's rule, would come out thus:

As $26^m\ 3^s : 12^h :: 11^m\ 30{\cdot}1^s$ to -	5^h	17^m	$53{\cdot}7^s$
But the correct difference as above is -	5	7	12
Error - -	0	10	41·7

which amounts to more than 2½°, or 150 miles, in a difference of longitude little exceeding five hours.

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[*] A place near Port-Royal, in Jamaica.

[†] By interpolation $26^m\ 3{\cdot}67^s$; but the fraction of the ſecond was not minded.

Mr. Edward Pigott adopts the very ſame rule for determining the difference of longitude between Greenwich and York, and ſtates the reſult in the Philoſophical Tranſactions for 1786, p. 417.

Mr. Vince has inſerted this rule and example in his Treatiſe of Practical Aſtronomy; but we have to regret that they were not accompanied with a ſtrict demonſtration.

The Rev. Mr. Wollaſton, in the appendix to his Faſciculus Aſtronomicus, publiſhed two or three years ago, has given a rule, without demonſtration or example, for finding the difference of longitude from the moon's tranſits, which produces the ſame error as Mackay's and Pigott's, although worded differently from theirs. Mr. Wollaſton makes the firſt term of his proportion apparent, and the third mean time; this renders the reſult erroneous. Since the motion of the ſun, moon, and planets are computed for apparent time, and given ſo in the Nautical Almanac, mean time is not at all requiſite for reſolving the difference of longitude either at ſea or at land. We ſhall therefore endeavour to apply Mr. Wollaſton's rule, according to its literal meaning, for finding the difference of longitude from the above obſervations.

The right aſcenſion of the moon's centre on the meridian of Greenwich being known, we can eaſily deduce the mean and apparent time correſponding to it; and in like manner the mean and apparent time at the diſtant meridian β*. The apparent and mean time of the tranſits of the moon's centre over the meridians of A and β, when ſtrictly computed, were as follows:

	Apparent Time.	Mean Time.
At A - -	11^h 26^m $47·81^s$	11^h 28^m $33·5^s$
At β - -	11 37 29·5	11 39 11·4
Time later at β than at A	0 10 41·69	0 10 37·9
From the increaſe of the moon's right aſcenſion in twelve hours - -	26 3	
Subtract the increaſe of the ſun's right aſcenſion in that time - -	1 49·65	
The moon's retardation in twelve hours - -	24 13·35	

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* Mean time, however, at β, before its longitude be known, is not a fair poſtulatum. The method above demonſtrated does not require it.

G. L.

Then, "As twice the moon's retardation in twelve hours : is to twenty-four hours : :

"So is the mean time later at β than at A : to the difference of longitude weſt from A."

After doubling $24^m\ 13{\cdot}35^s$, and alſo 12, which is totally unneceſſary, as the reſult would be the ſame if they ſtood ſingle, we ſtate the following proportion:

As $48^m\ 26{\cdot}7^s : 24^h :: 10^m\ 37{\cdot}9^s$ to--$5^h\ 15^m\ 1{\cdot}3^s$, the difference of longitude between A and β.

But as the third term is improperly reduced to mean time, we ſhall take the apparent time above found, and then $48^m\ 26{\cdot}7^s : 24^h :: 10^m\ 41{\cdot}69^s$ to--$5^h\ 17^m\ 53{\cdot}7^s$; the ſame as reſults from Mackay's and Pigott's rules.

We ſhall only remark, that $5^h\ 17^m\ 53{\cdot}7^s$ is the apparent time that the moon took in paſſing from the meridian of A to the meridian of β; but from what has been demonſtrated, the apparent time at β will be equal to the difference between the increaſe of the ſun's and moon's right aſcenſion in that interval of apparent time; for DB, or $24^m\ 13{\cdot}35^s$ is the difference for twelve hours, and therefore by proportion $\delta\beta$, or $10^m\ 41{\cdot}69^s$ will be the difference for $5^h\ 17^m\ 53{\cdot}7^s$; ſubtracting the former from the latter, we have $5^h\ 7^m\ 12^s$, the difference of longitude as before, and a clear proof that the authors above mentioned have omitted to deduct the apparent time at the diſtant place or ſtation β, from the apparent time at Greenwich.

If it ſhould be thought eaſier to employ ſidereal time in reſolving the difference of longitude between A and β, let ED, the increaſe of the ſun's right aſcenſion in twelve hours, be added to twelve hours, and we have then the arc of ſidereal time ABDE; from which ſubtracting EB, there remains AβB, equal to the difference of longitude. By proportion we can therefore ſay, As EB, the increaſe of the moon's right aſcenſion in twelve hours; is to ABD + DE, or twelve hours + the increaſe of the ſun's right aſcenſion; ſo is any other obſerved increaſe of the moon's right aſcenſion as $\varepsilon\beta$; to the arc of ſidereal time A$\beta\varepsilon$; and if from this we ſubtract $\varepsilon\beta$, then Aβ is the difference of longitude required.

To apply this rule for finding the difference of longitude from the above obſervations and data, we ſay,

As $26^m\ 3^s : 12^h\ 1^m\ 49{\cdot}65^s :: 11^m\ 30{\cdot}1^s$ to A$\beta\varepsilon$ $5^h\ 18^m\ 42{\cdot}1^s$
From this ſubtract $\varepsilon\beta$, the obſerved increaſe 0 11 30·1

The difference of longitude between A and β is 5 7 12
Q. E. I.

duces a foamy effervefcence, gives out nitrous gas, and diſſolves. This ſolution is decompoſed by water, like that of animal fibrous matter, and the white precipitate thence reſulting diſſolves in ammonia, aſſuming a dark yellow colour.

3. A diſcovery of two new principles in ſulphureous waters has lately been made by Counſellor Weſtrumb, of Hammeln, who gives the following details:

"For ſix years I have been excluſively employed in the analyſis of ſulphureous waters. I have found an eaſy and entirely new method of determining what they contain of ſulphurated hydrogen and carbonic acid gas; and I have diſcovered a principle hitherto unknown in theſe waters, namely, bitumen in combination with ſulphurated hydrogen gas and hydro-ſulphurated lime. I communicated the detail of this diſcovery to M. Wurzer. Counſellor Von Crell and Profeſſor Schaub of Caſſel ſaw at my houſe theſe new products. Theſe principles are found in the waters of Neuwdorf, Limmen, Rehburg, and Eylſe. In the laſt place, five ſulphureous ſprings, altogether different from each other, a ſpring of carbonic gaſeous water, and one of freſh water, are found in an extent of leſs than a thouſand ſquare feet. Near Buckembourg, the capital of the ſmall country of Leppe, to which Eylſe belongs, are found two other ſulphureous ſprings perfectly ſimilar, one of which we ſounded to the depth of eighty feet without finding its reſervoir. Baths of mud ſimilar to thoſe of St. Amand have been conſtructed at Eylſe: a very conſiderable quantity of hydro-ſulphurated mud, which may furniſh baths for ſeveral centuries, has been found there."

4. A new gas, ſulphurated azotic gas, has been diſcovered in the mineral waters of Aix-la-Chapelle by M. C. Gimbernat, ſub director of the muſeum of natural hiſtory at Madrid, who gives the following account of it:

"I have juſt finiſhed my labour on the analyſis of the ſulphureous waters of Aix-la-Chapelle. I have found that the excipient of the ſulphur in theſe waters is not hydrogen, as has hitherto been ſuppoſed and aſſerted, but azote. Theſe waters contain no ſulphurated hydrogen.

"Sulphurated azotic gas is a chemical body, the exiſtence of which has never been ſuſpected. Nature preſents it in abundance in the mineral waters of this country.

"Beſides ſulphurated azote, theſe waters contain a great deal of pure and free azote interpoſed, and in the ſemigaſeous ſtate."

5. We are informed by Mr. Paul of Geneva (now in London) that water holding ſulphate of lime in ſolution, and impregnated under preſſure with hydrogen gas, of which it can take up about one-fourth of its volume, after ſtanding ſtopped up for about three months, on being opened was found to contain ſulphurized hydrogen gas. This has been repeated ſeveral times with the ſame reſult. In diſtilled water not impregnated with ſulphate of lime, the hydrogen gas undergoes no change.

How is the ſulphurized hydrogen gas produced? There is plainly a decompoſition effected, not merely of the ſulphate of lime, but of the ſulphuric acid, which parts with a portion of its ſulphur.—What becomes of the oxygen? Does it join the hydrogen, and form water?

The hydrogen gas made uſe of in the above experiments was procured from the decompoſition of water, by applying it to iron expoſed to a high temperature. When obtained by means of ſulphuric acid, water, and iron, in the humid way, it has a different flavour, ſomewhat hepatic.

6. Mr. Paul has mentioned to us another circumſtance, which deſerves to be made known. Water impregnated with hydrogen gas is extremely injurious in dropſical complaints. A gentleman labouring under the dropſy was cured by drinking water impregnated with oxygen gas; but during the cure his ſervant by miſtake had purchaſed ſome hydrogen water. In leſs than 24 hours his malady returned: on finding out the cauſe, and having recourſe to the oxygenated water, the miſchief was ſpeedily remedied.

ANIMAL GALVANISM.

Some curious Galvanic experiments were made on February 18, by profeſſor Aldini, in Dr. Pearſon's lecture-rooms. They were by far more intereſting and ſatisfactory than any we have yet noticed on animals, owing to the pains taken to procure the fitteſt ſubjects for the operations. They were inſtituted in the preſence of his excellency the ambaſſador of France general Andreoſſi, lord Pelham, lord Roxburgh, lord Caſtlereagh, lord Hervey, the honourable Mr. Upton, Mr. Cholmondely, Mr. Anchora, Mr. Elliot, and ſeveral other gentlemen of rank. The profeſſor was aſſiſted ably, as on former occaſions, by Mr. Carpue, Mr. Cuthbertſon, and Mr. Hutchins.

Among other important facts, it was deciſively ſhown,

1. That a vital attraction ſubſiſts between a nerve and muſcle: for the ſuſpended ſciatic nerves of a frog, after detaching

taching the ſpine, being brought near the intercoſtal muſcles of a dog, while the aſſiſtant who held the frog did, with his other hand, touch the muſcles of the thigh of the dog, (thus forming a circle); in this ſituation, the nerves ſuſpended approached, and came into contact with the muſcle, as evidently as a ſilken thread is attracted by ſealing-wax.

2. The heart of a rabbit was excited to action in a little time after the animal was killed; but vitality diſappeared much ſooner than in the other muſcles: ſo that this organ is the *primum*, and not, as Harvey aſſerted, the *ultimum moriens*. The lungs, liver, and ſpleen could not be excited to action, even immediately after the animal was killed.

3. The moſt important fact of all was that of exciting contractions by making a circle of nerves and muſcles of different animals, without any metallic exciter or conductors.

4. The head of an ox, recently decapitated, exhibited aſtoniſhing effects: for, the tongue being drawn out by a hook fixed into it, on applying the exciters, in ſpite of the ſtrength of the aſſiſtant the tongue was retracted, ſo as to detach itſelf, by tearing itſelf, from the hook: at the ſame time a loud noiſe iſſued from the mouth by the abſorption of air, attended by violent contortions of the whole head and eyes.

After the exhibition of theſe phænomena of Galvaniſm, which, among other philoſophers, particularly occupy the attention of the Firſt Conſul, the company partook of an elegant *dejeune* and a *converſazione* in Dr. Pearſon's houſe.

XVII. *Deſcription of Mr.* PEPYS's *large Galvanic Apparatus.*

MR. PEPYS jun. has lately conſtructed the moſt powerful Galvanic apparatus that has, we believe, been yet produced, of which we ſhall endeavour to give our readers ſome idea. It conſiſts of 60 pairs of zinc and copper plates diſpoſed in two troughs conſtructed on Mr. Cruickſhank's plan, but with ſome accompanying arrangements which are extremely convenient and uſeful. That our deſcription may be the better underſtood, we refer to a view of the apparatus, Plate I.

A A. The two troughs, each conſiſting of 30 pairs of plates. The plates are cemented into troughs made of mahogany, properly varniſhed to ſtand the action of the fluids which may be introduced into the cells between the plates. The plates are each 6 inches ſquare, or contain 36 ſquare inches of ſurface on each ſide; and that they may be able to conti-

nue in uſe for a conſiderable time even with the application of ſtrong acidulous liquors, they are made of ſuch a thickneſs that each pair weighs four pounds. The troughs are furniſhed with pivots at each end, by which they reſt on the bar B of the table into which the whole apparatus is framed: on theſe pivots, which are ſo placed as to throw the centre of gravity of the troughs below them, the troughs revolve when it is neceſſary to empty the cells.

C. A japanned iron tray or receiver, capable of holding the contents of the cells of the troughs when they are emptied.

D. A range of ſix funnels made of tin, the diſtances of the tubes of which are ſo adjuſted that they enter ſix of the cells of the troughs at once.

E. A tin veſſel having ſix diſtinct cells with ſpouts, each capable of containing the exact quantity of fluid neceſſary to fill a cell of the trough.

By means of the funnel D and veſſel E, the troughs are filled with very little trouble, no more being neceſſary than to fill the veſſel E by dipping it into the fluid or acid, and then emptying it, by the ſpouts correſponding with the diviſions in the funnel D, into the latter, the pipes of which are placed in the correſponding cells of the troughs.

FF the two prime conductors, being two braſs rods with ſhoulders. They paſs through holes in the top of the table (which, to ſhow the arrangement of the troughs, appears elevated in the engraving) into the end cell of each trough.

G G two metallic conductors, each conſiſting of two parts joined by ſockets, and each having a ſocket which fits upon the prime conductors F F. By this contrivance they are movable in any direction required in the experiments.

H H two ſhallow veſſels made of tin, with tubes underneath, by means of which they can be placed on the tops of the conductors. When filled with water they are found convenient for giving the Galvanic ſhock in a very perfect manner, and for other experiments.

I, an arch of metal to unite the troughs at their further ends.

When the cells are filled, the troughs connected by the metallic arch, the top or lid ſhut down, and the prime conductors put in their places, the apparatus forms a Galvanic table free from any incumbrance, and extremely well adapted for every experiment in Galvaniſm.

On the 21ſt of February, with a number of ſcientific gentlemen, we witneſſed a trial of this apparatus. The experiments

ments made by Mr. Pepys on the deflagration of metals were the moſt brilliant and ſplendid we ever beheld.

The troughs were filled with 32 pounds of water mixed with two pounds of concentrated nitrous acid. With this charge—

Iron wires of $\frac{1}{200}$ to $\frac{1}{10}$ of an inch in diameter were deflagrated with great ſplendour. A number of the ſmall ones twiſted together produced ſomewhat like a little bruſh: a pleaſing appearance in deflagration.

Charcoal of box wood was not only deflagrated at the place of contact, but remained permanently red hot for near two inches in length.

Lead foil burnt with great vividneſs, becoming red hot, and emitting a ſmall volcano or adjutage of red ſparks along with the flame.

Tin foil burnt with great ſplendour, with ſmoke and ſparks.

Dutch leaf or braſs foil deflagrated vividly, with ſmoke and a profuſion of ſparks.

Silver leaf burnt with an intenſe vivid green light: no ſparks, but much ſmoke or fume.

Gold leaf deflagrated with bright white light and ſmoke.

Tin wire, 1-eighth of an inch in diameter, fuſed, burnt, and oxidated, with great ſplendour.

Platina wire, 1-ſixteenth of an inch in diameter, became red hot, white, and fuſed into globules at the contact.

Gunpowder, phoſphorus, and inflammable ſubſtances, are inſtantly fired by contact with conductors armed with charcoal.

The Galvanic power was capable of deflagrating charcoal, after paſſing through ſixteen perſons with wetted hands joined.

The beſt method of ſhowing theſe experiments is by introducing into an earthen plate filled with clean mercury, one of the prime conductors. The other conductor may be then mounted with the ſeveral ſubſtances to be tried. The leaves and laminæ of metal eaſily adhere to it by wetting. Wires may be twiſted round it, and will alſo ſerve to bind other ſubſtances to it.

The power of this large trough was ſo great as to keep up the deflagrations and combuſtion without intermiſſion.

XVIII. *An Eſſay on the Method of determining the Difference of Longitude between Places at Land, from the obſerved Tranſits of the Moon over their Meridians; with a Demonſtration and Example.* By Mr. GAVIN LOWE[*].

Ea eſt methodorum ſimpliciſſimarum ratio atque natura, ut poſtremæ in mentem veniant, et niſi obſtinatiore animo, ne veniant quidem.

BOSCOVICH.

ALTHOUGH the method of determining the difference of longitude at ſea from the lunar obſervations has been accurately laid down by Dr. Maſkelyne and other able nautical aſtronomers, it has, however, happened that ſeveral writers on longitude and aſtronomy have, in the courſe of the laſt twenty years, given rules for finding the difference of longitude at land from the moon's tranſits, either ſo erroneous or imperfect, that the adoption thereof might do a ſerious injury both to navigation and geography: they have given examples, but no demonſtrations; or at leaſt ſuch obſcure and imperfect ones, as prove that they had not a clear conception of the matter.

It is for theſe reaſons that the following demonſtration of a rule both eaſy and accurate for finding the difference of longitude, is now propoſed. The data are the obſerved increaſe of the moon's right aſcenſion in paſſing from the firſt to the ſecond meridian, and the increaſe of the ſun's and moon's right aſcenſion in twelve hours apparent time, which may be had from the Nautical Almanac.

DEMONSTRATION.

Let the circle A B C repreſent the equator, P its pole, and A P D the firſt meridian, as that of Greenwich. Suppoſe that the centres of the ſun, the moon, and a fixed ſtar are on that meridian at the ſame moment of time as repreſented at A, and that they move from thence to the weſtward with their reſpective velocities, the earth being conſidered as at reſt. Then, after twelve hours apparent time, the ſun will be at D, the oppoſite point to A, or 180° diſtant from it; but the fixed ſtar, moving in appearance over a greater ſpace than 180° in twelve hours apparent time, will be at E; while the moon, with a motion apparently ſlower than the ſun and the ſtar, will appear after twelve hours at the point B, or on a meridian B P. But E D is the diſtance of the ſun from the ſtar after an interval of twelve hours apparent time, and E B the diſtance of the moon, or, in other words, the increaſe of their reſpective right aſcenſions: and ſince E D and E B are known from the Nautical Almanac, if we ſubtract the firſt from the laſt, we have D B, equal to the difference between the increaſe of the ſun's and moon's right aſcenſion in twelve hours apparent time. Now the difference of longitude between the two meridians A P and B P is the arc $A\beta B$, equal to $A\beta D$ leſs the arc D B; that is, equal to 180° leſs the difference between the increaſe of the ſun's and moon's right aſcenſion in twelve hours: and, ſince the increaſe of the moon's right aſcenſion from the time of its paſſing the meridian A P to the time of its paſſing B P is known from obſervation, and equal to E B, we can make the following proportion for finding the difference of longitude between any other two meridians, A P and β P, from the obſerved increaſe of the moon's right aſcenſion $\varepsilon\beta$.

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[*] Communicated by the Author.

As $EB : A\beta D - DB :: \varepsilon\beta : A\beta$ the difference of longitude; or, in more familiar language, as the increaſe of the moon's right aſcenſion in twelve hours apparent time is to 180° or 12^h leſs the difference between the increaſe of the ſun's and moon's right aſcenſion in that time : :, ſo is any other obſerved increaſe of the moon's right aſcenſion between two meridians : to their difference of longitude. Q. E. D.*

If the increaſe of the moon's right aſcenſion in twelve hours were uniform, or ſuch that equal parts of it would be produced in equal times, the above rule would be ſtrictly accurate; but as that increaſe ariſes from a motion continually accelerated or retarded, and ſeldom uniform but for a ſhort ſpace of time, it will therefore be neceſſary to find the mean increaſe of the moon's right aſcenſion when it is at the intermediate point between A and β, in order to determine their difference of longitude with the greateſt preciſion; and for that purpoſe, Taylor's Tables of Second Difference are very uſeful.

EXAMPLE.

April the 8th, 1800, the tranſit of the moon's firſt limb was obſerved at the royal obſervatory (A); and, allowance being made for the error of the clock, its right aſcenſion

was - - - - -	12^h	35^m	$18{\cdot}22^s$
Add the time that the moon's ſemi-diameter took to paſs the meridian -	0	1	8·38
Right aſcenſion of the moon's centre	12	36	26·6

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* Demonſtrated for the firſt time by the author in 1770.

On

Brought over	12^h	36^m	$26{\cdot}6^s$
On a meridian (β[*]) far to the weſtward the tranſit of the moon's firſt limb was obſerved the ſame day, and being reduced to the centre, its right aſcenſion was -	12	47	56·7
Increaſe of right aſcenſion between A and β - - - - -	0	11	30·1
The increaſe of the moon's right aſcenſion in twelve hours apparent time per the Nautical Almanac was - - - -	0	26	3[†]
The increaſe of the ſun's in the ſame time	0	1	49·65
Difference - - - -	0	24	13·35

And 12 hours minus this difference is = $11^h\ 35^m\ 46{\cdot}65^s$; therefore, As $26^m\ 3^s : 11^h\ 35^m\ 46{\cdot}65^s :: 11^m\ 30{\cdot}1^s$ to $5^h\ 7^m\ 12^s$, the correct difference of longitude between A and β.

By reducing the three terms to ſeconds, and uſing logarithms, the operation is much ſhortened.

In a book publiſhed by Mr. Mackay on longitude about fifteen or ſixteen years ago, there is a rule given, and alſo an example, for finding the difference of longitude at land from the tranſits of the moon, but no demonſtration. The rule, when diveſted of its high-ſounding enunciation, runs thus:

As the increaſe of the moon's right aſcenſion in twelve hours apparent time : is to 180° ::, ſo is any other obſerved increaſe between two meridians : to their difference of longitude. It follows from this, that the moon as well as the ſun would, in twelve hours apparent time, paſs over an arc of 180°, although the apparent motion of the moon to the weſtward in twelve hours, or 180° of ſpace, be leſs than that of the ſun by ſix or ſeven degrees; and ſo much error would this method produce, if the two places differed about 180° in longitude.

The above example, wrought according to Mackay's rule, would come out thus:

As $26^m\ 3^s : 12^h :: 11^m\ 30{\cdot}1^s$ to -	5^h	17^m	$53{\cdot}7^s$
But the correct difference as above is -	5	7	12
Error - -	0	10	41·7

which amounts to more than 2½°, or 150 miles, in a difference of longitude little exceeding five hours.

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[*] A place near Port-Royal, in Jamaica.

[†] By interpolation $26^m\ 3{\cdot}67^s$; but the fraction of the ſecond was not minded.

Mr. Edward Pigott adopts the very ſame rule for determining the difference of longitude between Greenwich and York, and ſtates the reſult in the Philoſophical Tranſactions for 1786, p. 417.

Mr. Vince has inſerted this rule and example in his Treatiſe of Practical Aſtronomy; but we have to regret that they were not accompanied with a ſtrict demonſtration.

The Rev. Mr. Wollaſton, in the appendix to his Faſciculus Aſtronomicus, publiſhed two or three years ago, has given a rule, without demonſtration or example, for finding the difference of longitude from the moon's tranſits, which produces the ſame error as Mackay's and Pigott's, although worded differently from theirs. Mr. Wollaſton makes the firſt term of his proportion apparent, and the third mean time; this renders the reſult erroneous. Since the motion of the ſun, moon, and planets are computed for apparent time, and given ſo in the Nautical Almanac, mean time is not at all requiſite for reſolving the difference of longitude either at ſea or at land. We ſhall therefore endeavour to apply Mr. Wollaſton's rule, according to its literal meaning, for finding the difference of longitude from the above obſervations.

The right aſcenſion of the moon's centre on the meridian of Greenwich being known, we can eaſily deduce the mean and apparent time correſponding to it; and in like manner the mean and apparent time at the diſtant meridian β[*]. The apparent and mean time of the tranſits of the moon's centre over the meridians of A and β, when ſtrictly computed, were as follows:

	Apparent Time.			Mean Time.		
At A - -	11h	26m	47·81s	11h	28m	33·5s
At β - -	11	37	29·5	11	39	11·4
Time later at β than at A	0	10	41·69	0	10	37·9

From the increaſe of the moon's right aſcenſion in twelve hours - -	26	3
Subtract the increaſe of the ſun's right aſcenſion in that time - -	1	49·65
The moon's retardation in twelve hours - -	24	13·35

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[*] Mean time, however, at β, before its longitude be known, is not a fair poſtulatum. The method above demonſtrated does not require it.

G. L.

Then, "As twice the moon's retardation in twelve hours : is to twenty-four hours : :

"So is the mean time later at β than at A : to the difference of longitude weſt from A."

After doubling $24^{m}\ 13{\cdot}35^{s}$, and alſo 12, which is totally unneceſſary, as the reſult would be the ſame if they ſtood ſingle, we ſtate the following proportion :

As $48^{m}\ 26{\cdot}7^{s} : 24^{h} : : 10^{m}\ 37{\cdot}9^{s}$ to--$5^{h}\ 15^{m}\ 1{\cdot}3^{s}$, the difference of longitude between A and β.

But as the third term is improperly reduced to mean time, we ſhall take the apparent time above found, and then $48^{m}\ 26{\cdot}7^{s} : 24^{h} : : 10^{m}\ 41{\cdot}69^{s}$ to--$5^{h}\ 17^{m}\ 53{\cdot}7^{s}$; the ſame as reſults from Mackay's and Pigott's rules.

We ſhall only remark, that $5^{h}\ 17^{m}\ 53{\cdot}7^{s}$ is the apparent time that the moon took in paſſing from the meridian of A to the meridian of β; but from what has been demonſtrated, the apparent time at β will be equal to the difference between the increaſe of the ſun's and moon's right aſcenſion in that interval of apparent time; for DB, or $24^{m}\ 13{\cdot}35^{s}$ is the difference for twelve hours, and therefore by proportion δβ, or $10^{m}\ 41{\cdot}69^{s}$ will be the difference for $5^{h}\ 17^{m}\ 53{\cdot}7^{s}$; ſubtracting the former from the latter, we have $5^{h}\ 7^{m}\ 12^{s}$, the difference of longitude as before, and a clear proof that the authors above mentioned have omitted to deduct the apparent time at the diſtant place or ſtation β, from the apparent time at Greenwich.

If it ſhould be thought eaſier to employ ſidereal time in reſolving the difference of longitude between A and β, let ED, the increaſe of the ſun's right aſcenſion in twelve hours, be added to twelve hours, and we have then the arc of ſidereal time ABDE; from which ſubtracting EB, there remains AβB, equal to the difference of longitude. By proportion we can therefore ſay, As EB, the increaſe of the moon's right aſcenſion in twelve hours; is to ABD + DE, or twelve hours + the increaſe of the ſun's right aſcenſion; ſo is any other obſerved increaſe of the moon's right aſcenſion as εβ; to the arc of ſidereal time Aβε; and if from this we ſubtract εβ, then Aβ is the difference of longitude required.

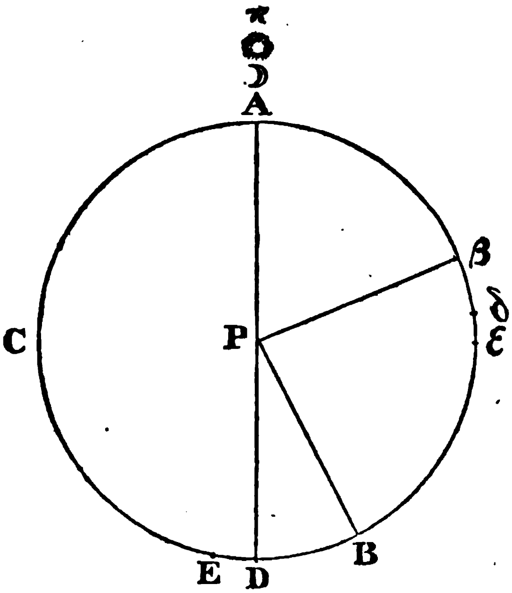
To apply this rule for finding the difference of longitude from the above obſervations and data, we ſay,

As $26^{m}\ 3^{s} : 12^{h}\ 1^{m}\ 49{\cdot}65^{s} : : 11^{m}\ 30{\cdot}1^{s}$ to Aβε $5^{h}\ 18^{m}\ 42{\cdot}1^{s}$
From this ſubtract εβ, the obſerved increaſe 0 11 30·1

The difference of longitude between A and β is 5 7 12
Q. E. I.

Although

Although we can ſafely truſt to the accuracy of the obſervations at the royal obſervatory, yet, at the diſtant ſtation β, great attention muſt be paid in adjuſting the tranſit inſtrument truly to the meridian, and taking care that it be perfect in collimation before the moon's tranſit be obſerved there; and alſo, that correct time be deduced as ſoon before or after the moon's tranſit as poſſible, from obſerving the meridian paſſage of one or more of the thirty-ſix ſtars, whoſe right aſcenſions, after a long ſeries of obſervations, have been aſcertained by the Aſtronomer Royal with the greateſt preciſion.



In order to put the above method to the teſt of experiment, a number of tranſits of the moon's firſt limb were obſerved many years ago at a place 26 ſeconds of time weſt from the royal obſervatory, with a ſmall portable tranſit inſtrument made by that eminent artiſt Mr. Edward Troughton, of Fleet-ſtreet. It had a teleſcope 20 inches long, a treble object-glaſs, and the price only 20 guineas. When the obſervations at Greenwich were publiſhed, all thoſe that were made on the ſame days at both places were employed to determine the difference of longitude, and the mean reſult of four or five correſponding obſervations gave it true to two ſeconds of time.

This is only mentioned to ſhow, that with ſmall tranſit inſtruments well conſtructed, and due attention paid to their adjuſtments, obſervations may be made ſufficiently accurate for anſwering the ends of practical aſtronomy.

We expect ſoon to have the lunar tables brought to a greater degree of perfection, by applying certain equations that have been lately diſcovered, and by which thoſe purpoſes will, of courſe, be more ſucceſsfully promoted. It is alſo much to be wiſhed that the moon's tranſits were as aſſiduouſly obſerved by public and private aſtronomers in every country as they are at the royal obſervatory in this, and from time to time communicated to the public at large.

It ſometimes, but not very frequently, happens, that favourable weather will permit one aſtronomer to obſerve four tranſits of the moon over his meridian in the ſame number of ſucceeding days. If another, or indeed a hundred others, ſhould obſerve only one or two on any of thoſe days, whether at the neareſt or remoteſt ſtations from the firſt meridian; ſuch obſervations are of great value, and afford the very beſt means of finding the difference of longitude with accuracy, becauſe the errors of the lunar tables are thereby completely obviated.

Paradiſe Row, Iſlington,
February 14, 1803.

XIX. *On the Preparation of the Hyacinthus non ſcriptus, or common Blue-Bell, as a Subſtitute for Gum-Arabic.* By Mr. THOMAS WILLIS, *of Lime-Street, London* [*].

IN the year 1794, whilſt collecting plants in a wood for botanical ſpecimens, I obſerved that the root of the *hyacinthus non ſcriptus*, the plant commonly called blue-bells, or hare-bells, was extremely mucilaginous; and, on taſting it, I diſcovered only a very ſlight pungency. I collected a pound of the bulbs, and, after ſlicing and drying them before a fire, they yielded about four ounces of powder. I thought that, by keeping the powder ſome time, the little acridneſs might go off, as it does in the arum-root powder. I taſted it about ſix months after, and found it perfectly inſipid. I concluded it might be rendered uſeful for food or nouriſhment, but at that time purſued the matter no further.

In the ſpring of 1800, gum-arabic having been a long time very dear, and likely to continue ſo, I thought this mucilaginous root might anſwer ſome of its purpoſes for external uſe. I therefore procured ſeven pounds and a half of the bulbs, which, when ſliced and dried, produced two pounds of powder. Being ſoon afterwards in company with Mr. Charles Taylor, ſecretary to the Society of Arts, &c. I mentioned to him that I had diſcovered a root which grew in great plenty in this kingdom, yielded a very ſtrong mucilage, and which I imagined would anſwer the purpoſes of gum-arabic in ſome of the manufactories. He ſaid, if I pleaſed, he would ſend ſome of it down to Mancheſter, to be tried by the calico-printers.

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[*] From the *Tranſactions of the Society for the Encouragement of Arts, &c.* vol. xx., who voted a ſilver medal to the author for his communication.

Three or four ounces of the powder were given him, and ſent down there: he was informed, upon trial, that it anſwered the purpoſes of fixing the calico-printers' colours equally as well as gum-arabic; and in the ſame proportion, of an ounce and a half of the powder to four ounces of the mordant. Mr. Taylor received the ſamples of the printed cottons on which it had been uſed.

On the 15th of January 1801 I furniſhed Mr. Taylor with eight ounces more of the powder; but have not ſince heard the reſult.

As this root can be eaſily procured, and uſed at a leſs price than gum-arabic has been ſold for ſeveral years paſt, I think it may be rendered of great utility; and the Society of Arts, &c., by patronizing it, may be the means of making it a public benefit.

Care ſhould be taken, and advice given, that the woods ſhould not be left deſtitute of the roots; and it would be adviſeable to offer premiums for cultivating the roots and offſets, as they are very increaſing. By ſuch means a conſtant ſupply may be had, if the roots anſwer the intended purpoſes.

I do not preſume to offer any thing reſpecting the mode in which the ſociety may think proper to divulge the diſcovery, and promote the uſe of theſe roots; but I imagine, that if the roots are bruiſed and uſed freſh, they would anſwer the purpoſe better than when dried and powdered; and as it is now a proper time of the year for taking them up, and will continue to be ſo for two months, I wiſh that the diſcovery may be made known as ſoon as poſſible.

I have ſent you ſpecimens both of the dried roots and powder, that they may be ſeen at the ſociety's rooms by the calico-printers. What I have done have been ſcorched a little in drying; but the colour would be much better, if proper care was taken in drying them. I am, &c.

Lime-ſtreet, THOMAS WILLIS.
March 17, 1802.

From the trials made before the committee with this powder with hot and cold water, from ſamples of the printed cotton

ton produced which had been printed therewith inſtead of gum-ſenegal, and from experiments made in Mancheſter, it appears that the *hyacinthus non ſcriptus* may, in many caſes, be found a uſeful ſubſtitute for gum-arabic.

XX. *On the Edulcoration of Fiſh-Oil.* *By* ROBERT DOSSIE, *Eſq.*[*]

Explanation of the Principles on which the Purification of Fiſh-Oil may be performed, and of the Uſes to which it is applicable.

THAT the fœtid ſmell of fiſh-oil is chiefly owing to putrefaction, it is unneceſſary to ſhow; but, though this be the principal cauſe, there is another likewiſe, which is, uſtion, or burning the oil, occaſioned by the ſtrong heat employed for the extracting it from the blubber of the larger fiſh, and which produces a ſtrong empyreumatic ſcent that is not always to be equally removed by the ſame means as the putrid ſmell, but remains ſometimes very prevalent after that is taken away.

In order to the perfect edulcoration of oils there are conſequently two kinds of fœtor or ſtink to be removed; viz. the putrid, and the empyreumatic: and the ſame means do not always equally avail againſt both.

The putrid ſmell of fiſh-oil is of two kinds: the rancid, which is peculiar to oils; and the common putrid ſmell, which is the general effect of the putrefaction of animal fluids, or of the vaſcular ſolids, when commixed with aqueous fluids.

Fiſh-oil has not only rancidity, or the firſt kind of putrid ſmells peculiar to oils, but alſo the ſecond or general kinds; as the oil, for the moſt part, is commixed with the gelatinous humour common to all animals, and ſome kinds with a proportion of the bile likewiſe; and thoſe humours putrefying combine their putrid ſcent with the rancidity of the oil, and, in caſes where great heat has been uſed, with that and the empyreuma alſo.

The reaſon of the preſence of the gelatinous fluid in fiſh-oil is this: that the blubber, which conſiſts partly of adipoſe veſicles, and partly of the membrana celluloſa, which contains the gelatinous fluid, is, for the moſt part, kept a conſiderable time before the oil is ſeparated from it, either from the want of convenient opportunities to extract the oil, or in order to the obtaining a larger proportion; as the putrid effervefcence which then comes on, rupturing the veſicles, makes the blubber yield a greater quantity of oil than could be extracted before ſuch change was produced; and the veſicles of the tela celluloſa, containing the gelatinous matter, being alſo burſt from the ſame cauſe, ſuch matter being then rendered ſaponaceous by the putrefaction, a part of it mixes intimately with the oil, and conſtitutes it a compound of the proper oleaginous parts and this heterogeneous fluid.

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[*] From the *Tranſactions of the Society for the Encouragement of Arts, &c.* vol. xx. So far back as the year 1761 the ſociety voted him a bounty of one hundred pounds for this communication, though they did not publiſh it till 1802.

The preſence of the bile in fiſh-oil is occaſioned by its being, in many caſes, extracted from the liver of the fiſh; which is not to be ſo profitably done by other means as by putrefaction; and the bile being conſequently diſcharged, together with the oil from the veſſels of the liver containing them, combines with it, both from the original ſaponaceous property of bile, and from that which it acquires by putrefaction.

This holds good particularly of the cod-oil, or common train, brought from Newfoundland, which, from its high yellow colour, viſcid conſiſtence, and repugnance to burning well in lamps, manifeſts ſenſibly the preſence of bile and the gelatinous fluid; which latter, by the ſaponaceous power of the bile, is commixed in a greater proportion in this than in any other kind of fiſh-oil.

A tendency to putrefy, or at moſt but in an extremely ſlow manner, is not an abſolute property of perfect oils in a ſimple or pure ſtate, but it is a relative property dependent upon their accidental contact or commixture with the aqueous fluid. This is evident from the caſe of oils concreted into a ſebaceous form; which being perfectly oleaginous and uncombined with any water, except ſuch as enters into their component parts, will not putrefy unleſs water, or ſomething containing it, is brought in contact with them. But the fluid animal and moſt vegetable oils being compounded of perfect oils with other mixed ſubſtances, either ſub-oleaginous or gelatinous, have always a putreſcence *per ſe*, or tendency to putrefy, without further admixture of aqueous moiſture. This commixture of heterogeneous matter in fiſh-oil, particularly of the gelatinous fluid and bile, gives riſe to a further principle of purification than *ſimple edulcoration*, or the removing the fœtor; for the preſence of ſuch humours in the oil renders it ſubject to a ſecond putreſcence *per ſe*, ſuppoſing the firſt corrected; makes it unfit for the purpoſe of the woollen manufacture, as the heat through which this is in ſome caſes employed cauſes this matter to contract a moſt diſagreeable empyreuma. It alſo prevents its burning in lamps, as well from its viſcidity as from the repugnance which the preſence of water gives to all oleaginous matter. It is therefore neceſſary to free the oil from this heterogeneous matter; after which it can be ſubject only to the rancid putreſcence, or that which is proper to oils as ſuch.

The ſubſtances which have been or may be applied to the removing or preventing the effects of putreſcence, are, acids, alkalies, metallic calces, neutral ſalts, ethereal and eſſential oils, vinous ſpirits, water, and air. With reſpect to acids, though they may be applied with effect to the removal or prevention of putrefaction in mixed animal and vegetable ſubſtances, yet they have not the ſame efficacy when employed in the caſe of oils; for in a ſmall proportion, without the ſubſequent aid of alkalies, they rather increaſe than diminiſh the fœtor, and in a large proportion they coagulate the oils, and change their other properties as well as their conſiſtence. Though they might therefore be employed with the aſſiſtance of alkalies, yet, requiring a more expenſive and complex proceſs, and not being moreover neceſſary, as the ſame end may be obtained by the uſe of alkalies only, they may be deemed improper for the purification of animal oils for commercial purpoſes. Alkaline ſubſtances, both ſalts and earths, are the moſt powerful inſtruments in the edulcoration of oils; but as their action on putrid oils, and the method of applying them to this end, are not the ſame in both, it is proper to conſider them diſtinctly.

Of alkaline ſalts it is the fixed kind only which are proper to be uſed for the edulcoration of oils. Fixed alkaline ſalts, in a diſſolved ſtate, being commixed with putrefying animal ſubſtances, appear to combine with the putrid matter, and, mixing with ſome of the principles, form inſtantly volatile alkaline ſalts. On the leſs putrid they ſeem to act, after their combination, by an acceleration of the putreſcent action, till they attain the degree which produces volatile ſalts. This is evident by the ſenſible putrid ferment and ſmell which appear after their commixture; but which gradually abating, the oil is rendered ſweeter, much lighter coloured, and thinner.

Their great uſe in the edulcoration of fiſh-oil ariſes therefore from their converting ſuch parts of the gelatinous fluid and bile as are highly putrefied inſtantly into volatile ſalts, and cauſing a rapid putrefaction of the other parts; by which means the oil is freed from them by their diſſipation. They do or themſelves form coagulums or correƈtions with it, a ſolution of ſalt will looſen the bond and diſſolve the cloſe union; ſo that the oil being ſeparated will float on the aqueous fluid, while the earth, if any be in the mixture, will be precipitated and ſink cloſe together to the bottom of the containing veſſel.

Sal catharticus, glauber ſalt, nitrum vitriolatum, tartar, and other neutral ſalts, though they counteract putrefaction in the mixed or ſolid parts of animals, ſeem to have little effect on oils with reſpect to their edulcoration, and cannot therefore be ranked amongſt the ſubſtances proper to be uſed for that purpoſe.

Lead reduced to the ſtate of a calx, either in the form of minium or litharge, has a ſtrong edulcorative power on fœtid oils, and is indeed applied to that end, with reſpect to one kind of vegetable oil, for a very bad purpoſe, conſidering its malignant qualities on the human body.

In the caſe of train-oil, which will ſcarcely ever be conſidered among the eſculent kinds in this country, the ſame objection againſt its uſe would not lie; and employed either with or without heat, it is a powerful abſorbent both of the putrid and empyreumatic parts that occaſion the fœtor.

As, however, there may be ſome prejudice againſt its uſe even in any way, and as it is not abſolutely neceſſary, I have not given it a place among the ingredients of the proceſſes I recommend.

The ochrous earth of iron, commonly called red ochre, has an abſorbing power on the putrid parts of oil, but combines ſo ſtrongly that the ſeparation is tedious even with the addition of brine: if, nevertheleſs, it is added when chalk and lime have been ſome time commixed with the oil, as in proceſs the firſt, it will promote the edulcorative intention, and will ſubſide along with them; and, as it has ſome advantage without increaſing the expenſe, unleſs in the moſt inconſiderable degree, its uſe may be expediently admitted in that proceſs.

Eſſential and ethereal oils are applicable to the prevention of putrefaction in the mixed and ſolid parts of vegetables, but are not ſo to the edulcoration of fœtid oils; and if they had the deſired effect, they would not, on account of their price, anſwer the commercial end, unleſs the due effect was produced by adding them to the oils in a very ſmall quantity.

The ſame holds good of ſpirits of wine as of eſſential and ethereal oils, both with reſpect to their efficacy and the expenſe.

Water has an edulcorative action on fœtid oils by carrying off the moſt putrid parts of the gelatinous fluid or bile, in which, as was above explained, the principal fœtor reſides, if the quantity added be large, and an intimate commixture be made of them by ſtirring them together for a conſiderable time: this only partially removing thoſe heterogeneous putreſcent ſubſtances, the remaining part ſoon acquires the ſame ſtate, and the oil again grows fœtid, though not to the ſame degree as before.

Water is, however, a neceſſary medium for the action of ſalts and the ſeparation of alkaline earths and calces of metals when they are employed for the edulcoration of oils, as will appear from a conſideration of my proceſſes.

Air edulcorates oil by carrying off the moſt putrid parts, which are neceſſarily extremely volatile. It may be made to act on them either by ſimple expoſure of them to it with a large extent of ſurface, or by forcing it through them by means of ventilators, as has been practiſed by ſome dealers; but is now, I believe, neglected on account of their finding the improvement of oils by it not adequate to the trouble, as the gelatinous matter and bile, not reduced to a certain degree of putrefaction, being left behind, putrefy again to nearly the ſame degree as before.

It appears from theſe ſeveral obſervations, that the cheapeſt ingredients which can be uſed for the edulcoration of train-oils are lime and chalk, which may, with the addition of a proper quantity of ſolution of ſea ſalt or brine, be made to procure a ſeparation of them from the oils, according to proceſs the firſt, ſo as to anſwer for ſome purpoſes; that the lixiviate ſalt is the moſt powerful purifier of oils, and, with the aſſiſtance of chalk and brine, will, without heat, according to proceſs the ſecond, effect a very conſiderable degree of edulcoration; and that lixiviate ſalt uſed with heat, with the addition of lime and chalk, to ſave a part of the quantity which would otherwiſe be neceſſary, and of brine to procure a quick ſeparation, will perform an edulcoration ſufficient for all commercial purpoſes, according to proceſs the third; but that calcined lead and the ochrous earth of iron may, perhaps, be applied in ſome caſes with advantage, where the oil is not deſigned for eſculent uſe.

PROCESS THE FIRST.

For purifying Fiſh-Oil in a moderate Degree, and at a very little Expenſe.

Take an ounce of chalk in powder, and half an ounce of lime ſlaked by expoſure to the air; put them into a gallon of ſtinking oil, and, having mixed them well together by ſtirring, add half a pint of water, and mix that alſo with them by the ſame means. When they have ſtood an hour or two, repeat the ſtirring, and continue the ſame treatment at convenient intervals for two or three days; after which ſuperadd a pint and a half of water in which an ounce of ſalt is diſſolved, and mix them as the other ingredients, repeating the ſtirring, as before, for a day or two. Let the whole then ſtand at reſt, and the water will ſink below the oil, and the chalk ſubſide in it to the bottom of the veſſel. The oil will become clear, be of a lighter colour, and have conſiderably leſs ſmell, but will not be purified in a manner equal to what is effected by the other proceſſes below given; though, as this is done with the expenſe of only one ounce of ſalt, it may be practiſed advantageouſly for many purpoſes, eſpecially as a preparation for the next method, the operation of which will be thereby facilitated.

PROCESS THE SECOND.

To purify, to a great Degree, Fiſh-Oil without Heat.

Take a gallon of crude ſtinking oil, or rather ſuch as has been prepared as above mentioned, and add to it an ounce of powdered chalk; ſtir them well together ſeveral times, as in the preceding proceſs, and, after they have been mixed ſome hours, or a whole day, add an ounce of pearl-aſhes diſſolved in four ounces of water, and repeat the ſtirring as before. After they have been ſo treated for ſome hours, put in a pint of water in which two ounces of ſalt are diſſolved, and proceed as before: the oil and brine will ſeparate on ſtanding ſome days, and the oil will be greatly improved both in ſmell and colour. Where a greater purity is required, the quantity of pearl-aſhes muſt be increaſed, and the time before the addition of the ſalt and water prolonged.

If the ſame operation is repeated ſeveral times, diminiſhing each time the quantity of ingredients one-half, the oil may be brought to a very light colour, and rendered equally ſweet in ſmell with the common ſpermaceti oil.

By this proceſs the cod-oil may be made to burn; and, when it is ſo putrid as not to be fit for any uſe, either alone or mixed, it may be ſo corrected by the firſt part of the proceſs as to be equal to that commonly ſold: but where this proceſs is practiſed in the caſe of ſuch putrid oil, uſe half an ounce of chalk and half an ounce of lime.

PROCESS THE THIRD.

To purify Fiſh-Oil with the Aſſiſtance of Heat, where the greateſt Purity is required, and particularly for the Woollen Manufacture.

Take a gallon of crude ſtinking oil, and mix with it a quarter of an ounce of powdered chalk, a quarter of an ounce of lime ſlaked in the air, and half a pint of water; ſtir them together, and, when they have ſtood ſome hours, add a pint of water and two ounces of pearl-aſhes, and place them over a fire that will juſt keep them ſimmering, till the oil appears of a light amber colour, and has loſt all ſmell, except a hot, greaſy, ſoap-like ſcent. Then ſuperadd half a pint of water in which an ounce of ſalt has been diſſolved; and having boiled them half an hour, pour them into a proper veſſel, and let them ſtand till the ſeparation of the oil, water, and lime be made, as in the preceding proceſs. Where this operation is performed to prepare oil for the woollen manufacture, the ſalt may be omitted; but the ſeparation of the lime from the oil will be ſlower, and a longer boiling will be neceſſary.

If the oil be required yet more pure, treat it, after it is ſeparated from the water, &c. according to the ſecond proceſs, with an ounce of chalk, a quarter of an ounce of pearl-aſhes, and half an ounce of ſalt.

Obſervations on Proceſs the Firſt.

This proceſs may be performed on any kind of fiſh or ſeal-oil that is putrid and ſtinking, and will improve it in ſmell, and generally render the colour lighter, if previouſly dark and brown: it will alſo conduce to render theſe oils fitter for burning, which are, in their crude ſtate, faulty in that point; but it will not meliorate them to the full degree they admit of even without heat, and ſhould therefore be practiſed when only a moderate improvement is required.

Secondly, When the oil is taken off from the dregs and brine, the dregs which ſwim on the brine ſhould be taken off it alſo, and put into another veſſel of a deep form; and on ſtanding, particularly if freſh water be added and ſtirred with them, nearly the whole remaining part of the oil will ſeparate from the foulneſs; or, to ſave this trouble, the dregs, when taken off, may be put to any future quantity of oil that is to be edulcorated by this method, which will anſwer the ſame purpoſe.

Obſervations on Proceſs the Third.

Firſt, This is moſt advantageouſly performed on train-oil, called vicious whale-oil; and the more putrid and foul it may be, the greater will be the proportionable improvement, eſpecially if there be no mixture of the other kinds of fiſh-oils, particularly the ſeal, which do not admit of being edulcorated perfectly by means of heat, but require other methods: but when the vicious oil is pure from admixture of others, however ſtinking it may be, the bad ſmell will be removed by this proceſs duly executed, and the brown colour changed to a very light amber; and theſe qualities will be much more permanent in this than in any crude oil, as it will not, from the degree of purity to which it is brought, be ſubject to putrefy again under a great length of time, whether it be kept open or in cloſe veſſels.

The oil in this ſtate will burn away without leaving the leaſt remains of foulneſs in the lamp; and, being rendered more fluid than before, will go further, when uſed in the woollen manufacture, than any other kind, and will be much more eaſily ſcoured from the wool.

If, nevertheleſs, there be any branches of the woollen manufacture which require the uſe of a more thick and unctuous oil, this may be rendered ſo by the addition of a proper quantity of tallow or fat, of which a certain proportion will perfectly incorporate with the oil, the fluidity and tranſparency being ſtill preſerved, as well as all the other qualities that render it ſuitable to the intended purpoſe. This may be moſt beneficially done by adding a proper quantity of the refuſe greaſe of families, commonly called kitchen ſtuff, which being put to the oil, when moderately heated, will immediately diſſolve in it, and let fall alſo its impurities or foulneſs to the bottom of the veſſel, and render the purified admixture a conſiderable ſaving to the manufacturers.

Secondly, The different qualities and diſpoſitions of different parcels of vicious oil with reſpect to edulcoration render various proportions neceſſary of the ingredients to be uſed. The quantities ſtated in the above proceſs are the leaſt which will effect the end in general, and frequently greater will be required; but this may always be firſt tried: and if it be found, after ſix or eight hours ſimmering of the mixture, that no gradual improvement is making in the ſmell and colour, but that the oil continues the ſame in thoſe particulars, and remains alſo mixed with the chalk and lime, and in a thick turbid ſtate, a fourth or third part of the firſt quantity of pearl-aſhes ſhould be added, and the ſimmering continued till the oil be perfect. As the quantity of the water is leſſened by the evaporation, it is neceſſary to make freſh additions from time to time, that there may be always nearly the original proportion.

Thirdly, If it be inconvenient to give the whole time of boiling at once, the fire may be ſuffered to go out and be rekindled at any diſtance of time; and if, in ſuch caſe, a ſmall proportion of pearl-aſhes diſſolved in water be added, and the mixture ſeveral times ſtirred betwixt the times of boiling, it will facilitate the operation. The time of boiling may be alſo much ſhortened, if the chalk, lime, and pearl-aſhes, be added for ſome days before, and the mixture frequently ſtirred.

PROCESS THE FOURTH,

Which may be practiſed alone inſtead of Proceſs the Firſt, as it will edulcorate and purify Fiſh-Oil to a conſiderable Degree, ſo as to anſwer moſt Purpoſes, and for Proceſs the Third, when the whole is performed.

Take a gallon of crude ſtinking oil, and put to it a pint of water poured off from two ounces of lime ſlaked in the air; let them ſtand together, and ſtir them up ſeveral times for the firſt twenty-four hours; then let them ſtand a day, and the lime-water will ſink below the oil, which muſt be carefully ſeparated from them. Take this oil, if not ſufficiently purified for your purpoſe, and treat it as directed in Proceſs the Third, diminiſhing the quantity of pearl-aſhes to one ounce, and omitting the lime and chalk.

ROBERT DOSSIE*.

XXI. *On Painting. By Mr.* E. DAYES, *Painter.*

ESSAY IX.

On Style.

> Nor, whilſt I recommend the ſtudying the art from artiſts, can I be ſuppoſed to mean that nature is to be neglected. *Sir Joſhua Reynolds.*

PERFECTION in painting requires that the choice, the imitation and execution be directed to the ſame idea: this only can produce a perfect unity in the whole work. It will appear defective in proportion as it is deprived of a union of theſe qualities; as that which poſſeſſes the feweſt

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[*] The dregs remaining after the ſundry proceſſes above mentioned will form an excellent manure, as has been ſince noticed in Dr. Hunter's Georgical Eſſays.

faults will ever be the moſt perfect work. Hence the foundation of a rational inquiry after excellence in art; for he who wiſhes to examine a picture nicely or critically, muſt make himſelf acquainted with the firſt idea, or general intention of the whole, then of each part ſeparately, conſidering them in themſelves as relative to each other and to the whole.

An inquiry after the different ſtyles of painting would naturally lead to a hiſtory of the art itſelf; but as that is a ſubject that has been ſo frequently handled, we ſhall only touch ſo far on it as may immediately anſwer our purpoſe.

The ſo much boaſted criticiſms on the works of the antients, which are to be found in various authors, can be but little depended on, though often dreſſed up in the technical phraſes of art; becauſe the ſtrongeſt language will always be uſed on the moſt extraordinary occaſion, but the advantage reſulting to the reader will depend on the capacity of the writer.

All diſcuſſion at preſent on works that have ceaſed to exiſt for near 2000 years would be only loſs of time, as, from want of reference, they could not tend to illuſtrate any one rule of art. Such inquiries may ſerve to amuſe the idle, but will not tend in the leaſt to benefit the artiſt.

No one can deny, however humiliating the thought may be, that we are indebted to the Greeks for ſome of the fineſt forms at preſent in art. Many have attributed their ſuperior powers to the climate: but we ought rather to aſcribe them to the freedom they enjoyed, and the wiſdom of their legiſlators; not forgetting their enthuſiaſm for beauty, which they conſidered as a gift of the gods; and that with them men were more valued for their perſonal merit than for the wealth they poſſeſſed. The happy Grecian might ſit himſelf down contented in his poverty, knowing it was not thought diſreputable, and that his abilities would prove a ſufficient paſſport to ſociety and to the higheſt honours. The modern artiſt is ſolicitous of making money, knowing it is now the firſt teſt of his abilities in the opinion of the world, and a ſtronger recommendation than wiſdom. Hence the nobler energies of the ſoul are weakened; and from want of the ſtronger motive, honour, the love of fame ſinks into the baſe and ſelfiſh deſire of wealth. This has induced the weak and puſillanimous to apply to the arts; and as they aſpire after nothing but intereſt, they are unacquainted with thoſe ſublime conceptions the arts require.

The Phœnicians are believed not to have carried the arts to any great degree of perfection. To be ſufficiently well for the purpoſe of exportation would be the utmoſt that would be required; and perhaps their merchants would not be inclined to riſk much on articles the buyers of which would be the wealthy, and often the ignorant. What might be requiſite for the purpoſe of ornament would be more likely to be ſhowy than juſt.

The Romans appear to have encouraged the arts as a means of adding to their pride, and of feeding their vanity. As a people, they are rather to be viewed at a diſtance; their character is too ſelfiſh and oppreſſive to invite to an intimacy. The beſt works produced among the Romans from Auguſtus to Trajan are juſtly held to be of Grecian workmanſhip.

It is always more eaſy to imitate the ſtyle than the reaſonings and ſcience of the original: hence thoſe who ſucceeded the early Greeks failed by degrees in the moſt eſſential parts; which may account for that ſort of mechanical harſhneſs, and often want of elegance, obſervable in ſome of the works called Roman.

We have no right to look for true taſte among a people who could drag their captives at their chariot wheels, or ſlaughter them in theatres. They appear to have preferred pomp and ſhow, " barbaric pearl and gold," to true taſte; and exhibit a picture of gaudy and well-diſciplined barbarians. A nation which places the firſt reputation on arms or war, will never have a higher eſteem for the peaceable profeſſor of deſign than a Spartan or Roman. Many of their moſt renowned works are recommended more to notice on account of their richneſs than beauty, as Nero's golden palaces, &c. Pomp and luxury was their object; and they fell into the common error of ignorance, that of augmenting the matter inſtead of improving the form *.

Among the moderns, every country that has formed a ſchool appears to have had a cauſe for its ſtyle in its national character. The gravity of the Florentines and Romans might require that juſtneſs and truth of form which they ſaw in the antique ſtatues. The Venetians, a wealthy and gay people, would delight in magnificence from their commerce with the Eaſt; and painting for the rich and luxurious would neceſſarily introduce ſplendour and brilliancy of colour, with proceſſions, feaſts, &c. The Flemiſh and Dutch were content with ſuperficial or general repreſentations of things. Of the French, the beſt maſters have ſought perfection in the Roman ſchool; while others, complying with the national love for ſplendour, have ſought perfection by buſtle and ſhow. Of our own nation, the love of locality and portraiture may be

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* In point of order, the Phœnicians ſhould precede the Grecians; but I have paid no attention to chronological arrangement.

ſaid ſtrongly to mark the *amor patriæ*, and to exhibit their charity and love for each other.

A knowledge of the different ſtyles is abſolutely neceſſary to enable us to think deeply and freely. No man, however great his powers, was ever capable of ſubſiſting on his own ſtock: the more wide the field of inquiry, the more we increaſe our knowledge, and quicken and enlarge our ideas. We cannot doubt that M. Angelo and Raphael poſſeſſed all the knowledge of art diſcovered in their time. 'Tis our duty to live, as it were, amidſt the great works of art, that, by enriching our minds, we may be able to produce ſomething great and noble of our own. This is the true ſpirit of imitation, and which we may continue, without fear of its doing us an injury, from infancy till we arrive at the fulleſt ſtate of vigour.

By ſtyle as well as manner we are enabled to judge if a picture be the work of a certain maſter; for, ſhould we remain doubtful from the former, a reference to the penciling may determine the point. For, as in writing or ſpeaking we ſhall generally diſcover in perſons a fondneſs for certain phraſes, or a peculiar turn or connection of their ſentences, by which their ſtyle may be known: ſo in painting we ſhall diſcover ſome favourite part forced on the eye; or thought, attitude, or habit occur, which ſtamps the maſter's ſtyle. Every one will poſſeſs more or leſs of the ſtyle and manner of the maſter or ſchool from which he comes. Raphael, in his oil pictures, never wholly got rid of that littleneſs of ſtyle derived from Perugino.

To inſure a good ſtyle we muſt early habituate ourſelves to contemplate noble works of imagination. This, if connected with a vigorous mind, a lively fancy, a ſtrong memory, and good judgment, will be attended with ſucceſs. That thoſe qualities are neceſſary to enable us to form a good ſtyle, is certain; for by the imagination we conceive images, and if the impreſſions be clear the ſtyle will be ſo too. But ſhould the images be faint and imperfect, the ſtyle will partake of theſe defects. For it cannot be denied that, as the painter is affected himſelf, in the ſame degree will he move the ſpectator. Hence, if the mind be dull, and indiſpoſed to receive clear and diſtinct ideas of things, the ſtyle will be ſtiff and heavy; or, if the images be irregular and diſordered, the work will be perplexed and confuſed. It appears to follow of courſe, that a lively fancy will be accompanied with a happy memory: through this we are ſupplied with the vaſt treaſures of art and nature; for without a large ſtock of images we ſhall never be able to diverſify our works in the way variety

riety demands, and they would appear inſipid from the too frequent return of the ſame ideas. But, unleſs all this be accompanied by a good judgment, the imagination will riot at the expenſe of reaſon, and we ſhall never poſſeſs a ſound and accurate ſtyle. Hence it is that we often confound genius with an active imagination, not recollecting that exceſs is not its character, and that the more we crowd with incidents the more we weaken; and that, like great talkers, we may be bad orators.

We ſhall now proceed to deſcribe, in the beſt way we can, the characters of the various ſtyles. Taſte and ſtyle differ from each other; the former applying to our choice of objects, while the latter appears to ariſe from our mode of treating them, or the augmenting certain parts and depreſſing others, as in the figures of M. Angelo, in which we ſee the convex lines raiſed ſo much as to give them a muſcular and gigantic air; by attempting which many of his imitators have often loſt the eſſential character of the object. We ſay "the great ſtyle of Raphael's drapery," not merely on account of the caſting of the folds, which ſhows *taſte*, but from his dropping the minute parts, and taking only thoſe eſſential to the great character in painting. The artiſts who have ſought the great ſtyle have purſued the above conduct; that is, have attended to the great and eſſential character, while thoſe who have forced the trifling parts on the eye have formed a mean or little one. The great ſtyle requires the human face, the other includes the wrinkles and other marks of the infirmities of nature.

Sublime Style.

The ſublime ſtyle is by many connected with a certain degree of intemperance and exceſs. The attempts of ſuch may juſtly be termed the hobgoblin ſtyle; for with them nothing is ſublime but what will ſcare a man out of his ſenſes. But ſcenes terrible or ſhocking, however admiſſible in poetry, or where an orator may have occaſion to work on the feelings of his audience, do not aſſociate happily with painting; and, when attempted without the greateſt care and circumſpection, become either ridiculous or diſguſting. Brueghel (called helliſh), as alſo Callot, in their whimſical ſcenes intended to affect us ſeriouſly. And Rubens, out of the number of repreſentations he has left of the fall of the damned, has rather ſhown a warmth of imagination than judgment in their treatment: it is true, the machinery he had to uſe was human figures; but inſtead of making the moſt of them by exhibiting their ſufferings, and thereby applying to our feelings, he has created a ſet of fiends that make us laugh. Scenes of horror require great delicacy of treatment, as the mind cannot dwell without diſguſt on repreſentations brought home in that determined way which painting demands. Pilkington relates, from Sandrart, that Spagnoletto repreſented an Ixion on the wheel ſo full of pain and agony, that the wife of Mr. Uffel, the poſſeſſor, from looking on it when with child, was ſo affected by it, "that her child when it was born had all the fingers diſtorted exactly as the fingers of the Ixion appeared in the picture." So ſenſible of the neceſſity of this delicacy have the beſt painters been, that they have concealed as much as poſſible the ſhocking parts of a ſpectacle. In the Slaughter of the Innocents, Pouſſin would be content with an incident or two, while Le Brun in the ſame ſubject has aggravated all the horrors by an endleſs variety of butcheries. Rubens, in one of the fineſt ſketches of his we have had in this country, has repreſented, in a diſguſting way, a ſaint with his hands and feet cut off; and has moſt injudiciouſly, nay, moſt inſenſibly and indelicately, introduced dogs licking up the blood. Contraſt the above and Titian's picture of the winding out a ſaint's bowels on a wheel, with Domenichino's delicate and ſenſible repreſentation of the death of St. Cecilia. A Britiſh artiſt would diſplay more feeling and delicacy in repreſenting a bull-bait, than either Titian or Rubens have done in the above compoſitions. Juſtice muſt allow that a want of delicacy is not often chargeable on the Italians.

In the ſmall compaſs we have been obliged to preſcribe to ourſelves, we fear it will become difficult ſufficiently to compreſs the matter, and at the ſame time render the ſubject tolerably clear.

We take it for granted that the mind well informed is the true ſtandard of whatever is great and illuſtrious in any point of view. Hence it will follow that in works of imagination we ſhall derive credit in proportion as we diſplay more or leſs mental energy. For though we may not poſſeſs the power of colouring, or ſuch other excellences as would captivate the eye, yet we may exert the vigour of the ſoul in the reaſoning and ſcience of the work; and this energy it is in our power to improve by education and habit. One of our firſt duties is to enlarge and elevate our notions; for the dignity and grandeur of our work depend entirely on the dignity and grandeur of our thoughts, and the elevation of the ſoul. A greater misfortune cannot attend the arts than for men of mean parts to practiſe them, either as painters or engravers, as they invariably communicate the ſame bad qualities to their works. We may look below the ſuperficies, and not be dazzled with a gaudy appearance, or ſuffer our admiration to dwell on what the wiſe would contemn; or we may be led away after pageantry and pomp, miſtaking them for true honour and glory. By exerting the mind we may raiſe our work into the ſublime, from a judicious introduction of ſuch acceſſory circumſtances as may add dignity or contribute to the illuſtration of the ſtory; particularly if they have a ſkilful connection, and affect the imagination. We have noticed ſome ſuch inſtances already in our Eſſay on Invention, as in Raphael's Paul at Lyſtra, Barry's picture of Elyſium, &c. Theſe acceſſory circumſtances occur frequently in the works of the poets, and contribute much to heighten the ſcene; as in Lear, the thunder ſtorm makes the heart bleed for the ſufferings of the old king: and how ſublime does the viſion of the dagger render Macbeth by its judicious introduction! Vaſtneſs is productive of the ſublime, by conſidering any object that takes up much room in the fancy. But we muſt be careful not to fall into a common error, and miſtake greatneſs of bulk for nobleneſs in works of art: one of Lyſippus's ſtatues of Alexander, though no bigger than the life, might give the mind more noble ideas than Mount Athos had it been cut into the figure of that hero. This ſubject has been in ſome meaſure amplified already in our Eſſays on Invention and Compoſition: it may therefore be ſcarcely neceſſary to mention, that the ſublime requires that the ſubject of our picture ſhould be a great one, and, if poſſible, carry with it a univerſal intereſt. In the compoſition, ſimplicity and gravity are eſſential to produce grandeur: we cannot wiſh for better examples than are offered to our view in the Cartoons of Raphael; particularly the Paul at Lyſtra, his Preaching at Athens, and in the Death of Ananias. We ſhall obſerve great grandeur in thoſe fine compoſitions by Weſt, of the Departure of Regulus, Mark Antony haranguing over the body of J. Cæſar, and the Swearing young Hannibal: the Death of Stephen by the ſame artiſt is full of the pathetic: the prints from the above are in every one's hands. Too much contraſt deſtroys the great ſtyle; it aſſociates beſt with the pleaſing. Beauty excites gaiety and pleaſure, the ſublime inclines to ſerioufneſs. Elevation of character is alſo neceſſary; a want of nobleneſs contributes much to depreciate the works of the Flemiſh, and particularly of the Dutch ſchool. The Hercules and the Apollo Belvidere in their form approximate moſt to the ſublime, except we be permitted to conjecture what might have been the famous Jove and Minerva of Phidias. A broad light and ſhade, or, in other words, a fine *chiaro-ſcuro*, add an effect of ſublimity. Some of Rubens's pictures ſtrike wonderfully on that account, and Reynolds's Infant Hercules receives an uncommon air of grandeur from the broad and judicious diſpoſition of the maſſes. Though this ſtyle does not require the ſoft harmonious glow of Venetian colouring, which agrees beſt with the beautiful, yet it by no means follows, that it authorizes a diſregard of the grouping of the colours, to the total neglect of the general harmony of the picture. The colouring ſhould be ſober and dignified; we do not mean black and heavy, but compoſed of ſuch colours as are full, rich, glowing, and rather deep than light. The penciling ſhould be firm and deciſive, with the parts well defined. We ſhould particularly guard againſt trifling events, poor or mean thoughts, and whatever is low and vulgar: ſuch things mar a good whole, and appear worſe by contraſt, like the blemiſhes on beautiful bodies. The moſt eſteemed maſters have carefully avoided introducing in their works things ſordid and baſe: in the higher ſtyle there ſhould never appear any thing inſignificant or unneceſſary, as dogs, cats, parrots, &c.: ſuch things as theſe often add a grace to the pictureſque, but deſtroy grandeur. Neither will theatrical ſplendour or gaudy apparel ſuit the ſublime: the reaſon is, they affect not beyond the eye; and that which leaves no impreſſion on the mind, we may reſt aſſured, is not the thing we ſeek. A work truly ſublime does not merely pleaſe: that is the province of inferior excellence. A grand work will confound, aſtoniſh, and, with the impetuoſity of a hurricane, bear down all before it. The ſublime appears founded on a union of the moſt noble and elevated parts of nature joined to the moſt profound efforts of the imagination. Its qualities appear to be ſimplicity, with a certain uniformity united with ſolemnity and gravity. Simplicity is neceſſary to nobleneſs, as ornament deſtroys greatneſs of manner. So the parts ſhould be ample, as is effected by large mantles, &c.

> At Agis' ſummons, with a mantle broad
> His mighty limbs Leonidas unfolds,
> And quits his couch. GLOVER.

M. Angelo and Raphael approach this ſtyle in their ideas and inventions, but neither appears to accord with it in his forms. M. Angelo, from attempting the ſublime, produced what may be termed the *terrible*, in which the attitudes are forced and extraordinary, and the figures vaſt, robuſt, and muſcular: he choſe in expreſſion the point moſt extreme, and generally departing from objects in themſelves pleaſing.

Expreſſive

Expreſſive Style.

In this Raphael ſtands a ſhining example, and appears to have touched the extreme point of excellence. Happily we have many of his fineſt works at hand in the Cartoons at Windſor Caſtle. Witneſs his Elymas the ſorcerer, whoſe figure is moſt juſtly expreſſive of the puniſhment of blindneſs which had befallen him; and the aſtoniſhment of the ſpectators is depicted with great judgment. But let us contraſt the above with the Death of Ananias, and obſerve the conſternation and terror in the ſpectators at a puniſhment ſo much more terrible. There we behold with what a maſterly hand the artiſt has raiſed the feelings in proportion to the exciting cauſe, without the leaſt waſte of force in the expreſſion. In the firſt picture is more repoſe; in the ſecond, a greater degree of action correſponding with the intereſt excited by the event. In the firſt picture we ſee the ſurpriſe of an earthly judge at an event beyond the power of his underſtanding; in the other, a calm and dignified ſet of beings, conſcious of the interpoſition of a Divine Power. Much has been ſaid of the Cartoons; every one ſpeaks of the dignity of the Paul in the picture repreſenting his "preaching at Athens," which for ſimplicity and dignity reaches the ſublime. Hiſtory is the walk that ennobles the art. This calls forth exertions that elevate and dignify our nature. It might be doubted if the Greek ever arrived to that degree of perfection in expreſſion which Raphael has ſhown in his works: they certainly ſacrificed it to the beauty of appearance. Still we ſhall look with delight on the Laocoon, the dying Alexander, the Niobes, &c. Raphael, to aſſiſt the expreſſion in his figures, appears to have marked the tendons; thoſe who have ſought beauty have attended more to the fleſhy parts of the muſcles. His ſtyle of drapery is ſimply grand, in the extreme of perfection, and well worthy of being ſtudied. Even Pouſſin, who is ſaid to excel in it, is in his beſt works inferior. Raphael is more judicious and ſelect; Pouſſin more natural. Juſtice muſt allow that the former took the hint for this great ſtyle of drapery from M. Angelo. Raphael ſometimes reached the graceful, and but ſeldom the beautiful, and never exquiſitely. We have ſelected the works of the above artiſt, eſteeming them the firſt for expreſſion; but we ſhall find admirable traits of the ſame kind in thoſe of the Caracci, Domenichino, Pouſſin, &c. Some who have attempted the expreſſive ſtyle, imagining the whole merit lay in exceſs and violence, have writhed their figures into convulſions, when the occaſion would not require more exertion than would be neceſſary to take a pinch of ſnuff or raiſe a ſtraw; nay, they will not let them even ſleep in peace. Such bombaſtic ſtuff may amuſe the weak, as faſhion in art will pleaſe for a time even at the expenſe of truth; but the triumph will be tranſitory as a ſun-beam on a winter's day.

Beautiful Style

Requires that the objects be elegant, void of ſuperfluity, and ſoft in the execution. Perhaps the truly beautiful ſtyle muſt be ſought in the works of the antients. The Apollo Belvidere approaches the ſublime, and forms, of its kind, a point of perfection: ſome of the Niobes exhibit exquiſite female beauty, but in the Venus de Medicis and Apollo we ſee it united with grace. It is certain that in the antique ſtatues we muſt ſeek the beauty of ſymmetry, and that we cannot ſtudy them too often to fix the impreſſion of their excellencies on our minds; for it is next to impoſſible but that every reconſideration muſt unfold new beauties. The true mode of ſtudy is to impreſs their beauties ſo ſtrongly on the imagination as never to be forgotten, and not to ſtand in need of their preſence as a pattern. Boys are too often put to draw after the living model before they have imbibed a proper notion of, or reliſh for, beautiful proportion. There is no danger of ſuch ſtudies injuring, as we are ſurrounded by fine examples in *chiaro-ſcuro* and colour; and of the two, deciſion is more commendable than that ſlovenly manner which preſents the mere idea of a thing like a dream: the former diſplays knowledge, the latter ignorance. There is great difference in painting, between taking the ideas of natural things without giving them form, and that determined method ariſing from a poſitive knowledge. Guido's heads are beautiful; but his choice was often injudicious, many of his objects requiring ſtrong expreſſion, which he loſt for fear of deſtroying beauty. Albano's females are delicately beautiful; and we are ſurrounded by beauty in the portraits of Reynolds and others.

Graceful Style.

To form this ſtyle the motions of the figures ſhould be moderate, eaſy, agreeable, and unaccompanied by violence*. The antique ſtatues will aſſuredly afford the moſt perfect examples, becauſe, on a compariſon with the antients, the moderns appear to have become a little affected, and too often to have ſought grace in difficult attitudes, with forced and conceited turns in their figures. There are few of the Greek ſtatues in which grace does not abound; for, as they ſought beauty in their works, they cultivated grace as its inſeparable companion. Very graceful are the Apollo and the Venus de Medicis; ſo are the Meleager and Hermaphrodite. The Hercules is alſo truly graceful and eaſy, as is the Antinoüs, &c. Raphael underſtood the grace of motion; but he had it leſs in the contours of his figures; and his dry manner of execution contributed much to deſtroy beauty. Corregio appears the moſt perfect modern, Parmegiano being ſometimes too forced, and bordering on affectation. Reynolds's female portraits appear to have been touched by the Graces themſelves. Among the works of our ſculptors, the females of Nollekens poſſeſs ſuch a combination of grace and beauty as the Greeks might have acknowledged for their own without a bluſh.

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* See the Eſſays on Grace and Beauty.

Florid Style.

This ſtyle muſt be ſought amongſt the Venetians, alſo in Rubens, and thoſe Flemings who have followed him. Thoſe who have practiſed it have been content to pleaſe the eye by a brilliant diſplay of colour, contraſt in their groups, and oppoſition in light and ſhade. They appear to have admitted every deſcription of objects in their picture, provided they contributed to the buſtle or pleaſure of the whole. If this is not the moſt juſt ſtyle, experience teaches every one that it is the moſt popular, and perhaps we may add the moſt profitable.

The above maſters muſt not be confounded with thoſe who have practiſed a ſtyle ſufficiently eaſy, but who have been content with giving ſuch an idea of a thing in their works as would ſerve to diſtinguiſh one from another without regarding their perfection. At the head of this latter claſs ſtand P. Cortona and L. Giordano.

Natural Style.

This ſtyle is called natural, from the artiſts practiſing it wanting the power to improve on the original, or of chooſing the beſt of the kind. It is to be ſeen in the Dutch ſchool in the works of Rembrandt, G. Dow, Meris, young Teniers, &c. This ſchool has improved the aërial perſpective, and poſſeſſes great excellence of colour and *chiaro-ſcuro,* to which it has joined great truth, as far as a ſimple repreſentation would permit. It has excelled in what may be termed the mechanical part of the art; and thoſe who wiſh to ſeek after an elegant power of penciling will be highly gratified in the admirable works of Teniers, Cuyp, Berghem, &c. &c.

As each of the above ſtyles has its peculiar beauties, we muſt cautiouſly guard againſt falling into the oppoſite extreme. For the ſublime is nearly allied to the extravagant; the expreſſive may be eaſily rendered bombaſtic or pedantic; and the beautiful, ſmirking or coquettiſh. The graceful, with the ignorant, may eaſily become the affected: perhaps there is but a ſlight partition between that affectation of grace in the works of Watteau, Bouché, and Le Moine, and the pure repreſentations of Corregio, or thoſe of Parmegiano. The florid may be miſtaken for the ſhowy and tawdry. This is moſt likely to happen from a cold lifeleſs imitation.

Works of genius are ever the reſult of feeling, to which we muſt be directed by ſpirit and judgment. A painter, a poet, or player, that imitates cloſely, will never excel. To be tamely alive in our works is not enough; ſpirit, vivacity, and vigour, are required. Whatever comes ſhort of this is but one remove from imbecility.

XXII. *Memoir on the comparative Anatomy of the Electric Organs of the* Torpedo, *the* Gymnotus electricus *and the* Silurus electricus. *By* E. Geoffroy *.

SINCE philoſophers have applied with ſo much ſucceſs to reſearches reſpecting the Galvanic phænomena, it is of more importance than ever that a correct deſcription ſhould be given of the particular organs of certain fiſhes which are found to poſſeſs electric properties.

Analogy gives us reaſon to believe, that beings which poſſeſs faculties ſo extraordinary are indebted for them to an organization almoſt entirely ſimilar; conſequently, that theſe beings approach very near to each other, or rather, that they form only one family. But by the examination we are enabled to make, this is not obſerved to be the caſe: on the contrary, we are ſurpriſed to find that the electric fiſhes belong to genera exceedingly different, and that they are placed in theſe genera without violating in any manner the order of natural relations. An electric ſpecies, therefore, is known in each of the genera, of ray, tetrodon, trichiurus, gymnotus, and ſilurus.

To account for the great diſſimilarity of the fiſhes which are diſtinguiſhed from others of the ſame genera by the preſence of electric organs, we muſt admit that the latter are not eſſentially connected with organs of the firſt importance, and that they belong at moſt to the common integuments, which vary in each ſpecies without producing any remarkable modification in the reſt of the organization.

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* From *Annales du Muſeum National*, No. 5.

This, however, is a reſult to which our ſtate of knowledge in regard to thoſe electric organs hitherto obſerved, does not ſeem to conduct; for, if we adopt the opinion of moſt anatomiſts, it will be found that the electric organ in the torpedo is very extenſive, exceedingly complex, and particularly remarkable in this reſpect, that nothing analogous to it has ever been obſerved. The Italian ſchool, at the head of which were Redi and his pupil Lorenzini, for a long time conſidered the numerous tubes of which that organ is formed as ſo many peculiar ſmall muſcles, *muſculi falcati*; and this opinion was generally adopted till the late John Hunter publiſhed his excellent Anatomical Deſcription of the Torpedo.

During my travels I have alſo had an opportunity of ſeeing torpedoes, and I ſoon diſtinguiſhed the electric batteries with which they are abundantly furniſhed. As it was by touching theſe kinds of apparatus that I experienced the ſtrongeſt ſhocks, and as the other rays exhibited nothing of the ſame kind, I entertained no doubt that I had before my eyes thoſe organs by means of which the torpedo renders itſelf ſo formidable in the boſom of the water, and can at pleaſure ſtrike with numbneſs thoſe animals which it wiſhes to make its prey. But at that time I was ignorant whether others before me had examined this organization; and, in that caſe, what addition to the obſervations already made it would be neceſſary for me to make. Being ſhut up in Alexandria during the time of the ſiege, and deprived of the uſe of my library, I conſoled myſelf for not being immediately able to clear up my doubts, by entertaining a hope that theſe organs were not known, at leaſt in regard to their relation with general phyſiology. To obtain that knowledge I endeavoured to find ſomething analogous in the other rays, being perſuaded that it was not ſo much the preſence of this organ, as a diſpoſition peculiar to it, that gave the torpedoes alone of all the rays the aſtoniſhing faculty of fulminating, as we may ſay, the ſmall inhabitants of the ſea. Thoſe who have compared only a few animals muſt know that few new organs are found among them, and particularly in ſpecies which reſemble each other ſo much as the rays. It was natural therefore to believe, that the tubes containing a gelatinous ſubſtance in the torpedo exiſted in a ſtate of concealment in the other rays; and it will here be ſeen that I indeed found in the latter an analogous organization, with differences to which we muſt refer the different modes of exiſtence and action of each ſpecies.

Rays, as is well known, are flat fiſh, the pectoral fin of which is prolonged anteriorly on the ſides of the head by means of a cartilage that borders its circumference. The torpedo differs from other fiſhes of the ſame genus by a very conſiderable interval between this cartilage and the head. All this large vacuity is filled up by priſms of ſix, five, and ſometimes four planes. Theſe priſms adhere by their baſes to the ſkin above and to that below. They are arranged parallel to each other, follow the projecting and irregular contours of the head and branchiæ, and externally form a ſemi-elliptic ſtratum. When the ſkin is removed, all theſe priſms, the baſes of which are then obſerved, exhibit the appearance of a honey-comb. They are ſo many ſmall tubes filled with a ſubſtance which by chemical analyſis I found to be a compound of gelatin and albumen. The texture of theſe tubes is aponeurotic, and they are united to each other by a kind of lax reticulation formed of tendinous fibres which envelop them in every direction: in the laſt place, they are covered and ſhut by an aponeurotic membrane, and above theſe coverings the ſkin is applied. This apparatus is furniſhed with nerves remarkable for their large ſize. There are diſtinguiſhed four principal trunks, which are diſtributed to all the tubes, and which at length penetrate into their ſubſtance and expand in it.

Though rays, in which the cartilage of the pectoral fin immediately borders the contours of the head, were not, like the torpedo, in a condition to exhibit priſms or vertical tubes, they did not differ from them ſo much as might be expected. In the rays, as well as in the torpedoes, there iſſues from the cranium, a little before the ear, a nerve ſo large that it ſurpaſſes the volume of that which proceeds to the eye. This nerve proceeds laterally, creeps over the ſuperior face of the maſſeter, and expands below, between that muſcle and the firſt branchia, in a maſs which on the firſt view might be taken for a gland, but which is really the focus from which proceed, in ſeveral bundles, a great number of tubes analogous to thoſe of the torpedo. A bundle proceeds towards the noſe, another ſpreads over the belly, a third aſcends on the maſſeter and terminates behind the occiput, and a fourth extends over the muſcles of the pectoral fin. In this reſpect there are ſome differences according to the ſpecies: but theſe tubes, in the ray as well as in the torpedo, always adhere to the ſkin above and to that below; only, inſtead of being vertical, which is impoſſible, for want of room, they follow the contour of the head, extend over the moſt exterior muſcles, and are longer according as they have a larger circuit to make before they are inſerted in the ſkin. Theſe long tubes ſeem to be of the ſame nature as thoſe of the torpedo, and contain a gelatinous and albuminous ſubſtance entirely ſimilar. Hitherto we obſerve in this reſpect no other differences between the common rays and the torpedo, except that the tubes in the latter are very ſhort, vertical, cloſe to each other, and parallel; while in the other rays they are much longer, bend around the principal muſcles of the electric machines, and divide into ſeveral bundles formed of divergent radii.

But if theſe organs do not vary in each ſpecies but by a different arrangement of parts, is it not to be apprehended that we may fall into a conſequence contrary to the facts obſerved, and ought we not to ſuppoſe that all rays are more or leſs endowed with the electric properties of the torpedo? Such, indeed, would be the opinion which we ought to form, if theſe organs were not diſtinguiſhed by a character on which depend, in part, the aſtoniſhing properties of the torpedo. The tubes in the common rays open on the outſide of the ſkin by orifices peculiar to them, and are ſo many excretory organs of the gelatinous matter which they contain. In the torpedo all theſe tubes are completely ſhut, not only by the ſkin, which has no perforations, but alſo by aponeuroſes which extend over the whole ſurface of the electric organ. As the gelatinous matter cannot then eſcape, it is forced to be accumulated in theſe tubes: hence, no doubt, the greatneſs of their diameter, and on this account alſo their number increaſes at the different periods of life. Walſt and Hunter, indeed, found this progreſſive augmentation. They counted more than two hundred of theſe tubes in young ſubjects, from four to five hundred in adult torpedoes, and even twelve hundred in an individual of a large ſize.

It is to John Hunter, as already ſaid, that we are indebted for the beſt deſcription of the electric organs of the torpedo*. Monro, in his Phyſiology of Fiſhes, has alſo deſcribed the correſponding apparatus found in the other rays; but I flatter myſelf that I am the firſt who compared theſe organs, who proved their identity, and reduced them to the ſame ſyſtem of organization. The electric organ of the torpedo is really an organ of touching, furniſhed with an apparatus as extenſive as that of ſeeing or ſmelling. The nerves which proceed thither are ſo large that their volume appeared to Hunter as extraordinary as the phænomena to which they give riſe. They ſuddenly expand in a gelatinous mucus, and nothing impedes their free communication with external bodies.

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* Philoſophical Tranſactions 1773, p. 481.

There can be no doubt that they perform a very conſiderable part in the electric phænomena. Hunter was of opinion that they are deſtined to form, collect, and direct the nervous fluid. Their influence however is proved, ſince it is known that the concurrence of the will of the animal is indiſpenſably neceſſary for giving ſhocks. This evidently reſults from the obſervations of M. Valſt, and from thoſe which I had occaſion to repeat myſelf.

However, ſince theſe nerves are found in other rays diſtributed nearly in the ſame manner as in the torpedo, it muſt be allowed that they are not alone ſufficient for the production of electricity, and that for this purpoſe they muſt alſo be in a certain relation with the ſurrounding parts. Do the apertures of the tubes in the rays favour the iſſue of the nervous fluid? Or, as in the torpedo, do the nerves require a larger quantity of gelatinous ſubſtance to enable them to expand in numerous rami, and to become proper for acting with more energy?

In order that we may aſcertain how far theſe conjectures may be founded in truth, let us compare the electric organs of the torpedo, thoſe deſcribed by Hunter in the *gymnotus electricus*, and thoſe which I diſcovered in the *ſilurus electricus*. Theſe two fiſhes are ſo different from the torpedo, that they afford reaſon to hope that this compariſon will throw conſiderable light on the preſent queſtion.

The gymnotus belongs to that order of fiſhes diſtinguiſhed by the name of *apoda:* it is the genus which approaches neareſt to that of the murenæ and eels; conſequently they have a very long body, almoſt cylindric, and ſimilar to that of ſerpents. Separated from the murenæ becauſe they have no fins on the back or tail, they are diſtinguiſhed from them alſo by the great ſhortneſs of the abdomen. The anus, indeed, is ſo near the head that it opens before the pectoral fins. But, on the other hand, the gymnoti have the tail of a moſt extraordinary length: it is an organ to which all the reſt ſeem to have been ſacrificed, and it is rendered lighter by a diſpoſition peculiar to this kind of fiſh. The air-bladder, inſtead of being incloſed in the abdominal cavity, extends to the inſide of the tail, and is continued to its extremity. It is above this bladder that there is found in the gymnotus electricus a very ſingular apparatus ſtill more aſtoniſhing by its enormous ſize than by its ſtructure; an apparatus of which there is no veſtige in the other ſpecies of this genus, and which may be eaſily known to be the electric organ of the gymnotus.

This organ is formed by the union of a very great number of aponeuroſes, which extend in the direction of the length of the fiſh, forming ſo many horizontal ſtrata, parallel, and diſtant from each other about a millimetre: other vertical laminæ, of the ſame nature, and much more numerous, interſect them almoſt at right angles; which produces a large profound reticulation compoſed of numerous cells of rhomboidal planes. The interior of theſe cells is filled with an unctuous ſubſtance of a gelatinous appearance.

The electric batteries are divided into four diſtinct maſſes, two of which are large and two ſmall. Hunter gives to each pair the name of large and ſmall electric organs.

The large organs are ſituated immediately below the air-bladder and the vertebral muſcles. They are of ſuch a thickneſs that they form alone more than half the volume of the tail. They are divided by a broad partition, which furniſhes them with points, to which they are ſtrongly attached. They adhere ſuperiorly to the air-bladder, and to the muſcles which accompany it, by a very compact cellular tiſſue, and they terminate inferiorly in a round edge towards the origin of the oſſeous pieces which ſupport the radii of the fin of the anus.

The ſmall organs occupy the lower region of the tail. They begin and end nearly at the ſame points as the large organs; are ſituated below them, and on each ſide of the oſſeous ſupporters of the anal fin: their general form is that of two long triangular pyramids. The two lateral faces of theſe pyramids are covered by muſcular fibres, the aggregate of which produces the different motions of the fin. In this reſpect theſe ſmall organs differ eſſentially from the large ones which are attached immediately to the ſkin, and which, having a more intimate connection with external bodies, are capable of producing more energetic effects.

The horizontal laminæ of the ſmall organs, inſtead of being parallel throughout their whole length, are undulated at intervals. Hunter counted 24 in a large organ, and 14 in a ſmall one. The laminæ which interſect the latter at right angles are far more numerous: they are ſo thin, and ſo near each other, that 240 are found in the extent of 9 inches.

This apparatus of electric organs is put in play by a ſyſtem of nerves furniſhed by the ſpinal marrow, and diſtributed with admirable mechaniſm. Above the vertebral column is found a large nerve, which proceeds in a ſtraight line from the cranium to the extremity of the tail; but though larger, and nearer the vertebral column, in the gymnotus electricus than in other fiſhes of the eel kind, it gives out only a few rami, which proceed to the electric organs. On the other hand, however, there iſſues from each vertebra a nerve, which not only diſtributes branches to the muſcles of the tail, but ſends ſome alſo to the electric organs. The different rami of this nerve creep firſt over the ſurfaces of theſe organs, and ſpread and expand in their alveoli. It is thus that the vertebral nerves, by means of this deviation from their uſual route, and of an augmentation of volume, become in the gymnotus electricus ſo many inſtruments capable of ſtriking with death, or at leaſt with torpor, all thoſe animals which they touch.

The electric organ of this fiſh being placed under the tail, and in a part which conſequently is removed from the principal ſyſtems of life, it was difficult to conceive what were the nerves which could proceed thither. The ſimpleſt combination was to deduce them immediately from the ſpinal marrow; and this, indeed, is that which we ſee realized in the gymnotus electricus.

The ſimplicity of the means employed by nature is obſerved in a manner ſtill more particular in the ſilurus electricus. This fiſh differs almoſt as much from the gymnoti as from the rays, and therefore we muſt expect a quite different organization.

It is, indeed, neither on the ſides of the head, as in the torpedo, nor below the tail, as in the ſpecies we have deſcribed, that the electric organ of the ſilurus electricus is found. It is extended all round the animal; it exiſts immediately below the ſkin, and is formed by a conſiderable collection of cellular tiſſue, ſo thick and compact, that on the firſt view it might be taken for a ſtratum of lard: but when cloſely inſpected it is obſerved that this organ is compoſed of real tendinous or aponeurotic fibres interwoven with each other, and which by their different croſſings form a reticulation the meſhes of which are not diſtinctly viſible without the help of a magnifying glaſs. The ſmall cells or alveoli of this reticulation are filled with an albumino-gelatinous matter. They are prevented from communicating in the inſide by a very ſtrong aponeuroſis, which extends over the whole electric reticulation, and which adheres to it ſo cloſely that it cannot be ſeparated without tearing it. This aponeuroſis in other reſpects adheres to the muſcles only by a thin cellular tiſſue of little conſiſtence.

The nervous ſyſtem, which completes this electric organ, has no more relation with the nervous branches which we examined in the torpedo and the gymnotus than the tubes of the latter have with the peculiar covering of the ſilurus electricus. Theſe nerves proceed from the brain; they are the ſame as thoſe which my friend Cuvier has obſerved in all fiſhes to proceed under the lateral line: but theſe two nerves of the eighth pair in the ſilurus electricus have a direction and volume which are peculiar to that ſpecies: they deſcend, approaching each other on their iſſuing from the cranium towards the body of the firſt vertebra, which they traverſe. They firſt introduce themſelves through an orifice peculiar to each of them, and then iſſue on the oppoſite ſide by one aperture: after reaſcending they ſuddenly ſeparate, and proceed under each of the lateral lines. They are then found lodged between the abdominal muſcles and the general aponeuroſis which extends over the electric reticulation. In the laſt place, they penetrate beneath the ſkin by means of large branches, which proceed to the right and left of the principal nerve. Theſe branches are in number 12 or 15 on each ſide; they pierce the aponeuroſis which lines the interior ſurface of the reticular tiſſue, penetrate to the centre of the reticulation, and at laſt expand in it.

The examination of the three electric organs, which I have compared with each other, neceſſarily conducts us to ſome intereſting reſults reſpecting the kind of modification which organs common to all fiſhes ought to undergo to develop in ſome ſpecies electric properties. We find, 1ſt, That the part where the electric batteries are lodged is a matter of indifference, as they are diffuſed all around the ſilurus electricus, collected in the tail of the gymnotus, and united on the ſides of the head in the torpedo. 2d, That no branch of the nervous ſyſtem is particularly ſet apart for theſe organs, ſince the nerves diſtributed thither are all different. 3d, That the form of the cells is alſo of little importance, as this form varies in each ſpecies; but in other reſpects it is found alſo that the electric batteries, which on the firſt view we might be tempted to believe to be ſo different, have however a great many relations with each other, and may be reduced to the ſame ſyſtem of organization. This will appear evident, when it is conſidered that the electric fiſhes are the only ones in which we find aponeuroſes ſo extenſive and ſo multiplied in their ſurfaces, with ſo conſiderable an accumulation of gelatine and albumen in the cells formed by theſe aponeuroſes, and nervous rami ſo large and of ſuch a length. It is by the union, indeed, of theſe ſimple inſtruments that the electric organ is conſtituted; and in this ſtate, according to the judicious remark of my colleague Lacepede [*], it may be compared to the Leyden flaſk, or an electric picture, ſince it is alternately compoſed of bodies which conduct the electric fluid (the nerves, and the albumino-gelatinous pulp to which the action of the nerves is continued), and of non-conducting bodies, ſuch as the aponeurotic laminæ, extended through this maſs of albumen and gelatine. What proves that it is on the mechanical arrangement of theſe idio-electric and non-electric elements that the properties of the torpedo depend, is the exiſtence of the ſame parts in other rays, though theſe fiſhes are not capable of producing the ſame effects. Theſe parts, ſimilar in regard to their intimate nature and texture, are diſpoſed in a manner entirely different. The nerve of the fifth pair in the rays and ſquali is of a conſiderable volume, and expands in a medium from which flows a great quantity of albumino-gelatinous ſeroſity: but this gelatine either is loſt on the outſide by tubes which open without the ſkin, or is accumulated in a maſs on the ſides of the bones of the noſe. In the latter caſe, the gelatine, whatever be its quantity, is of no uſe for the production of electricity. This, no doubt, muſt be aſcribed to the want of aponeuroſes, which divide it into ſmall inſulated portions—in the ſame manner as the Leyden flaſk, or the electric picture, would fail of their effect if deprived of the glaſs laminæ interpoſed between the metallic coatings.

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[*] *Hiſtoire Naturelle des Poiſſons*, vol. ii. Deſcription of the *Gymnotus electricus*, p. 156.

The electric organ, being formed of nerves and aponeurotic laminæ, interlarded, if I may uſe the expreſſion, with albumen and gelatine, we ought not to be aſtoniſhed at meeting with it in families altogether different. All animals have nerves which are loſt under the ſkin; all thoſe immediately below it are more or leſs provided with cellular tiſſue: all then have, in ſome meaſure, the rudiments of an electric organ. If we now ſuppoſe that nouriſhing veſſels depoſit albumen and gelatine between the leaves of the cellular tiſſue which fixes the ſkin to the exterior muſcles, we ſhall eaſily form an idea of the manner in which this depoſition may give riſe to the exiſtence of an electric organ. All this may take place without the influence, at leaſt in an immediate manner, of the other organs eſſential to life. It is a development which takes place almoſt without the animal, and which has no action but on the ſkin and parts which depend on it; and hence the reaſon why ſpecies which exhibit alone a development ſo extraordinary belong, however, to a numerous genus without preſenting any ſtriking anomaly.

I have thought it neceſſary, for the benefit of naturaliſts who apply to the ſtudy of natural relations, to inſiſt on this remark; and I ſhall terminate this memoir by another, which in my opinion muſt prove intereſting to the learned.

I have reaſon to believe that the Arabs, at the period no doubt when they cultivated the ſciences with ſo much ſucceſs,

ceſs, had approached nearly to the theory of electricity; it is at leaſt probable that they referred to the ſame cauſe the fulminating effects of the torpedo, and thoſe, much more terrible, of celeſtial electricity. We can form no opinion of their knowledge in natural hiſtory but by the names which they have given to moſt of the productions of nature. Theſe names have been preſerved without alteration; for it is only to the æra when the ſciences flouriſhed in the Eaſt that we muſt refer the rational nomenclature ſtill employed by the rude inhabitants of Egypt. Every animal in that country, as well as in books of natural hiſtory, has two names, that of the genus and that of the ſpecies. There is no exception but in favour of the torpedo and the ſilurus electricus. Every thing which related to the form of theſe fiſhes has been neglected, and nothing has been attended to but their aſtoniſhing faculty of ſtriking with torpor all the ſmall marine and freſh water fiſhes. Though very different, they have been diſtinguiſhed by the ſame name; and, what is very remarkable, this name, *râad* or *raaſch*, is that uſed to expreſs thunder. In giving this denomination to the torpedo and the ſilurus electricus, did the Arabs think of referring the phænomena of animal electricity to celeſtial electricity?

Explanation of the Figures, Plate III.

Fig. 1. the torpedo (*Raïa torpedo*).

a, electric organ compoſed of tubes.

b, the upper ſkin turned back on the ſide to ſhow the electric organ.

Fig. 2. the red ray (*Raïa rubus*).

a, a, aponeurotic tubes which communicate on the outſide of the ſkin by peculiar orifices.

b, the ſkin of the flanks turned back on the ſide.

n, nerve of the fifth pair.

i, focus in which the nerve of the fifth pair expands, and from which proceed, in a radiated form, in ſeveral bundles, the tubes which open on the outſide of the ſkin.

Fig. 3. tranſverſe ſection of the electric eel, *gymnotus electricus*.

g, g, large electric organs.

p, p, ſmall electric organs.

v, air-bladder.

m, m, m, m, longitudinal muſcles.

o, vertebral column.

d, ſkin ſeen on the outſide.

c, anal fin.

FIG. 7.

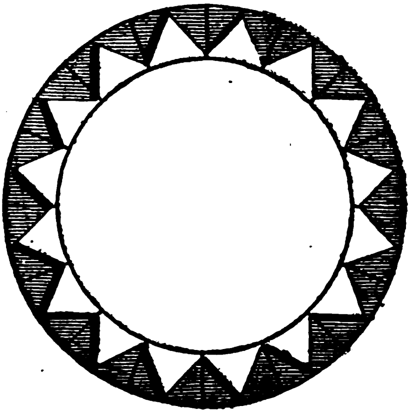


FIG. 8.

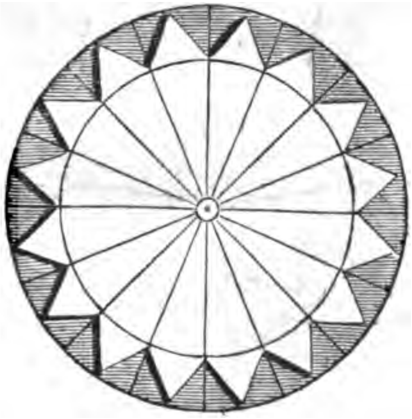
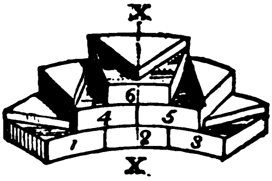


FIG. 9.



ment in Pennſylvania early in the 17th century. But a little conſideration has compelled me to relinquiſh this idea: for it is certain that the Indians were in poſſeſſion of this breed of dogs long before the arrival of the Swedes in America.

We have already ſeen that the Spaniards found domeſticated dogs among the Indians of Florida before the middle of the 16th century, almoſt one hundred years earlier than the Swediſh ſettlement in Pennſylvania. It is not, indeed, certain that theſe dogs were of the half-wolf breed; but it is probable that they were. In the year 1585 the celebrated navigator captain John Davis obſerved dogs "with pricked ears" in the lands about Hudſon's Bay*. It is highly probable that theſe dogs were a variety of the half-wolf breed of the Indians. Captain John Smith, who arrived in Virginia in 1607, a few years before the Swediſh ſettlement, expreſsly mentions the affinity of the Indian dogs in that country to wolves. I might eaſily adduce other inſtances. Theſe are ſufficient for my purpoſe. They evidently ſhow that the Indian dog exiſted in America before the Swedes planted their firſt colony in Pennſylvania: conſequently, we have no difficulty in anſwering Mr. Bartram's firſt queſtion, which I have ſtated.

Still, however, Kalm's obſervation is intereſting. It leads us to ſuſpect that the dog of the Indians is common to North America and to the northern parts of Europe. Neither ſhould this circumſtance, in whatever light we may view the original of the Indian dog, excite our ſurpriſe. If, as very many of the traders and others ſuppoſe, this dog is the produce of the wolf and the common fox, his parents exiſted in the old as well as in the new world. He may have been formed in America by the union of theſe two animals; or he may have migrated into America from Europe along with many other animals, which, it is highly probable, owe their original to that portion of the globe, or to Aſia. For that America has received ſome of its animals (beſide its human inhabitants) from Aſia and from Europe, I have very little doubt†.

I conjecture it will be found that the dog of the Greenlanders, mentioned by Crantz ‡ and other writers, is only a variety of the Indian dog. And, perhaps, the dogs of the Kalmuc Tartars, which are ſaid to have a great reſemblance to the jackal, or ſchakal, will alſo prove to be of the ſame breed. But with me theſe muſt remain, for ſome time, mere conjectures. For I am incapable of giving ſuch a minute deſcription of the external and internal appearance of the Indian dog as would enable the naturaliſts of Europe to decide a queſtion not the leaſt curious in the zoology of the new world. In another work I hope to be able to give a much more complete account of this animal than that which I now communicate to the public.

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* Forſter.

† See New Views, &c. Preliminary Diſcourſe, p. 101, 102.

‡ The Hiſtory of Greenland, &c. vol. i. p. 74. Engliſh Tranſlation, London 1767. "The Greenlanders (ſays this author) have no tame beaſts but dogs of a middle ſize, which look more like wolves than dogs. Moſt of them are white, yet there are ſome with thick black hair: they don't bark, but growl and howl ſo much the more."

I have ſaid that the Indian dog is a much more ſavage or unreclaimed animal than the common dog which has been introduced into America from Europe *. This circumſtance has not eſcaped the notice of ſome preceding writers. But I know of no writer who has deduced from it ſo important a concluſion as Mr. Zimmermann has done. This truly learned naturaliſt, after remarking that the Europeans who have viſited America have conſidered the Indian dogs merely as tamed wolves, proceeds to deduce the concluſion, that neither America itſelf, nor its inhabitants, are ſo antient as the countries or the people of other parts of the world †.

This concluſion proceeds upon the notion, ſo ingeniouſly defended by Mr. Zimmermann, that the dog of the old world is merely the wolf reduced to a ſtate of domeſtication, and varied, both as to his external aſpect and as to his eſſential qualities or manners, by the influence of climate and other phyſical cauſes, during the term of ſome thouſand years ‡. This ſubject is worthy of ſome of our attention. I regret, however, that in this place I can only touch it in the moſt ſuperficial manner. This I ſhall do under the following ſeven heads.

I. I may obſerve, in the firſt place, that it is by no means probable that the wolf is the ſole parent or original ſtock from whence have proceeded all the numerous varieties of animals which go under the general appellation of dogs. I think it much more probable that theſe varieties are derived from ſeveral different ſtocks or ſources beſide the wolf; ſuch as the jackal, the hyæna, different kinds of foxes, &c. &c. In their inquiries into the genealogical hiſtory of the dog kind, naturaliſts, by aiming at ſimplicity, have only tended to involve the ſubject in confuſion. A comparative view of the internal ſtructure of the ſuppoſed parents of the dog kind, and the dogs, has been too much neglected. Some attention,

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* See page 7.

† Specimen, &c. p. 91.

‡ The whole of Mr. Zimmermann's inquiry into the origin, &c. of the dog kind, is well worthy of the attention of the naturaliſt. See Specimen, &c. § 3. p. 83—98.

however, has been paid to this ſubject. Eſſential differences between the ſtructure of the wolf and the dogs (I mean the common dogs of the old world, for I know not that any anatomiſt has hitherto inſpected the ſtructure of the Indian dog of America) have been diſcovered; differences ſo eſſential, that I think they forbid the idea that the wolf and the dog are one and the ſame ſpecies. I do not, however, deny that the pure unmixed wolf has, in ſome countries, been reduced to the domeſtic ſtate of the dog. But I think it more probable, that even thoſe dogs, which are moſt nearly allied to the wolf, are hybrids, begotten between this animal and ſome other ſpecies of the genus.

II. From their agreement in internal ſtructure, it is much more probable that the jackal or ſchakal (the canis aureus) is one of the principal original ſtocks of the dogs of the old world. Profeſſor Gueldenſtaedt has remarked, that the *cœcum* of the jackal "entirely agrees in form with that of a dog, and differs from that of the wolf and fox." "I may add (ſays Mr. Pennant, whoſe words I have been uſing), that there is the ſame agreement in the teeth with thoſe of a dog, and the ſame variation in them from thoſe of the two other animals *." Moreover, in his manners the common dog is much more allied to the jackal than he is to the wolf, or to any other animal with which we are acquainted. If then this animal, and not the wolf, be the principal parent of the dog kind, the ſpeculations of Mr. Zimmermann ought to have little weight in eſtabliſhing the poſition, that the continents of America are a new creation, and their inhabitants new poſſeſſors of the ſoil. For, I think the form of the Indian dog is very conſiderably remote from that of the jackal, which is not known to exiſt in any part of America.

III. In order completely to eſtabliſh his opinion Mr. Zimmermann ſhould have proved that the dog is certainly derived from the wolf. I have juſt endeavoured to render it probable that the wolf is not the parent of the dogs of the old world; and I formerly† gave ſome reaſons for believing that the Indian dog of America, notwithſtanding the conjectures of Lawſon, and other writers, is not the pure unmixed wolf, but an hybrid, begotten between this and ſome other animal.

IV. But in the old world there are dogs not perhaps more completely domeſticated, or, in other words, not more thoroughly deprived of their ſavage aſpect and manners, than are the dogs of North America. Such are the dogs of the Kalmuck Tartars. And who that attentively conſiders the hiſtory of the country in which the Kalmucks reſide will believe that that country is a new creation? Who will venture to conjecture that the Kalmucks themſelves are a new people? Moreover, Kalm's obſervation would lead us to believe that the Indian dogs are the ſame (and of courſe not more ſavage) as ſome of the dogs in the north of Europe.

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* Hiſtory of Quadrupeds, vol. i. p. 262.

† See p. 7.

V. Some animals are very eaſily brought into the domeſticated ſtate. Others are domeſticated with great difficulty. Perhaps there are ſome incapable of domeſtication. If the Indian dog be the offspring of the wolf and the fox, or any other animal, we ought not, perhaps, to wonder that he is ſtill more an *animal ſylveſtre* than the generality of the dogs of the old world; for both the wolf and the fox are with difficulty tamed. In this inquiry we ought alſo to remember that the maſter of the Indian dog is a ſavage. It may readily be conceived that this circumſtançe will influence the genius of our animal. Living in the woods, and too frequently badly treated by his maſter, the dog muſt often leave the huts of the Indians, and, perhaps, imbibe from his parents, in the woods, a new tincture of their aſpect and their manners. Even in our cultivated towns, how much do the manners of the dogs ſeem to depend upon the calling of their maſters! It is a fact, that the dogs of our frontier ſettlers have a much more ſavage aſpect than the dogs (the ſame variety) in the villages and populous towns.

VI. In America there were found ſome kinds of dogs which were not leſs domeſticated than the dogs of the old world. Such were the *alco* and the *itzcuintepotzotli*, of which I have already given ſome account. I think it very improbable that theſe two ſpecies or varieties were derived from the wolf. Nor is it certain that they were not a ſpecies of canis eſſentially diſtinct from thoſe of the old world. In whatever light we view them, they ſeem to oppoſe an objection to Mr. Zimmermann's notion concerning the recent creation of America, and the recent population of this great portion of the globe. Could it be proved that the *alco* and the *itzcuintepotzotli* have ſprung from the wolf, it would be natural to infer that an immenſe period of time had elapſed before theſe animals could have been brought into the mild, domeſticated ſtate in which the diſcoverers of America found them.

VII. and laſtly. This is not the place to inquire into the period of the population of America. I have touched upon this queſtion in another work [*], and ſhall examine it more fully in a work in which I have long been engaged. Here, however, I may obſerve, that many circumſtances forbid the idea that America is a new creation, recently emerged from the influence of the ocean. And circumſtances, impreſſive in their nature, render it extremely probable that many of the nations of America have reſided in this portion of the world for ſome thouſand years. Trying them by their languages, the Americans will appear to be children of the earlieſt human families of which hiſtory or the traditions of mankind have preſerved any memorials.

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[*] New Views, &c. Preliminary Diſcourſe, p. 104—1[illegible].

Among the almoſt innumerable charges which have been brought againſt the Indian inhabitants of America, there is one which it becomes the hiſtorian of Indian dogs to take ſome notice of. The Indians are accuſed of great ſeverity or cruelty in the treatment of their dogs. Mr. Lawſon ſays the ſavages are the "worſt dog-maſters in the world, ſo that it is an infallible cure for ſore eyes ever to ſee an Indian's dog fat [*]." I have already made mention of this reſpectable traveller's notion of the proceſs by which he ſuppoſed wolves are turned into dogs [†]. The faithful father Charlevoix ſays, the Indians feed their dogs "but poorly, and never fondle them [‡]." Carver, who ſo frequently borrows from Charlevoix, ſays nearly the ſame thing [§]. It is even ſaid, that, owing to their ſcanty allowance of food, the Indian dogs are often ſo weak, that they are obliged to lean againſt a tree, or ſome other prop, whilſt they bark.

It is well known how much ingenuity, eloquence, and ſcience, have, within the laſt fifty years, been employed to repreſent the Americans as the degenerated, or imperfectly organized, children of the earth. To complete the large volume of calumny againſt theſe poor people, even the manner in which they treat their dogs is not ſuffered to paſs unnoticed by the hiſtorians of the new world. "Prior to their intercourſe with the people of Europe," ſays the eloquent Dr. Robertſon, "the North Americans had ſome [||] tame dogs, which accompanied them in their hunting excurſions, and ſerved them with all the ardour and fidelity [¶] peculiar to the ſpecies. But, inſtead of that fond attachment which the hunter naturally feels towards thoſe uſeful companions of his toils, they requite their ſervices with neglect, ſeldom feed and never careſs them [*]."

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[*] A New Voyage, &c. p. 38. [†] See p. 7.

[‡] A Voyage to North America, &c. vol. i. p. 79.

[§] Travels, &c. p. 416.

[||] They had many tame dogs. The liberality with which the Indians ſupplied Soto's men, and the facility with which the men ſupplied themſelves with dogs, leave us no room to doubt that tame dogs abounded in Florida.

[¶] Their fidelity has been called in queſtion. See p. 7.

It would, I believe, be a much eaſier taſk to prove that Dr. Robertſon was unqualified to write the hiſtory of America; to prove that the Indian Americans are not the inferiors of the people of the old world in the meaſure of their intellectual endowments; and to ſhow that more than one-half of the charges which have been brought againſt theſe people are charges reſulting from ignorance or from ſyſtematic zeal, than to prove that the Indians are peculiarly entitled to the character of kind and tender dog-maſters. After ſome attention to this ſubject, I muſt candidly confeſs that I poſſeſs not materials for a ſatisfying defence of the Indian. The charges which have been brought againſt him by the writers whom I have mentioned will be convictive. But why, in this inquiry, if the hiſtorian will condeſcend to mention the fact, and to interweave it with his eloquence, ſhould he forget the hardſhips of the ſavage life? Where the maſter labours under a ſcarcity of food, his ſervants, the animals which depend upon him for their ſubſiſtence, muſt ſhare in the hardſhips and the evils of his ſtate. The miſerable condition of the Indian dogs is a neceſſary reſult of the miſerable condition of the Indians themſelves. This is certain; though the Indians tell us that they keep their dogs poor that they may be light and nimble, and therefore the better fitted for the purpoſes of hunting.

Dr. Robertſon, however, might have found, in the writings of ſome of the authors whom he has repeatedly quoted, mention made of the tenderneſs which the Indians manifeſted towards their dogs in ſome parts of America. The following paſſage in Acoſta ſhould not have eſcaped the hiſtorian's notice. Speaking of the *alco*, the learned jeſuit ſays: "The Indians doe ſo love theſe little dogges that they will ſpare their meate to feede them, ſo as when they travell in the countrie they carrie them with them upon their ſhoulders or in their boſomes, and when they are ſicke they keepe them with them, without any uſe, but only for company [†]." Hence it appears, that of one ſpecies or variety of their dogs, the Indians, in ſome parts of the new world, were peculiarly careful, and even ſolicitouſly tender.

The Wunaumeeh Indians call the dog *allum*, *al-loom*, *mo-e-kan-neh*, and *mé-kan-ne:* the Monſees, *al-lúm:* the Mahicans, *dee-a-oo*, *de-a-oo*, and *an-nun-neen-dee-a-oo:* the Chippewas, *a-lim*, *anu-moſch*: the Meſſiſaugers, *an-nee-*

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[*] The Hiſtory of America, vol. ii. p. 216, 217: London 1788.

[†] The Naturall and Morall Hiſtorie, p. 301, 302.

mooſh: the Ottawas, *an-nee-moo-kau-che:* the Indians of Penobſcot and St. John's, *allomooſe:* the Natics, *anum:* the Narraganſets, *alum:* the Miamis, *aul-la-mo:* the Wiahtanah, *lemah?* the Pottawatameh, *an-ne-mooſh:* the Shawnees, *wiſſi, wee-ſeh:* the Kaſkaſkias, *remoah:* the Nanticokes, *alum*, and *ihnwállum:* the Mohawks, *ahgarijoo? er-har?* the Cochnewagoes, *er-har:* the Oneidas, *er-har, ale-haul, ale-hall:* the Onondagos, *tſchierha:* the Cayugas, *ſo-waus:* the Senecas, *chee-aah*, and *che-eh:* the Tuſcaroras, *cheeth, cheetht:* the Wyandots, *nee-a-nooh:* the Sioux, *ſhúngau, chonga, ſhun-guſh:* the Oſages, *ſhong-eh:* the Cheerake, *keera, keethlah, keethlegth:* the Creeks, *ee-ſa, e-ſa, ef-ſa:* the Chikkaſah, *o-phe, oo-phe:* the Choktah, *o-phe:* the Katahba, *taunt-ſee, taunſee, taſe:* the Woccons, *taub-he:* the Natchez, *worſe:* the Mexicans, *chichi:* the Poconchi, *tſi:* and the Chileſe, *tewa.*

If the affinity between the Wunaumeeh words, *mo-e-kan-neh, mê-kan-ne*, and the Latin *canis*, the Italian *cane*, the Neapolitan *cane*, be not accidental, and who, attentively conſidering the very many affinities that ſubſiſt between the languages of the old and new world, will imagine that it is accidental?), we have probably arrived at the knowledge of the real meaning of the Latin word *canis*. *Mékanne*, in the language of the Delawares, ſignifies "the barking beaſt." It appears from Dr. Pallas's great work (*Vocabularia Comparativa*) that certain tribes of Semoyads call a dog *kanang, kának*, and *konak*; and that the Karaſſini call it *kannak.*

XXIV. *A ſhort Account of the Improvement in Circular Architecture made by Colonel* TATHAM.

FOR this invention, which appears to be ingenious, and applicable to many important uſes, a patent has been taken out by Meſſrs. John Scott, James Clarkſon, William Tatham, and Samuel Melliſh. The invention conſiſts in the form given to the bricks or ſtones to be employed in circular buildings, which is ſuch as to make them lock, as it were, into each other, and that ſo effectually as to preclude the poſſibility of their parting joint by any accident whatever when laid down properly.

Two of the vertical ſides of each piece employed in ſuch circular ſtructures anſwer to two radii from a common centre; a circle from the ſame centre forms the third vertical ſide; and the fourth ſide is formed by another circle of a radius as much ſhorter than the laſt, whatever that may be, as the thickneſs intended for the wall: they are, in fact, wedge-formed. This is ſo preciſely the caſe, that where the pieces (to which the patentees have given the name of *Tatham's clumps*) are intended to form ſolid pillars, or the like, they exhibit only the three firſt-mentioned vertical ſides.

The horizontal, or under and upper ſides of the clumps, have a kind of ſhouldering or protruding part, and a correſponding hollow or depreſſed part to fit the ſimilar parts of the next courſe, and theſe are at the ſame time ſo adjuſted that each courſe will break joint with the one before laid; that is, the vertical joinings of every row fall on the middle of the clumps which form the one below and the one above it.

The form given to the ſhoulderings or joggelings is ſo ingeniouſly contrived, that, were a cylinder formed of theſe clumps, with the two extreme circles tied together by means of long bolts paſſing up the centre, ſecured by ſcrews or other faſtenings, it might be laid upon its ſide and employed as a garden roller, without the poſſibility of any one of the clumps falling out of its place.

It is eaſy to ſee that this contrivance is applicable to a variety of purpoſes: as,

1ſt, In rural architecture, &c. For example, in cottages, country houſes, villas, circular barns; granaries, ſecure from rats, mice, and other vermin; feeding houſes, cattle ſheds, ſummer houſes, caſtles, towers, turrets, battlements, paliſadoe work, door ſteps, ſtair-caſes (ſecure from fire), pleaſure houſes on eminences and near the ſea, bathing houſes, &c.

2dly, In ſolid work; as columns, colonnades, agricultural rollers, rollers for roads, garden rollers, ſtone or brick piers in water works, &c.

3dly, To hydraulic and ſubterraneous architecture, for ſewers, culverts, tunnels, conduits; cylinders in bridge work, thereby giving ſtrength, cheapneſs, waterway, and lightneſs, to the ſtructure; mills, reſervoirs, communications, wells, ceſspools, and the like.

4thly, In marine architecture, to dock work, baſins, harbours, piers, jetties, light-houſes.

5thly, In military architecture, to magazines, block-houſes, redoubts, covert ways, lines of communication, watch towers, &c.

6thly, In architecture applying to warehouſes and manufactures; for example, circular windows and other apertures admitting light and air, brewers' vaults, boilers, &c. &c.

7thly, Church architecture; for example, circular chapels,

FIG. 1.

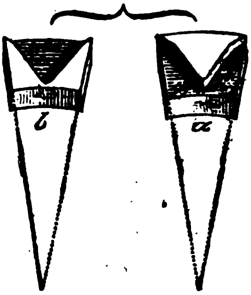


FIG. 2.

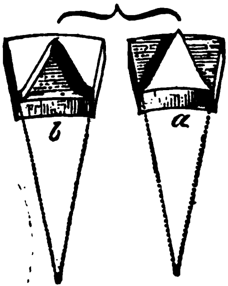


FIG. 3.

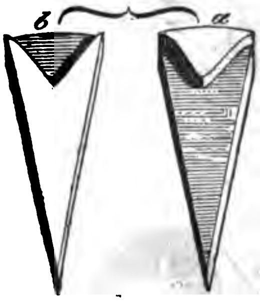
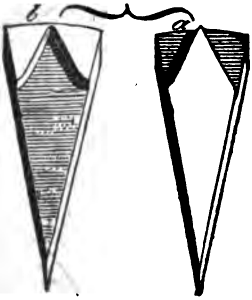


FIG. 4.



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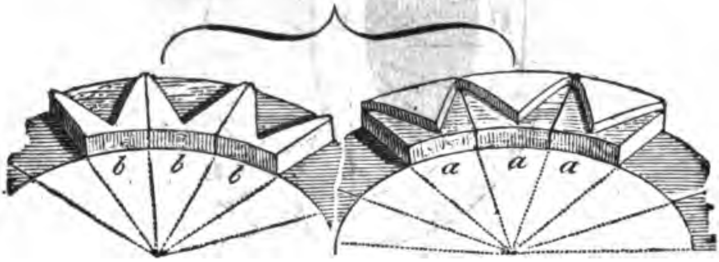


FIG. 6.

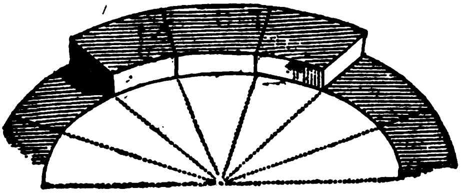
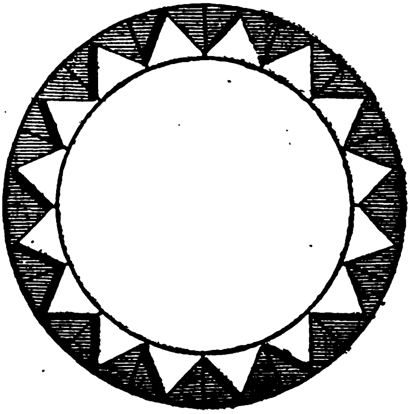


FIG. 7.



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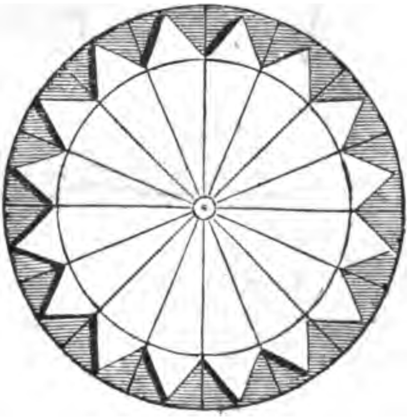


FIG. 9.

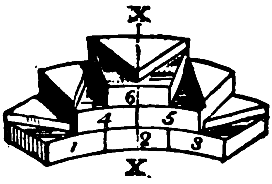


FIG. 10.

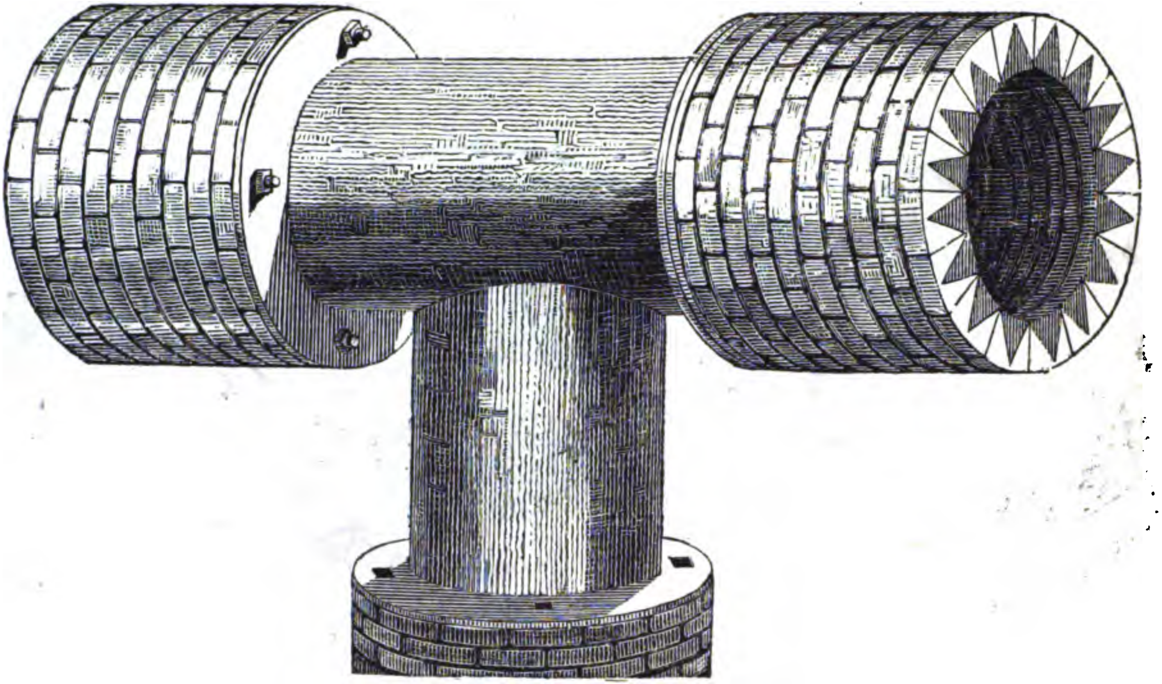


FIG. 11.

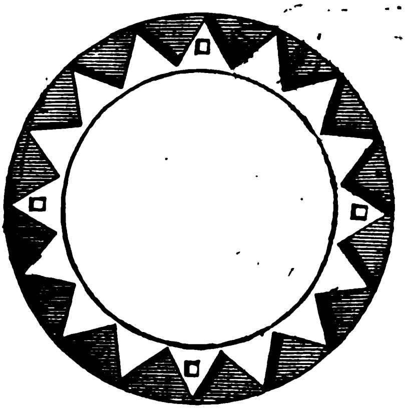


FIG 12.

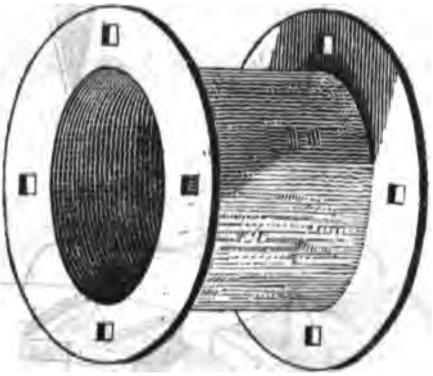
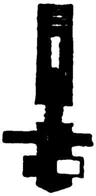


FIG. 13.



pels, ſteeples, columns to ſupport ponderous roofs, the hollow ones with ſtair-caſes when wanted, niches, cells, cloiſters, vaults, &c.

Explanation.

Fig. 1. *a* is a ſingle clump, having its male point inwards on the ſide in view; *b* is one with the female point inwards on the reverſe of the ſame clump.

Fig. 2. *a*, one with the male point outwards; *b*, one with the female point outwards. Each of theſe figures, 1 and 2, repreſent different kinds of clumps, which being laid, one kind at a time, in a circular form, with the ſame ſides uppermoſt, will make one courſe: ſee fig. 5. for an example of one courſe of clumps, fig. 1, *a*. Then add one courſe of fig. 2, fitted in male and female (or joggled); and ſo on alternate circles, fig. 1 and fig. 2, to the end of the length deſigned, terminating the ſame with a caſt iron plate, flaunch piece, or key clump, with two male ſides ſo as to admit of taking down for repairs, &c. This method will form a broken joint, ſuch as the one exhibited in perſpective view fig. 9.

Fig. 3. is an elongated clump for forming ſolid columns, colonnades, &c.; *a* having its male point inwards, and *b* having its female point inwards.

Fig. 4. a clump elongated as in fig. 3. *a* ſhows the view with the male point outwards: *b* the reverſe of the ſame clump, with the female point outwards. Theſe clumps are to be laid into each other in the ſame manner as fig. 1 and 2, to form a column of ſolid work, of which a ſection is repreſented in fig. 8. If it is intended to conſtruct a circular ſtair-caſe with dome-light, &c. on this principle, one or more clumps in each courſe of work (be the ſame of brick, earth, ſtone, or other material, according to the deſign and extent,) muſt be of the long kind, to form the *ſtep* of the ſtairs; all the reſt of ſhort clumps, fig. 1 and 2, to admit light and acceſs through the aperture of a ſpiral aſcent.

Fig. 5. *a*, *a*, *a*, ſhows the poſition of clumps *a*, fig. 1, according to the mode of laying them, with the male point inwards: *b*, *b*, *b*, ſhows the reverſe of the ſame clump with the female point inward. The bottom circle in this and the next figure ſhows merely the poſition which a continuation of the clumps would take.

Fig. 6. clumps with blank ſides uppermoſt: theſe are merely for laying the firſt courſe on the ground, or, in ſome inſtances, for ſaving the expenſe of a caſt iron rim next the flaunch of a branch piece. Their reverſe muſt be always indented, or joggled, to fit the next courſe of clumps. If it is

required

required to form a fluſh edge, ſuch as the cheek of a window or door, it can be done by cutting down the clumps into half clumps, as repreſented by the dotted line X, X, fig. 9. Raviels, rabbits, grooves, &c., and returns of corners, elliptics, &c. muſt be ſpecifically moulded on the ſame principle; or girders, ties, &c., may ſecure any variation by means of ſuitable caſt iron clamps, modified according to the particular contingency.

Fig. 7. a ſection of a well or pipe, &c. the ſurface of the circle in view being put together with clumps fig. 2, the male ſide *a* pointing outwards.

Fig. 8. ſhows the plan of a ſolid column formed of elongated clumps of the kind *a*, fig. 4, the male points outwards being in view. As this ſame modification contracts the circle to a ſmaller column, in that caſe fewer pieces will be required.

Fig. 9. is a perſpective view, ſhowing a ſegment of the work (part of a circle) formed out of ſix clumps laid in their order from figures 1 and 2, the bottom courſe having a blank ſide as deſcribed fig. 6.

Fig. 10. part of a clump main pipe, ſhowing the manner of flaunching in a caſt iron branch piece in order to join the ſame to a branch main. This can be performed with the help of an iron rim (joggled) as repreſented in fig. 11; or it can be fitted to blank ſides next the flaunch, and the oppoſite end of the bolt can be counter-ſunk into perforated clumps, having the prominent ends fitted with a male and female ſcrew, in order to ſlide the flaunch piece in or out as occaſion demands.

Fig. 11. a caſt iron rim indented or joggled to fit a courſe of clumps as repreſented on the ſurface in view, in order to admit the flaunch to ſlip in and out as required on its ſmooth ſurface.

Fig. 12. a plain caſt iron flaunch for joining a clump main in ſuitable lengths to be occaſionally taken down: they may alſo be laid in lengths by means of a key clump, having two male ſides to have the joints banded with a hoop.

Fig. 13. ſhows the mode of fitting the prominent point of the flaunch bolt by means of a male and female ſcrew, ſo that the flaunch piece may be ſlided into or out of its birth at option, without hanging on the flaunch plate or joins.

XXV. On

XXV. *On the Freezing Point of Tin, and the Boiling Point of Mercury; with a Deſcription of a Self-regiſtering Thermometer invented by Mr.* JAMES CRICHTON, *of Glaſgow* *.

SOME time ago, having made ſome high ranged thermometers, I wiſhed to try their correſpondence at high temperatures. I procured for this purpoſe 1½ pound of the beſt eſſayed tin (block), and, after ſuſpending two thermometers upon a ſliding ſupport, I melted the tin in a heat of from 20° to 30° above the melting point: I then lowered my thermometers down into the metal, and obſerved the mercury to ſink *gradually* in both till it arrived at 442°; then it *inſtantly* ſunk to 439°, and as inſtantaneouſly roſe to 442°. At this temperature the mercury remained perfectly ſtationary for five minutes, at which time the metal became ſolid to the centre of the crucible. After obſerving this fact, which to me was new, I waited upon Dr. Mickleham, and requeſted that he would be preſent when I ſhould repeat the experiment, to aſſiſt me in diſcovering the fallacy, if there was any. We then procured, from another work, the ſame quantity of tin as before, and repeated the experiment ſeveral times with the ſame reſult.

Another accurate ſtandard point, that of 442°, for adjuſting thermometers, has thus been aſcertained; a circumſtance which, judging by the uſe it will be of to myſelf in conſtructing high ranged thermometers, I think too important to be withheld from chemiſts and others who may wiſh to avail themſelves of it.

Before I quit the ſubject of mercurial thermometers I beg to ſtate another fact which may alſo be of uſe to the philoſophical world. Quickſilver does not boil at 600°, but at 655° at the loweſt. The preciſe point I have not yet found leiſure enough to aſcertain.

I ſhall now deſcribe a regiſtering thermometer fit for meteorological purpoſes, which I have lately invented and conſtructed, and which ſome of my friends have requeſted me to make public. Its action depends on the different degrees of expanſibility of different metals when expoſed to a change of temperature. (See Plate III.)

The length of the inſtrument is about 13 inches. Fig. 1. repreſents a front view of it, and fig. 2. a ſide view; the let-

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* Communicated by the Author.

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ters of reference are the ſame in both, where they can be applied.

The bar A, of which the ſide BC is of iron, and the other DE of zinc, is about one inch broad, a quarter of an inch thick, and eight inches in length. It is firmly fixed, at the lower extremity I, to a board *a*, *b*, *c*, *d*, made of mahogany.

On the application of heat, this bar, by the ſuperior expanſion of the zinc, is inclined at the top towards B. In the upper end of the bar there is a pin (ſeen at A, fig. 2.) which goes into the fork-like opening L of the index LM (fig. 1.), and carries the ſhort end of this index along with it; conſequently the long end is carried along the ſcale from 0° towards 100°.

When the inſtrument is expoſed to a lower temperature, the movement is exactly the reverſe of what I have been deſcribing.

Beneath the index LM are two others on the ſame axis G, one of which can be carried towards the right, and the other towards the left, by means of a pin which goes through the firſt-mentioned index at H. By this means, the greateſt and loweſt degree of temperature that occurs from the time the thermometer is ſet or adjuſted till it be again conſulted, is accurately marked.

In adjuſting the thermometer for obſervation, all that is neceſſary is to bring the two ſmall indexes immediately below the broad one. As the laſt-mentioned one is moved backwards or forwards by variations of temperature, it carries one or other of the ſmall ones along with it, by means of the pin H, leaving one of them at the higheſt and the other at the loweſt degree that may have occurred from the laſt time of adjuſtment.

The face of the upper part of the inſtrument is covered with a glaſs door, the hinges of which are at N and O.

The greateſt range of any thermometers of this kind which I have yet made, is from 10° below to 100° above 0°. This inſtrument proves very accurate in its indications.

XXVI. *Memoir on the Chemical Nature of Ants, and the ſimultaneous Exiſtence of two Vegetable Acids in theſe Inſects.* By A. F. Fourcroy *.

1ſt. So much has been already ſaid in regard to ants and their acid, that it appears nothing more remains to be added on the ſubject. Since the time that Samuel Fiſher firſt deſcribed this acid in 1670, and taught a method of obtaining it by diſtillation, till citizen Deyeux confirmed by exact experiments the identity of the acid of ants with the acetous acid, (an identity firſt announced in 1749 by Margraff, then in 1777 by Ardviſſon and Oëhrn, and afterwards by Bergman,) all that chemiſts have done on this ſubject ſeems merely to be, that ſome of them have maintained the ſuppoſed analogy; while others have wiſhed that this analogy ſhould be only apparent, and that the formic acid ſhould be conſidered as a peculiar acid *ſui generis.* I do not ſpeak here of the opinion of Thouvenel, who pretended that the acid of ants was the phoſphoric acid, becauſe this opinion was ſupported by no poſitive fact: I thought it might be ſtill uſeful to reſume the analyſis of ants; and the reader will find that, having been employed in this reſearch with C. Vauquelin, it preſented to us ſome facts which had eſcaped our predeceſſors.

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* From *Annales du Muſeum National*, No. 5.

2d. Having cleaned the ants which were collected in the wood of Boulogne, and of that kind called by Linnæus *formica rufa,* they were bruiſed in a marble mortar. During this operation a vapour was diſengaged of an odour ſo ſharp and pungent that it hurt the eyes, and could be compared to nothing but the acetic acid or radical vinegar.

3d. The ants thus bruiſed were put to macerate in alcohol for ſeveral days, and in a temperature of about 68 or 70 degrees the alcohol aſſumed a yellow colour.

4th. The alcoholic infuſion of ants ſubjected to diſtillation furniſhed an inflammable liquor ſlightly acid. During the diſtillation there was formed in the liquor a browniſh depoſit which was collected on a filter.

5th. The acid liquor which remained after diſtillation, when filtered and ſeparated from the depoſit, was ſaturated with lime. It then became brown, thick, and acquired a nauſeous and pungent ſavour. By blowing through a tube into the thick liquor there was formed a multitude of bubbles, which exhibited the ſame priſmatic colours as ſoap bubbles.

6th. This combination of the acid of ants with lime, when ſubjected to the teſt of different re-agents, exhibited the following phænomena:

a. Mixed with concentrated ſulphuric acid it exhaled the odour of vinegar.

b. With nitrate of lead it gave an abundant white precipitate.

c. With nitrate of ſilver a yellowiſh precipitate.

d. With muriate of barytes there was nothing ſenſible.

e. Ammonia produced no change in it.

f. Alcohol formed a ropy and ductile precipitate.

7th. A part of the thick liquor being mixed with half a part of concentrated ſulphuric acid and two parts and a half of water, there was immediately formed a very thick magma. This mixture was ſubjected to diſtillation, and the product was divided into three parts. They were all clear and colourleſs: they had an empyreumatic odour, and a very ſtrong acid taſte.

The reſiduum of this diſtillation had acquired a very dark brown colour, and a diſagreeable burnt taſte, though it had not been carried to dryneſs.

The firſt product tried with acetite of lead gave no ſigns of the preſence of the ſulphuric acid: the ſecond and third gave the ſame reſult; which proves that they contained no ſulphuric acid.

This acid diſtilled and combined with potaſh furniſhed real acetite of potaſh which attracted the moiſture of the air; exhaled, by the addition of concentrated ſulphuric acid, a ſtrong vapour like that of radical vinegar; and in a ſolution of nitrate of mercury formed a flaky precipitate like common acetite of potaſh.

The greater part then of the acid of ants is acetous acid, as C. Deyeux has already proved by a careful analyſis and experiments[*].

8th, The calcareous combination of the acid of ants, obtained by infuſing them in alcohol, exhibited to us a very remarkable character. In a ſolution of acetite of lead it formed a very abundant depoſit, which was rediſſolved by the acetous acid, and even by that which was obtained in the ſolution above deſcribed, No. 7.

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[*] Bergman had found in it ſome reſemblance to the acid of vinegar; the following is what he ſays on this ſubject:—Hoc acidum indole aceto proximè accedit, in variis tamen differunt. Prius cum magneſia, ferro et zinco cryſtalliſabiles præbet ſales, poſterius nonniſi deliqueſcentes. Magneſia formicata in primis notatu digna eſt. In another part of his works, *A Diſſertation on Magneſia*, Bergman ſpeaks alſo of the formic acid, and deſcribes the properties of the ſalt which it forms with magneſia as follows:—Acidum formicarum magneſiâ ſaturatum aquam deſerens fundum petit, abundante autem acido ſolvendum et evaporatione cryſtallós deponens, quæ ſaporis fere expertes ægerrime aquâ ſolvuntur, in igne non funduntur; ſed tamen, parum decrepitant, ſubito nigreſcunt, tandemque in pulverem fatiſcunt album, in acidis efferveſcentem quum acidum formicarum ſub deſtructione magneſiam præbeat aëream. Cryſtallorum figura eſt hemiſphærica, plano ſecante ſurſum verſo, paulum concavo, polito, ſtriiſque, oculo armato vix diſcernendis, è centro radiante: hinc luculenter aceto dignoſcitur acidum formicarum, cui alioquin indole proximè accedit, illi tamen, æquè ac acidis vitrioli nitrique phlogiſticatis, magneſiam eripiens hujus ſalis dimidium pondus eſt ex magneſia. Una ejuſdem pars tredecim requirit aquæ, ut ſolvatur in calore quindecim graduum; ſpiritus vini eumdem non ſuſcipit.

As this experiment ſhows that the acid taken from the ants by alcohol, and combined with lime, contained ſomething elſe than acetous acid, ſince this calcareous ſalt precipitated acetite of lead; and as this different matter was not volatile, ſince the product of the diſtillation with ſulphuric acid did not produce the ſame effect, we tried to find it again in the reſiduum of the diſtillation: but this reſiduum contained ſo much ſulphuric acid that it was impoſſible for us to ſucceed; we therefore had recourſe to the following experiment.

9th. A part of the combination of the formic acid with lime was mixed with a ſolution of nitrate of lead, and there was immediately formed a very abundant precipitate of a yellowiſh colour, which when put upon burning coals, after having been well waſhed and dried, became immediately black, exhaled an animal and ammoniacal odour, while the lead was reduced to a metallic ſtate.

Sulphuric acid diluted with about ſix parts of water was poured over this precipitate reduced to fine powder. At the moment of mixture the powder remained at the ſurface of the liquor as if it had been a fat ſubſtance, and it did not become mixed with ſulphuric acid till after long agitation.

When the mixture was well made the volume of the precipitate was diminiſhed: it became heavier and whiter. The liquor then had a ſlight acid and ſaccharine ſavour, which was deſtroyed by the addition of the ſulphuric acid, and was ſoon ſucceeded by a nauſeous ſavour. It precipitated ſlightly muriate of barytes, which indicates ſome traces of ſulphuric acid. It precipitated lime water only very weakly; but at the end of ſome hours there were formed a great many cryſtals at the ſurface of the liquor, and on the ſides of the glaſs containing the mixture.

It precipitated in abundance nitrate of mercury, and thoſe of ſilver and lead.

Combined with barytes it gave a reddiſh ſolution of a ſaline and pungent ſavour. This combination reduced to a ſmall volume did not cryſtallize; a ſlight pellicle only was formed at its ſurface. Potaſh did not ſeparate the barytes from this combination, but the alkaline carbonate produced a precipitation. The oxalic acid formed in it a very abundant depoſit, but the tartarous and citric acids produced in it no change.

10th. The facts here detailed indicate to us that the matter combined with lime along with the acetous acid was of the nature of the malic acid; but to be more certain we examined it comparatively with the latter, combining both of them with the ſame ſubſtances. Without here entering into a tireſome account of theſe comparative experiments, we ſhall content ourſelves with ſaying that theſe two compounds exhibited phænomena abſolutely ſimilar.

Ants contain then, and conſequently form, malic acid, like vegetables. It is no doubt the preſence of this acid which had miſled the chemiſts who preceded us in this labour. While they acknowledged in the acid of ants a great analogy with vinegar, they however found differences which induced them to conſider it as a particular acid; and theſe differences aroſe from the preſence of the malic acid in the acetous acid of ants.

Diſtillation of the Product of Ants treated with Alcohol.

11th. The ants, exhauſted by alcohol, furniſhed a limpid and ſlightly alkaline water, a reddiſh brown oil, thick and exceedingly fœtid. The firſt liquor, diluted with water, and filtered to ſeparate the oil, produced a ſlight effervеſcence with acids.

As it emitted at the ſame time an odour of vinegar, eaſily diſtinguiſhed amidſt the fœtidneſs by which it was accompanied, a certain quantity of it mixed with ſulphuric acid was diſtilled, and we obtained a colourleſs acid product of an empyreumatic odour, which contained a ſmall quantity of acetous acid. Was this acetous acid completely formed in the ants exhauſted by alcohol, or was it formed by the action of the fire?

The product of the diſtillation of the exhauſted ants contained then a fœtid empyreumatic oil, carbonate of ammonia, and acetite of ammonia, all diſſolved in a large maſs of water.

Examination of the Matter which ſeparated from the Alcohol during the Diſtillation mentioned in No. 4.

12th. The reader will remember that the alcoholic infuſion ſubjected to diſtillation ſuffered to be depoſited a brown ſubſtance which had been ſeparated from it by the filter. This ſubſtance was of ſo dark a red colour that when ſeen in a large maſs it appeared to be black: when dried it was brittle, its fracture was ſmooth and brilliant like that of reſin, it had no ſenſible ſavour, it was not ſoluble in water; and this may ſerve to explain its precipitation in proportion as the alcohol evaporated. Though inſoluble in water, when macerated a long time in that fluid it communicated to it a ſlight fawn colour, owing, in all probability, to a ſmall quantity of extractive matter interpoſed between its parts.

Alcohol ſlightly heated over this ſubſtance diſſolved the greater part of it. It aſſumed a pretty dark red colour: there however remained a browniſh matter, which did not combine with that liquid whatever quantity of it was added. This alcoholic ſolution became milky by the addition of water, and at the end of ſome days there was ſeparated a reſin-like depoſit ſoft and ropy, of a reddiſh colour and a very diſagreeable nauſeous taſte, ſlightly ſoluble in water, ſince it communicated to it a little of the colour and of the nauſeous taſte. This depoſit is a fat matter of a peculiar nature.

The browniſh matter not diſſolved by alcohol, already mentioned, appeared to us to be albumen, which the moiſture, and perhaps the acid contained in the ants, contributed to render ſoluble in alcohol. This albumen was coagulated by the heat, and precipitated with the fat matter in proportion as the alcohol was volatilized. When placed on burning coals it decrepitated, became corneous, ſwelled, and puffed up, emitting white fœtid ammoniacal fumes: it left on the ſupport a large and light charcoal. It was in no manner ſoluble in water; it contained a little hydrogenated carbon, which gave it a browniſh colour.

13th. A portion of the ants exhauſted by the action of alcohol was ſubjected to diſtillation in an open fire. There remained a charcoal, which, like all thoſe of animal ſubſtances, burnt with difficulty, and left, after long combuſtion, white aſhes, which contained nothing but phoſphate of lime.

This was proved by treating it with nitric acid, which diſſolved the greater part of it without effervescence, and by adding to the ſolution ammonia, which formed in it a white gelatinous depoſit which exhibited all the properties of phoſphate of lime.

The portion of aſhes which did not diſſolve in the acid was ſilex; but as the incineration had taken place in an earth crucible, this ſubſtance aroſe, in all probability, from that veſſel. However, to aſcertain whether this earth had been really furniſhed by the crucible, ſome of this charcoal was burnt in ſmall portions in a crucible of platina until we obtained a quantity of aſhes ſufficient to be ſubjected to examination, and the following reſult was obtained:—150 parts of charcoal of ants were reduced in an ignited platina crucible, at the end of ſeveral hours, to 22 parts of aſhes; 14 parts of theſe aſhes were diſſolved by nitric acid; the eight parts not diſſolved were ſand mixed at firſt with the ants.

It reſults from this experiment that the oſſeous ſkeleton of ants is formed, like that of warm-blooded animals, of phoſphate of lime. It is not improbable that the long and ſtrong calcination them with the ſame ſubſtances. Without here entering into a tireſome account of theſe comparative experiments, we ſhall content ourſelves with ſaying that theſe two compounds exhibited phænomena abſolutely ſimilar.

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tial organs of life, and which open only on the outſide of their bodies. Anatomy only can unveil to us this remarkable apparatus of ſecretion.

15th, In regard to the malic acid which in ants accompanies the acetous acid. We ſhall terminate this memoir by a general remark on the exiſtence of this acid in organic compounds.

Few vegetable acids are ſo generally and ſo abundantly diffuſed throughout nature as the malic acid; and, though it has not been long known in compariſon of many others, it has been diſcovered in a multitude of ſubſtances, and its properties have been carefully ſtudied.

It has been found in fruits with ſeeds, with ſtones, and in a great number of berries.

It exiſts in a multitude of plants in the ſtate of malate of lime. The joubarbes, craſſula, and cotyledons, meſembryanthemum, ſedum, and even aloes, contain more or leſs conſiderable quantities of it, according to the analyſis made by C. Vauquelin, and inſerted in the *Annales de Chimie.*

It is found in abundance in the liquor which is ſeparated by the hairs of chicken peaſe (*cicer arietinum*), where it is accompanied with a ſmall quantity of oxalic acid and by ſome atoms of acetous acid.

It is formed by the action of the nitric and oxygenated muriatic acids on all vegetable ſubſtances, and in particular on ſugar, gums, ſtarch, honey, &c.: it always precedes the formation of the oxalic acid by theſe re-agents: vegetable and even animal ſubſtances are conſtantly changed into malic acid before they are converted into oxalic acid, by the acids above indicated.

It is thus that blood, urée, the uric acid, and jelly, when treated with either of the acids above mentioned, are firſt changed into malic acid, and afterwards into oxalic acid if the action of the acid be continued in a powerful manner; but it is always united with ammonia when it ariſes from animal matters, becauſe there is formed at the ſame time a certain quantity of that alkali.

It is not vegetables only that continually give birth to the malic acid: animals are equally ſuſceptible of producing it: it is in the claſs of inſects in particular that this property is manifeſted.

It appears that the malic acid is in ſome meaſure the firſt ſtage of acidification in the proceſſes of nature and of art. It precedes in a ſpecial manner the formation of the oxalic and acetous acids, becauſe it contains a greater quantity of the radicals, or of carbon and hydrogen, and conſequently less oxygen than they do. It is that of all the vegetable or animal acids which retains in the fulleſt manner the nature of the animal or vegetable ſubſtance from which it has been formed; it is that which is decompoſed with the greateſt eaſe by the action of the fire. Preceding all the vegetable acids, by interior and ſucceſſive elaborations it produces the tartarous, citric, malic, and acetous acids: it is by loſing a part of its radicals, which are converted into water and carbonic acid by atmoſpheric oxygen, that it paſſes itſelf to the ſtate of theſe other acids: and though none of the latter but the acetous acid has yet been found in animals, there is reaſon to believe that all the reſt will be found.

The malic acid is then formed by living plants: it often exiſts in them in its full purity: ſometimes it is found in them united to lime, and no doubt to potaſh, according to the nature of the circumſtances which accompany its formation. It is alſo formed during the life of certain animals, particularly in ants, and certainly in many other inſects: in a word, it is produced by the action of the nitric and the oxygenated muriatic acids on animal and vegetable compounds.

Nature then, every time it can diſpoſe of the neceſſary principles, tends to form malic acid; and there is no reaſon to doubt, that if plants were examined when very young, at which time they are all acid, the malic acid would always be found preſent in them. Theſe ſucceſſive changes afterwards give birth to the other acids already mentioned, and which may be found by further analyſes in animals, as it has been found in vegetables.

XXVII. *On the Property which the Acetic Acid poſſeſſes of diſſolving Camphor and various eſſential Oils.*

To Mr. Tilloch.

SIR,

I AM ſenſible that an apology is neceſſary for obtruding on you and on your readers a ſubject which may appear, on firſt view, to have little claim to general attention. If the [illegible] of an individual were alone involved on this occaſion, I ſhould not have requeſted a place in your Magazine for the following ſtatement. But it is ſurely matter of general concern, that the appropriation of inventions and diſcoveries ſhould be dealt, with ſtrict juſtice, to their [illegible] the proſpect of this diſtribution of “ honour [illegible] one of the moſt animating principles of [illegible] of this motive would certainly follow

low an indifference, on the part of the public, to the claims of inventors.

More than fifteen years ago, during the delivery of a courſe of lectures, by my father, in this town, he had occaſion to notice a property of the radical vinegar, or acetic acid, which had not, to his knowledge, been before obſerved; viz. its property of diſſolving camphor and various eſſential oils. The compound was found to poſſeſs a moſt pungent and agreeable odour, and as the *vinaigre des quatre voleurs* had gained much reputation in obviating infection, it occurred to him that the newly diſcovered ſolution would have ſtill more powerful effects in conſequence of its high ſtate of concentration. A bottle of this preparation he gave to a late active magiſtrate and philanthropiſt (J. B. Bayley, Eſq. F.R.S.), who, in the courſe of an unwearied and undaunted exerciſe of his public function, was frequently expoſed to the danger of foul and infected air. Mr. Bayley was highly gratified with its effects, and not only made conſtant uſe of the aromatic vinegar on the bench, and on his viſits to the priſon, but introduced it to the adoption of ſeveral of the judges and principal gentlemen at the bar. He alſo firſt ſuggeſted to my father the propriety of benefiting by his diſcovery, and was the medium of a connection with Mr. Bayley, perfumer, in Cockſpur-ſtreet, London, which has continued to the preſent day.

The aromatic vinegar, like every other article in general demand, has been a frequent ſubject of imitation. But it is not of this that I complain; for, in conſequence of unremitting attention, our preparation has maintained a decided ſuperiority over all others, both as to quality and extent of ſale. The occaſion of this appeal to your readers is, that one of theſe imitations has lately been ſanctioned by the name of a reſpectable phyſician, who, though not expreſsly, yet by implication, has conferred on another the credit of that invention, which, in juſtice, belongs to my father. (See a letter from Dr. Trotter, phyſician to his majeſty's fleet, contained in an advertiſement publiſhed in the newſpapers by a London druggiſt.)

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XXVII. *On the Property which the Acetic Acid poſſeſſes of diſſolving Camphor and various eſſential Oils.*

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I believe there are few of your readers who will not agree with me, that the ordinary forms of civility required that Dr. Trotter ſhould have taken further notice of the letter addreſſed to him; that ſuch an attention ought to have been paid to one of the oldeſt practitioners of medicine in this country; and that more reſpect was due to a man (whom I truſt it is not unbecoming in me to characterize, in terms already publicly applied to him *viris laudatis* *) "reſpectable in ſcience and in literature," and "diſtinguiſhed by ingenuity, honour, and the ſtricteſt integrity."

I remain reſpectfully, Sir, your obedient ſervant,

WILLIAM HENRY.

XXVIII. *A general View of the Coal Mines worked in France, of their different Products, and the Means of circulating them. By* C. LEFEBVRE, *Member of the Council of Mines, of the Philomatic Society, &c. &c.*

[Continued from p. 76.]

Department of Ardennes.

THERE are no coal mines known in this department.

Reſearches have been made at Etion, but without ſucceſs. They were begun in ſchiſtous ſtrata, which exhibited no indication ſufficient to cauſe the labour to be continued; and the manner in which they were conducted was calculated only to produce loſs, ſince thoſe who directed it followed the courſe of the ſchiſtous ſtrata, inſtead of traverſing them to diſcover the changes, if there were any, in the order of the ſtrata.

This department receives coals from that of Ourthe, which are conveyed to it up the Meuſe.

Department of Arriege.

This country (7), abundant in metallic ſubſtances, and particularly in iron mines of an excellent quality, poſſeſſes no coal mines worked.

C. Vergniez-Bouiſcher, proprietor of the forges of Vicdeſſos,

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* Dr. Aikin and Dr. Percival.

deſſos, diſtinguiſhed by his knowledge and his zeal for improving the iron manufactory, has announced indications of coal at Monteſquieu near Foix; and Duhamel, the engineer, has found ſome alſo at Mas-'dAzil.

Department of Aube.

No coal mines are worked in Aube: the general compoſition of the ſtrata of the earth, which exhibit only chalk or beds of ſhells, affords no reaſon to conceive any hope of diſcovering collections of this combuſtible mineral, unleſs it be at great depths, and after having traverſed the whole thickneſs of the beds of chalk and ſhells.

It receives no coals but thoſe which are conveyed on the Seine, or which are carried up by La Marne and the river Aube.

Department of Aude.

Coals are found in the neighbourhood of Caſcaſtel (8), Quintillan, Ruchan, and the mountains of Fabrezan.

The mines worked near Caſcaſtel, Quintillan, and Segur, furniſh about 14000 myriagrammes of fuel per annum.

The products of the other mines are not known, nor do we poſſeſs any certain information reſpecting the price at which the coals are ſold. It is probably very low, becauſe there is very little conſumption for theſe mines.

The proprietors complain loudly of the roads, and wiſh they were repaired; which would facilitate the tranſportation of coals to Perpignan.

It would be uſeful alſo, if it were poſſible, to provide means for conveying the products of the mines of Fabreſan to the banks of the Aude.

Department of Aveyron.

This department (9) is one of the moſt abundant in coal mines. It is intereſting alſo on account of ſeveral other mineral ſubſtances, and particularly of thoſe proper for ſupplying ſulphates of alumine and of iron (common alum and green copperas), which are found there in great plenty in the cantons of Milhau, Saint-Affrique, and ſeveral others.

The collections of coal known near Cranſac, Vialarets, Livignac, and Montignac, and the neighbouring places on the borders or at a little diſtance from the river Lot, are inexhauſtible, and for the moſt part can be very eaſily extracted.

In the year 3 theſe mines produced above 500,000 myriagrammes, and the price was not more than five cents per myrialeſs oxygen than they do. It is that of all the vegetable or animal acids which retains in the fulleſt manner the nature of the animal or vegetable ſubſtance from which it has been formed; it is that which is decompoſed with the greateſt eaſe by the action of the fire. Preceding all the vegetable acids, by ulterior and ſucceſſive elaborations it produces the tartareous, citric, oxalic, and acetous acids: it is by loſing a part of its radicals, which are converted into water and carbonic acid by atmoſpheric oxygen, that it paſſes itſelf to the ſtate of theſe other acids; and though none of the latter but the acetous acid has yet been found in animals, there is reaſon to believe that all the reſt will be found.

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Theſe coals eaſily pulverize. They muſt not be long left expoſed to the air. When they have been totally reduced to duſt by the action of the atmoſphere, or the effect of carriage, they loſe almoſt entirely their combuſtible property.

The very high price of wood in this country ought to produce much greater care in the working of theſe coal mines. It is probable that better products would be obtained; and even in caſe the lower ſtrata ſhould not preſent coal of the beſt quality, the longer continuance of the extraction might be enſured.

Subterranean fires have already deſtroyed a part of the ſtrata of the coals of this country, and particularly at a place called la Galere; at another mine not far diſtant the ſtrata took fire, and have been burning for ſeveral years.

Department of Calvados.

A coal mine (11) is worked in the commune of Litry, in the canton of Baynes. It furniſhes from four to five millions of myriagrammes of coal of the beſt quality, the greater part of which is conſumed in the country. A conſiderable quantity, however, is exported by the port of Iſigny; and this mine, during the war, was a valuable reſource to the ports of Cherbourg, Havre, and Honfleur at the mouth of the Seine; and for the manufactories of arms which were put in a ſtate of activity at Saint-Valery-ſur-Somme. The price of coals at the mine varies from 12 to 25 cents, according to its quality.

Great praiſe is due to the renters of the mine of Litry, who uſe every exertion to give proper activity to this enterpriſe. In the year 9 they erected there a ſteam-engine, which exhauſts the water and at the ſame time draws up the coals. This machine was conſtructed by C. Perrier. It anſwers the purpoſe exceedingly well. It ſaves at this mine eighteen horſes daily; and conſumes about 50 myriagrammes of coals. It is much to be wiſhed that the uſeful example ſet by the renters of the mine of Litry were imitated in other enterpriſes of a ſimilar kind. I have been aſſured that the renters of the mines of Anzin, in the department of the North, are going to erect ſimilar engines at their works.

The coal mine of Litry is the only one now worked in the department of Calvados. Searches have been made in ſeveral places, but hitherto without ſucceſs. From the report of C. Duhamel, inſpector of the mines, it however appears that thoſe begun at Feuguerolles, at a ſmall diſtance from Caen, deſerve to be proſecuted.

Department of Cantal.

This department (12), ſo intereſting to natural hiſtory, and in particular on account of the antient volcanoes found in it, is not abundant in coal mines. It is only in the north-weſt, in the diſtrict comprehended between Mauriac and Bort, that ſome collections of this combuſtible are diſcovered.

It appears that the ſmall quantity of pits dug are only at the ſurface, and have been dug in a very irregular manner. We have no accurate information in regard to the quantity of coals extracted, or to the price. Theſe mines, however, which are not far from the courſe of the Dordogne, and from the place where that river is navigable for boats, might by theſe means obtain a very extenſive conſumption, and deſerve to be worked with greater care.

Departments of Charente and Lower Charente.

There are no coal mines worked in theſe two departments, and therefore they are obliged to procure their fuel from others. The ports of Rochelle and Rochefort may receive and give circulation to the coals carried thither by ſea from the departments of the North or from England, or thoſe conveyed by the internal navigation, and which might be ſent to La Gironde by the Tarn, the Lot, and La Dordogne.

Department of the Cher.

No coal mines are worked in this department; but it might be ſupplied in abundance with coals of an excellent quality by the Cher, if that river were rendered navigable with more ſafety to above Saint-Amand, which would require very little expenſe. The rich collections of coal of Commentry, and the other ſtrata lately diſcovered in the environs of Meaulne, in the department of Allier, would obtain a conſumption, which is at preſent wanting; and the working of them would give life to theſe cantons, and would concur with the products of the valuable forges in the department of Cher to increaſe the means of induſtry in that country.

Department of Correze.

Coals are dug up in ſeveral communes of this department (13), and a great many indications of this combuſtible are found in it. The communes where the beſt known mines are

now

now worked are Argental, where it appears there are abundant collections of coal; La Pleau, where ſeveral ſtrata are known, and worked with facility, becauſe they form part of a mountain in which galleries for extracting the coals, and ſuffering the water to run off, are conſtructed at a ſmall expenſe: in a word, the communes of the Cublac, Venteſſac, and the environs of Alaſſac.

The annual product of theſe different mines may be eſtimated at 50,000 myriagrammes. That of La Pleau ſupplies the manufactory of arms at Tulles, it affords the principal conſumption; and that of Montignac ſupplies the manufactory of Bergerac.

The Vézère, which begins to be navigable at Uzerche, and La Corréze at Tulles, furniſh ſome means of tranſportation for the other mines. The Dordogne, which is navigable for boats only at Souillac, would furniſh more means of activity to the mines of La Pleau, and thoſe of Argental, were it rendered navigable as far as the latter.

The mean price of the coals is 10 cents per myriagramme. The mines in general are badly worked, as they want a ſufficient conſumption.

Corſica and the Iſland of Elba.

The information hitherto obtained in regard to the mineral productions of theſe two iſlands does not afford reaſon to think that they contain coals.

[To be continued.]

XXIX. *Sketch of the General Hiſtory of Mining.*

To Mr. Tilloch.

SIR,

I AM happy in having it in my power to add to the Sketch of the Hiſtory of Mining in Devon and Cornwall, with which I ſome time ſince troubled you *, a more extended view of the ſubject from the hands of an ingenious friend. If the inſertion of either the former or the preſent in your valuable work ſhall ever draw to the ſubject the attention of thoſe who are able, in different diſtricts, to enlarge our knowledge on this head, it may be a general benefit, and will gratify

Your obedient ſervant,

JOHN TAYLOR.

Taviſtock,
March 1803.

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* See vol. v. p. 357—365.

Sketch

Sketch of the General Hiſtory of Mining.

Conamur tenues grandia! HORACE.

IT gratifies the curioſity, and proves ſufficiently amuſing, to trace the origin and delineate the progreſs of any thing which occupies our thoughts or engages our attention; but when ſuch an inquiry is connected with any important art or ſcience, it ſurely muſt be more worthy the proſecution, as being calculated to afford ſome uſeful information as well as agreeable entertainment. Such are the effects that ſeem likely to reſult from the hiſtory of the riſe and progreſs of the art of mining; and it is hoped that the reader may meet at leaſt with the latter of theſe effects in this humble attempt to collect into a ſhort compaſs what may perhaps convey ſome general idea of the antiquity of the art of working mines, and its gradual advancement to its preſent dignified and important ſtation.

When we conſider the many improvements and important diſcoveries that have been made in chemiſtry within theſe few years, and compare the increaſe of chemical knowledge in the preſent age with that of former periods, it may perhaps be thought that we cannot receive any advantages from old authors, and that hiſtories of this kind ſcarcely deſerve notice, becauſe they are not abſolutely neceſſary to the knowledge of mining, nor to the juſt performance of the ſeveral operations belonging to it. Yet, as nothing is more intereſting to an artiſt than the riſe and fate of his art, and nothing more uſeful to him than the labours and experience of his predeceſſors, it appears incumbent upon us to receive with acknowledgment, and ſtudy with attention, the works of thoſe wiſe and enlightened men in the middle ages who directed their attention to the improvement of the arts, and, being free from the prejudices of hoſtile ſects and attached to the love of truth, contemplated with ſteady zeal the operations and productions of nature, and deſcribed appearances as they exiſted, without any mixture of theory or hypotheſis.

The art of diſcovering metals in the mine, and rendering them fit for uſe by chemical proceſſes, is of the moſt remote antiquity. Moſes, the oldeſt of all authors, mentions the uſe of braſs and iron, which could only have ariſen from ſome acquaintance with the metallurgic art; and this account is confirmed by the moſt antient hiſtorians and fabuliſts. Diodorus Siculus relates that the Egyptians adored Vulcan as a god, and eſteemed him the firſt inventor of all arts and operations relating to metals; and all the profane authors coincide in this belief of Vulcan being the firſt inventor of the uſe of metals, and that in the earlieſt ages of the world. Greece, the firſt enlightened nation of Europe, received the rudiments of her arts and knowledge from Aſia and Egypt, and probably tranſmitted her knowledge of working mines to the Romans. The antient Greeks and Romans were not deficient in the mathematical parts of mining, as ſome of their ſubterraneous works ſtill remain objects of admiration; and we may conclude from their accounts that they were good metallurgiſts and ſmelters, though they have left but few traditional practices and proceſſes, and theſe vague and not to be depended upon.

It ſeems probable that mines were objects of attention and inquiry among the Romans, as we find that the hiſtorian Tacitus, in his account of Germany, remarks that the inhabitants were not acquainted with their preſent valuable mines of gold, ſilver, &c.; but doubts whether treaſures might not be diſcovered upon ſearching for them. "Argentum et aurum propitii an irati Dei negaverint, dubito. Nec tamen affirmaverim nullam Germaniæ venam argentum aurumve gignere. Quis enim ſcrutatus eſt? Poſſeſſione et uſu haud perinde afficiuntur. (*De moribus German.*)

And it ſeems probable alſo that the Romans were acquainted with the proceſſes of digging, ſeparating, and purifying, metals; as the mines in Tranſylvania are generally ſuppoſed to be Roman works; and ſome mines on the Rhine and the Danube, in Lorrain, Alſace, Suevia, and Noricum, appear to have been worked in the decline of the antient Roman empire.

After the deſtruction of the weſtern empire, when all learning, books, arts, and ſciences, were overwhelmed in the general wreck, we have no account of any mineralogical purſuits until the middle of the tenth century, when the mines in the Rammelſburg, near Goſlar, and ſome of the adjacent ones in the Hartz mountains were diſcovered, and worked to advantage. The metallurgical proceſſes at Goſlar were probably conducted upon traditional proceſſes either Roman or German; and we may ſuppoſe the knowledge of the Germans was not conſiderable if we judge from their operations, which, on account of the irony and zincous refractory mixtures, were various, compound, and tedious. The commencement of the ſixteenth century may be looked upon as one of the moſt remarkable and ſplendid epochs in the annals of mining, as well as in the annals of the world. Arts and ſciences then began to be proſecuted with ardour and ſucceſs, and from this æra the productions of nature were ſtudied and unfolded to man. The traditional and empirical ſcience of metals now gave way to the ſcientifical principles of chemiſtry, which were derived from the Arabians.

Agricola was the firſt of the learned writers on this ſubject; and for his extraordinary learning and practical erudition in his treatiſes *De Re Metallicâ et de Re Foſſilium* he is juſtly entitled to the firſt place among the chemical metallurgiſts who have ſince appeared. H. R.

XXX. *On the Converſion of Graſs Land into Tillage, &c.**

To the Editor of the Philoſophical Magazine.

SIR,

WITHOUT offering you any apology for intruding upon your time, I take the liberty of ſending you an abſtract of a paper to which the utmoſt conſequence is attached from its ſubject, and written by a man whoſe name is too well known amongſt thoſe ſkilled in agriculture to need any commendation.

The eſſay is divided into three chapters, and each chapter is divided into ſections. The preſent communication is an abſtract of the firſt chapter.

Mr. Cloſe ſets out in this chapter by ſaying (which is an undoubted truth) that agriculture is the parent of commerce, but that it has made a very ſlow progreſs, conſidering its utility and the advantages we derive from ſuch a beautiful ſcience. The obſtacles which are ſuppoſed (and juſtly too, I firmly believe) by Mr. Cloſe to the improvement of agriculture, are tithes: the difficulty and expenſe of incloſing waſte lands: expenſive and injudicious leaſes: want of knowledge in the practical farmers: the great increaſe of the poor-rates; and a want of that energy which formerly characterized the agricultural labours of this iſland.

Mr. Cloſe conceives it would be much more to the advantage of the farmer, and not in the leaſt injurious to the clergy, if ſome mode of paying the latter were deviſed inſtead of taking their income as it is done at preſent, which is one great check to the improvement of agriculture.

The tithes ſhould be valued throughout the kingdom by ſome able perſon or perſons, and, after a proper valuation has been ſet upon them, to be offered to the owner at that valuation[*]. When the tithes are ſold, the money ſhould be inveſted in the funds; and Mr. Cloſe calculates, that half, or at moſt three-fourths, of the intereſt ariſing from this capital would pay the clergy their preſent incomes, and the ſurplus to accumulate for their benefit ſhould proviſions, &c. advance. In order that the annuitants ſhould receive an adequate compenſation if any ſuch advance took place, the average price of wheat ſhould be taken once in four or five years, and a proportionate addition be made to their incomes as the corn had advanced. This meaſure is propoſed by Mr. Cloſe to originate with the crown. He calculates that the livings now in the patronage of his majeſty amount to about 110,000l. per annum, to which ſhould be added at leaſt one-third to make their full value, which would come to about 146,000l. per annum. If theſe tithes were ſold at only twenty-ſix years purchaſe, it would produce a capital of 3,796,000l., which, at only four per cent., would ſecure to the clergy holding ſuch preferment their full income, and leave above 40,000l. a year to accumulate for their benefit ſhould any unforeſeen accident ariſe. By this ſcheme, neither his majeſty nor any other patron of a living would be deprived of their patronage.

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* From a paper on the ſubject by the Rev. H. Cloſe, of Hordle, near Lymington; publiſhed in the Communications to the Board of Agriculture, vol. iii. part 1.

To obviate the difficulties attending an incloſure of waſte land, Mr. Cloſe propoſes, inſtead of a bill being brought into parliament, that there ſhould be an act "to empower the magiſtrates at quarter ſeſſions to judge of the expediency or inexpediency of ſuch incloſure," and that the uſual notices ſhould be given of ſuch a meaſure in the county papers, &c.—The bench of magiſtrates ſhould appoint five acting magiſtrates who are entirely diſintereſted in the incloſure; theſe five to appoint two commiſſioners, who ſhall be ſworn to do juſtice to all parties in adjudging the claims. Theſe commiſſioners ſhould have a certain ſum (firſt agreed upon) paid them, and by no means to be paid by the day or mile; and not to act upon more than one incloſure at the ſame time. Should any diſpute ariſe, the magiſtrates to decide, and the party againſt whom ſuch a deciſion is made, to pay all coſts[†]. Mr. Cloſe obſerves, that as this is empowering the magiſtrates to decide upon law and fact, he would recommend that a ſtatement be made for the opinion of the "higheſt legal authority." No right ſhould be preſerved to render this incloſure null and void; and ſhould any portion of land be allotted to a perſon who is the oſtenſible owner of the eſtate, and ſhould afterwards be evicted, the common then to be given to the owner. Mr. Cloſe believes that there is great want of ſkill, and not, as many ſuppoſe, capital.

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[*] There are few proprietors, I believe, in this country, from all I am able to learn, who would not jump at ſuch a propoſal, were the tithes valued at a price worth his attention.

[†] It is with great deference I beg leave to differ with Mr. Cloſe reſpecting his mode of deciding diſputes by magiſtrates: however impartial they are ſuppoſed to be, and whatever may be the opinion of the "higheſt legal authority," it is putting the property of individuals into the power of

Of leaſes, the following ſketch he recommends *:—The landlord covenants to give quiet poſſeſſion of the houſe and out-houſes, and to put and keep the ſame in repair; alſo to put the gates, ſtiles, and every ſort of fence in repair; to inſure quiet poſſeſſion of the above, and of ——— nu mber of acres of lands ſituated in the pariſh of ————, for ——— number of years; to pay the land-tax, and allow the tenant to hold certain part of the buildings and lands for a ſpecified term after the expiration of his leaſe. The tenant covenants to take the premiſes, to pay his rent in two equal half-yearly payments; to cart all materials for repairs gratis; to find the workmen, when employed in ſuch repairs, the uſual allowance of wholeſome beer; to keep and leave the gates, poſts, rails, and every ſort of fence in repair; to allow the landlord or his agents free ingreſs and regreſs to ſhoot, fiſh, or hunt, over the lands; to warn off every one, and to bring actions, if neceſſary, at the landlord's expenſe, againſt all perſons who may treſpaſs upon the premiſes; not to break up certain portions of meadow and paſture lands; to keep the ditches and water courſes open; to crop the lands according to the rotation recommended in the table at the end of this eſſay †, having reſpect to the different ſoils: to pay all parochial taxes; to expend the hay, ſtraw, haulm, and other fodder, produced from the lands, upon the premiſes; and to uſe all the dung, ſoil, and compoſt, ariſing from the ſame, upon the ſame lands belonging to the premiſes hereby demiſed; except the produce of ſuch dung and compoſt, &c. ariſing from the two laſt years' crops previous to the expiration of the leaſe; all of which ſhall be left, in a farmer-like manner, in the farm-yards, or other convenient place, for the uſe

of a ſet of men whom the old common law of England did never authorize to decide upon ſuch queſtions, and wreſting that invaluable bulwark of our liberties, *the trial by jury*, from our hands, which the conſtitution does authorize to determine. This bleſſing is too much curtailed already, and I truſt we ſhall never ſee it laid aſide upon ſuch an important caſe as the one in queſtion.

* This ſketch appeared to me ſo important, that I thought it would be better to give it complete, inſtead of abridging it.

† This will be given in a ſubſequent paper.

of the landlord or in-coming tenant: to leave all the ſtraw of the laſt year's crop to the in-coming tenant, he carting the corn to any market within the diſtance of ten miles[*]; to pay five pounds an acre additional rent for every acre of meadow or paſture land, ſpecified as not to be broken up, ſhould he plough or till the ſame; to pay five pounds an acre additional rent for every acre cropped contrary to covenant; to preſerve timbers; not to cut, lop, or top, any pollard trees, underwoods, or hedge rows, under eight or ten years growth, according to the productiveneſs or ſterility of the ſoil: to allow landlord or his agents to cut or fell timber, and to cart the ſame off the land, at ſeaſonable times.

For the want of knowledge amongſt practical farmers, which Mr. Cloſe thinks very great, he propoſes that an experimental farm ſhould be taken by the Board of Agriculture, and to allow a certain number of pupils, on paying a certain ſum of money, to be inſtructed in every branch of agriculture, and alſo thoſe ſciences more immediately connected with it. This farm to be ſupplied with proper perſons as inſtructors, and particular rules for the conduct of the pupils to be laid down and ſtrictly obſerved. There is a great want of communication (obſerves Mr. Cloſe) with reſpect to approved facts in agriculture; and being generally known in Norfolk and Suffolk, he propoſes that noblemen and gentlemen ſhould take a proper perſon as a tenant from either of theſe counties, and place them upon a centrical part of their eſtates, lying in a county where the management and improvement of graſs lands and ſtock are well underſtood. He then gives his opinion from what part of Suffolk or Norfolk theſe men ſhould be taken, according to the nature of the land upon which they are to be placed.

The poor rates juſtly come under Mr. Cloſe's cenſure. He conceives that the enormous rates lately levied are owing to the inadequate price of labour in proportion to the price of proviſions[†]. This not only burthens the farmer with a conſtant load upon his operations, but falls extremely heavy upon the poor mechanic, whoſe profeſſion will not permit him to make that advance on his articles the heavy taxes he pays would juſtify him in doing. It likewiſe ultimately falls upon the land owner, who, when he lets his farm, the valuation is laways taken according to taxes, rates, &c., which the farm pays, and of courſe a deduction is made in favour of the tenant. It has likewiſe deſtroyed a very valuable claſs of ſociety, viz. the induſtrious and labouring people, whoſe honeſt pride was to boaſt that they never were beholden to their pariſh for aſſiſtance. The plan of relieving theſe induſtrious people is likewiſe bad; not that it can be altered, but by aſcertaining what a man earns, and paying him accordingly: he who is ſtrong and able, and gets a tolerable maintenance, ſeeing the man who is not able, by reaſon of his weakneſs or ill health, receive more, thinks that by ſlackening his work he ſhall receive more; and this is abſolutely the caſe almoſt every where: and he adds, " I am fully perſuaded, that by the aggregate body of agricultural labourers not more than three-fourths of the ſame work is performed as was done ten years back by the ſame number of hands." Mr. Cloſe then goes on to propoſe a plan for the relief of what is called "ſecond poor;" viz. thoſe labourers who have never received pariſh relief, but are entitled to a relief from charities left to pariſhes. As there are but few who have not had pariſh relief, and conſequently not entitled to theſe charities, an act of parliament ſhould be paſſed entitling this claſs of people to theſe charities, notwithſtanding having received pariſh relief. To guard againſt this valuable part of the community being again degraded, he propoſes that " the price of labour ſhould be regulated by the price of bread corn." Mr. Cloſe alſo propoſes that thoſe who do receive pariſh relief after ſuch a regulation as this, ſhould be badged *.

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[*] This clauſe not to extend to dairy farms; as, where a large ſtock is kept, the out-going tenant ſhould have permiſſion to fodder his cattle with it until the ſpring, leaving the dung, &c.—*Note by Mr. Cloſe.*

[†] The following curious circumſtance deſerves to be mentioned:—When the miniſters and their friends in the houſe of commons were propoſing ſubſtitutes for bread, and bounties upon different articles, Mr. Horne Tooke, in one of the debates upon a potatoe bill, threw out to the houſe this very idea of the price of labour being inadequate to the price of proviſions, and that the poor man's one pound was not worth one-fourth to him now, when compared with its former value: he recommended to miniſters to drop this method of relieving them, as it never would anſwer, for

I am, Sir,
Your very obedient ſervant,
AGRICOLA.

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for they only increaſed the price inſtead of lowering it. They ſaw the wiſdom of this remark, and they never propoſed any more bounties to lower the price of proviſions.

* Here too, again, I beg leave to differ from Mr. Cloſe with reſpect to *badging* the poor. It is making a diſtinction to which the loweſt of mankind only will ſubmit, and making a diſtinction to a claſs of people who probably may be obliged to ſeek parochial relief owing to misfortunes, and not to dearneſs of proviſions.

XXXI. *Hiſtory of Aſtronomy for the Year* 1802. *Read in the Athenæum of Paris December* 30, *by* JEROME LALANDE.

IF we were aſtoniſhed laſt year to ſee the Hiſtory of Aſtronomy begin with the diſcovery of a ninth planet, we ought to be much more ſo to find this year alſo a diſcovery of the like kind, which we did not expect. It was alſo by a fortunate chance that the tenth planet was found; but it was neceſſary that chance ſhould favour an aſſiduous and intelligent aſtronomer.

At nine in the evening on the 28th of March 1802, Dr. Olbers obſerved at Bremen Piazzi's planet, which had afforded occupation to aſtronomers for a year. He was examining with his teleſcope all the ſmall ſtars in the Virgin's wing to aſcertain their poſitions; and that he might be enabled to determine with more eaſe the places of the planet, he had arrived at the twentieth ſtar of Virgo, near which he had obſerved the planet in the month of January, and was ſurpriſed to ſee near this ſtar, which is of the ſixth magnitude, another ſmall one of the ſeventh. He was certain that it had not been there at the time of his firſt obſervations: he therefore haſtened to determine its poſition; and having continued to view it for two hours, he perceived that in this interval it had changed its poſition. The two following nights afforded him the means of determining its motion, which was 10′ per day. As ſoon as he had made public this curious obſervation, aſtronomers took the earlieſt opportunity of obſerving this new ſtar, and of calculating its orbit. Dr. Gauſs, an able geometrician of Brunſwick, calculated the elements, and C. Burckhardt employed himſelf on ſimilar calculations.

On the 4th of June C. Burckhardt finiſhed his calculations in regard to this ſtar of Olbers. We expected them with great impatience, being deſirous to know whether it was a planet or a comet. He found its revolution to be 4 years 7 months and 27 days; its diſtance 2·785, or 96 millions of leagues; its eccentricity 0·2463, which produces an inequality of 28° 25′; its inclination 34° 51′; its node 5^{s} 22° 28′; its aphelion 10^{s} 2° 3′; and the epoch of its longitude for 1802 4^{s} 23° 10′.

On the 14th of November C. Burckhardt publiſhed others in the Moniteur, which correſpond to the obſervations made as far as the 20th of September 1802. The revolution 4 years 7 months 13 days; diſtance 2·7699; eccentricity 0·2463; inclination 34° 38′ 0″; node 5^{s} 22° 27′ 35″; aphelion 10^{s}

1° 12′ 19″; epoch of 1802, 4^s 23° 21′. 38″, the 31ſt of December preceding; equation 28° 25′. C. Burckhardt has calculated alſo the perturbations of Olbers's planet, which amount to ſeveral degrees; they are long and difficult, and will require changes in the foregoing elements. Dr. Gauſs has publiſhed in Von Zach's journal for September an ephemeris of Olbers's planet up to the 1ſt of July 1803. But C. Burckhardt, when he has finiſhed his calculation of the perturbations, will give us elements ſtill more certain.

In the beginning of January Dr. Olbers found again Piazzi's planet, which had diſappeared for a long time; and aſtronomers continued to obſerve it till the month of July. After the 15th of February Dr. Gauſs calculated the new elements; but C. Burckhardt undertook to calculate the perturbations which that planet experiences by the attraction of Jupiter, and found more exact elements. We afterwards received the perturbations calculated by M. Oriani of Milan, and which Dr. Gauſs took into account that he might better obtain the elements of its orbit. Thoſe given in Von Zach's journal for the month of November; diſtance 2·7675; tropical revolution 1681 days 9 hours; eccentricity 0·078835; equation 9° 2′; inclination 10° 57′ 37″; epoch for 1803 at Gotha, 233° 37′ 35″; aphelion 326° 37′ 40″; node 80° 35′ 1″.

The king of Naples has made an addition of 1200 francs to Piazzi's ſalary, in conſequence of his diſcovery of the new planet, and the homage he has rendered to his majeſty by naming it in his writings *Ceres Ferdinandea.*

This year we have had alſo a comet, and though very ſmall it was diſcovered in three places: at Marſeilles, on the 24th of Auguſt, by Louis Pons, keeper of the obſervatory; on the 28th, by C. Mechain, one of our moſt celebrated aſtronomers, to whom we are already indebted for a great number; at Bremen, on September 2d, by Dr. Olbers, who diſcovered the tenth planet. This comet was in Serpentarius, very faint and ill defined, having a very ſenſible nucleus.

C. Mechain and C. Meſſier at Paris, and C. Vidal at Mirepoix, obſerved it with care till the 3d of October; and C. Mechain calculated its elements in the following manner from his own obſervations: Node 10^s 10° 17′; inclination 57° 0′; perihelion 11^s 2° 8′; diſtance 1·0942; paſſage of the perihelion, September 9th, 20^h 43′ 15″ direct motion. The elements calculated by Dr. Olbers, as well as his obſervations, have been inſerted in Von Zach's journal.

It may here be ſeen that this comet is among the ſmall number of thoſe which at their greateſt proximity to the ſun are further diſtant than the earth: it is the 93d the orbits of which are known.

C. Lalande junior has had the ſatisfaction of furniſhing exact poſitions of ſtars, before unknown, with which aſtronomers had often been obliged to compare this comet; and the ſame thing has happened to him theſe fifteen years.

The new tables of the moon by M. Burg form a very important epoch in the hiſtory of aſtronomy for this year. I was informed by Dr. Von Zach that M. Burg had been employed at Vienna, amidſt poverty and obſcurity, in calculating obſervations of the moon made at Greenwich, with the hope of improving the tables of the moon: and on the 19th of March 1798 the commiſſioners of the Inſtitute, being aſſembled at the Board of Longitude to determine on the ſubject for a prize, I propoſed to them to require the eſtabliſhment of the epochs of the moon by a great number of obſervations. I knew that M. Burg had calculated a great many, and I judged that this would give him an opportunity of publiſhing them, and at the ſame time afford us the means of rewarding his labour. When the prize came to be adjudged, as C. Bouvard made a great many intereſting reſearches alſo, it was thought proper to divide it. But general Bonaparte, who that day preſided, propoſed to double the prize, in order that each might have 3400 francs: and this was agreed to.

C. Laplace, finding that this ſum was too ſmall for ſo immenſe a labour as that of C. Burg, and that he could deduce from theſe calculations all the moon's equations with a preciſion never before obtained, induced the Board of Longitude to propoſe a prize of 6000 francs, and prevailed on the miniſter of the marine and of the interior to furniſh each the half of this ſum. The queſtion was publiſhed on the 20th of June 1800, and by the commencement of November 1801 we received the tables ſo much wiſhed for; and then ſeveral ſupplements. On the 26th of January C. Laplace announced to the Inſtitute, that he had found in the theory of the moon an equation the period of which is 180 years, and which amounts to 16″; ſo that it will ſerve to explain the diſagreement obſerved between the mean motion of the moon 100 years ago and that given by the laſt obſervations. This equation is compoſed of two terms, of which, for the moment, we can have only the ſum; but it has ſerved to eſtabliſh in the motion of the moon, at different periods, a regularity which before could not be obtained.

On the 25th of July a deputation from the Board of Longitude went to give in its report to the firſt conſul on the labours of Burg, and the prize of 6000 francs. I reminded him, that he had cauſed the firſt prize to be doubled, and that it would be worthy of his character to double the preſent one. He inſtantly complied. The miniſter Chaptal, who was preſent, propoſed to me to induce Burg to come to Paris, where he ſhould have a penſion of 3000 francs. He would be an excellent co-operator in improving aſtronomy in France, where it is already ſo much cultivated: but this worthy aſtronomer preferred his own country with leſs advantage. His tables of the moon are about to be printed, as well as the new tables of the ſun by Delambre; and we have already communicated them to Dr. Maſkelyne, the aſtronomer royal of England, who will be enabled by them to improve the Nautical Almanac, which ſince 1767 has been of the greateſt utility to navigators.

The report of C. Delambre will be publiſhed in the *Connoiſſance des Temps* for the year 13, which is about to appear.

The Arabic text of all the obſervations in the manuſcript of Ibn Iunis, lent to us by the Batavian republic, with a tranſlation by C. Cauſſin, and an extract from the part which it was not judged neceſſary to tranſlate and to print, has been finiſhed at the printing-office of the republic. C. Cauſſin is the firſt of all the profeſſors of Arabic who has rendered his knowledge really uſeful. I reproached them fifty years ago, on account of their tranſlating nothing but romances.

The obſervation of the eighteenth tranſit of Mercury over the ſun's diſc on the morning of November 9 perfectly ſucceeded. We were the more intereſted in it, as it will not be again ſeen at Paris till the 5th of May 1832. It confirmed, in the completeſt manner, the exactneſs of my tables of Mercury. The egreſs of the centre, according to a mean of all the obſervations, took place at $7' 34''$ afternoon; and the longitude of Mercury which I thence deduced is $7^s 16° 17' 9''$, at $21^h 2' 40''$ mean time of the conjunction, even taking into account the correction to be made in the tables of the ſun, which by my nephew Lalande was obſerved to be $-10'' 4$; the geocentric latitude in conjunction $56''$. I have had the moſt ſatisfactory confirmation of the theory of Mercury, which I gave in the firſt memoir, read in the firſt ſitting of the firſt claſs of the Inſtitute on the 1ſt of January 1796. More details on this ſubject will be found in the Memoirs of the Inſtitute.

M. Caſella, phyſician to the king of Naples, and M. Bygge, aſtronomer royal of Copenhagen, &c. have ſent me exact obſervations of this tranſit.

The ſolſtice was exactly obſerved with whole circles by C. Delambre, and by C. Burckhardt and Lalande junior.

The mean of four hundred obſervations gives 23° 28′ 7″, or 7″ more than in my tables: laſt year it was only 6″; we ought to be ſatiſfied with this agreement.

Duc-la-Chapelle has publiſhed, in the fourth volume of the Memoirs of the Inſtitute, ſolſtitial obſervations which give him 31″ for the ſecular diminution of the obliquity of the ecliptic. C. Mechain has ſtill found 10 ſeconds leſs for the obliquity of the winter ſolſtice.

C. Vidal has ſent in his obſervations of the new planets and a great many others: he has even had the complaiſance to terminate ſome zones of circumpolar ſtars which were not complete in the *Hiſtoire Céleſte Françoiſe* publiſhed in 1801.

The inferior conjunction of Venus, obſerved in the end of December by C. Burckhardt and Lalande, gave us for the correction of my tables + 29′, which do not make 10 in the longitude ſeen from the ſun.

But as the tables of Venus had not yet been calculated with the perturbations, and as C. Lalande junior propoſed to undertake this labour, C. Chabrol furniſhed him with a table of the equation calculated to tenths of ſeconds, which was an eſſential preliminary. It is time it ſhould be begun. We have 40 years exact obſervations of Venus; and theſe 40 years obſervations, from 1761 to 1801, will give us the motion of Venus as exact as the Babylonian obſervation made 2072 years ago, and reſpecting which there is ſome doubt, as I have already explained. (See *Mémoires de l'Academie* 1785, p. 250.)

The oppoſition of Mars, which took place on the 24th of December, could not be obſerved; but the obſervations of that night gave a correction in the tables of Lalande junior — 5″; which make only two in the heliocentric longitude.

The diſappearance of Saturn's ring, which will take place in 1803, has been preceded by a ſingular phænomenon. The *anſes* were on the point of diſappearing, and it was with difficulty that C. Mechain diſcovered them on the morning of December 20. C. Flaguergues alſo loſt ſight of them on the 16th. But we ſhall ſoon ſee them re-appear, as the earth will return on the 28th of June: the ſun paſſing through the plane of the ring, we ſhall loſe ſight of them till the 23d of Auguſt, at which time the earth will paſs to the north of the ring, and permit us to ſee the ſurface of it illuminated by the ſun. *Duſejour, Traité analytique des Mouvemens Céleſtes*, vol. ii. p. 155.

C. Chabrol and C. Flaguergues have calculated a great number of tables of aberrations and nutations for 600 ſtars of the fundamental catalogue which Lalande junior inſerts every year in the *Connoiſſance des Temps*, and always with new improvements.

C. Lalande, my nephew, continues to obſerve the right aſcenſions and declination of a great number of ſtars not well known; and madame de Lalande continues the reductions, which ſhe promiſed, for the 50,000 ſtars: 1500 will be found in the *Connoiſſance des Temps* for the year 13, which will ſoon appear; and M. Bode has publiſhed more than 10,000 in the catalogue of 17,000 ſtars which accompany the large and beautiful atlas in twenty ſheets which I have already announced.

Laſt year I gave an account of the labour undertaken by the Swediſh aſtronomers Svanberg, Oſverbom, Olmquiſt, and Palander, to verify the degree of the meridian under the polar circle. M. Melanderhielm, though 76 years of age, ſet on foot and directed this enterpriſe. In 1801 they had already diſcovered the ſtations, elevated the ſignals, and built two obſervatories. They ſet out in the month of January 1802, and meaſured the baſe on the ice of the river Torneo between the 6th of February and the 8th of April, though the cold was 24°. At the beginning of September they had completed the meaſurement of the angles of their triangles, and ſet out to proceed northwards to Pathavara to commence their aſtronomical obſervations. We ſhall therefore ſoon have the curious reſult of this new meaſurement.

On the other hand, C. Mechain, in conſequence of the conſular decree of September 17, is about to reſume the meaſurement of the meridian as far as the iſland of Cabrera, which is 40 leagues ſouth of Barcelona, agreeably to his wiſh, which I announced in the *Connoiſſance des Temps* for the year 10. By theſe means the 45th degree, which it is particularly intereſting for us to be well acquainted with, will be a mean of the whole interval.

The Portugueſe, whoſe ſilence we regretted, begin to diſtinguiſh themſelves. M. Demonfort has ſent us calculations of eclipſes of the ſun viſible at Liſbon during this century: M. Monteira de Rocha, new tables of Mars, with all the perturbations. The equation is 10° 41′ 39″, greater only by 4″ than in the tables of C. Lalande junior. M. Damoiſeau, captain-lieutenant of the royal brigade of the marine at Liſbon, has written to me that he is employed on the Nautical Ephemerides of 1806. Thoſe of 1805 were calculated directly, without employing the Nautical Almanac. I have requeſted him to wait for the new tables of the ſun and moon, which are about to be printed.

We have received alſo a deſcription of the obſervatory of

Coimbra, from which it appears that it is pretty well furniſhed with inſtruments: a ſector of 10 feet, a 5-feet meridian teleſcope, a 3½-feet quadrant divided by Troughton of London.

We have received the Aſtronomical Ephemerides of father Coſſali, of Parma: they are not deſtined for aſtronomers ſuch as thoſe of Berlin, Vienna, Milan, Bologna, and Paris; but the author has added a very minute hiſtory of the two new planets, and he follows my example in giving them the names of Piazzi and Olbers; he alſo employs that of Herſchel, as a mark of homage due to them by aſtronomers.

On the 22d of June 1802, M. Van Swinden ſent us a Dutch memoir on the new meaſures, which engaged him a long time; and he has obtained a decree for eſtabliſhing the new meaſures in the Batavian republic.

Mr. Mackay has publiſhed a curious work, in England, on the longitude.

M. Van Swinden has ſent us the fourth edition of his Diſſertation, in Dutch, on the Determination of the Longitude by the Obſervation of the Moon's Diſtance from the Stars; with a diſſertation, which he publiſhed in 1788 conjointly with M. Nieuwland, on the Uſe of Sextants and Octants. He propoſes to publiſh alſo a memoir on reflecting circles, which he wiſhes to introduce into the Dutch navy; and alſo on marine time-keepers. So early as the year 1774 I went to Holland to ſolicit the introduction of aſtronomy into navigation, and obtained a promiſe for that purpoſe from the ſtadtholder and the grand penſionary. My Aſtronomy was even tranſlated into Dutch: but this circumſtance was attended with little advantage at that time, notwithſtanding the need which navigators had of it. At preſent, ſince the navy reſumes new activity in the Batavian republic, and that the learned profeſſor enjoys there a well merited influence, we have reaſon to believe that aſtronomy will be employed there in an effectual manner.

M. Van Swinden explains in this work all the methods, by calculation, by graphic operations, and by inſtruments; the corrections made by Mr. Mackay to the methods of Borda and Dunthorn; thoſe of Kraft and of Douwes de Steinſtra. He has added a collection of ſuch tables as are neceſſary in navigation.

M. Mendoza has already publiſhed two large collections of tables for navigation: he has added a new method of determining the latitude by two altitudes taken out of the meridian, the calculation of which is ſhorter than the firſt approximation, which is only the commencement of the method of Douwes.

We have received the Ephemerides of Vienna for 1803, which contains the new tables of the moon by M. Trieſnecker; but it is evident that he has ſeen thoſe of M. Burg already mentioned: alſo the Ephemerides of Berlin for 1805, where M. Bode has collected 170 pages of obſervations on the new planets, and on other important points in all parts of Germany.

M. Schroeter has publiſhed a continuation of his obſervations on the ſpots of the moon, in a large volume in quarto, entitled *Selenotopographiſche Fragmente*; that is to ſay, Topographical Fragments reſpecting the Moon: to ſerve for giving a correct idea of the ſurface of that luminary, and the changes which have taken place in it, in its atmoſphere and in its mountains; by Dr. F. F. Schroeter, grand bailiff of Lilienthal, near Bremen. Part ii. 1802. pp. 565. 4to. with 32 plates. Göttingen.

The firſt part appeared in 1791: we publiſhed two extracts from it, which ſhow with what patience and minuteneſs this able aſtronomer has examined the ſurface of the moon.

Theſe new obſervations were made with the ſame care for twelve years, and with optical inſtruments of the greateſt power, (two teleſcopes, one of 13 and the other of 27 feet.) They entitle him to the gratitude of aſtronomers, as they will ſerve them as a baſe and term of compariſon in their future reſearches reſpecting the changes that may take place at the ſurface of the moon. He has obſerved mountains which riſe to the height of 4000 toiſes, and others to the height of 2400. M. Schroeter has alſo determined, that the part of the lunar atmoſphere which is ſufficiently denſe to produce the crepuſcula, is 300 toiſes in height.

The author has alſo ſeen objects which he did not ſee in the courſe of his preceding obſervations, and which on the firſt view might be aſcribed to changes that have taken place at the ſurface of the moon: but he remarks, with the reſerve of an able obſerver, that the particular ſtate of the lunar atmoſphere may have concealed theſe objects at the time of his firſt obſervations.

Dr. Henzenberg, of Hamburgh, between the months of July and October 1802, made thirty-one experiments on the fall of bodies from the ſteeple of St. Michael, which is 235 Paris feet in height: and found that heavy bodies do not fall vertically; there are four lines of declination towards the eaſt, and 1·5 line towards the ſouth.

M. Guglielmini, of Bologna, found a little more. But all theſe experiments tend to prove the rotation of the earth.

[To be continued.]

XXXII. *Notices reſpecting New Books.*

Elements of Chemiſtry. By JOSEPH FRANCIS JACQUIN. Tranſlated from the German. 8vo.

A SECOND edition of this work has made its appearance; a proof of the favourable reception it has experienced from the public. We have no heſitation in ſaying that it is a uſeful work, and ought to make a part of every chemical library. The language poſſeſſes that preciſion which conſtitutes the chief merit of a tranſlation, eſpecially from a ſcientific or practical work. It is by Mr. Stutzer.

A Syſtem of Chemiſtry. By THOMAS THOMSON, *M.D.* 4 Vols. 8vo. 1802.

The taſk of compoſing elementary treatiſes, as the author juſtly obſerves, has in this country been uſually left to men of inferior endowments, as a piece of drudgery below the dignity of a philoſopher; while in France, excellent ſyſtematic treatiſes have appeared on almoſt every part of ſcience by men of the firſt abilities. Though we can rank ſeveral diſtinguiſhed names among our authors of elementary works, it is certain that "we have more frequently ſatisfied ourſelves with tranſlating the ſyſtematic works of foreigners, even when the diſcoveries of our own philoſophers had furniſhed a conſiderable portion of the materials of theſe works. The conſequence has been, what was naturally to be expected, the labours of our philoſophers have been frequently overlooked, and their diſcoveries claimed by others to whom they did not belong; while theſe claims, conſtantly inculcated in all the elementary treatiſes of chemiſtry, have been received as firſt principles by the greater number of readers. It is incontrovertible, that, for the rapid progreſs which chemiſtry has lately made, the ſcience is deeply indebted to the philoſophers of this country. Much, indeed, has been done by the illuſtrious body of French chemiſts; but theſe gentlemen, not ſatisfied with a part, have laid claim to the whole."

The author ſtates his work to have for its object the giving a full detail of the vaſt number of facts which conſtitute chemiſtry, blended with the hiſtory of their gradual development, and of the theories which have been founded on them, and accompanied with exact references to the original works in which the different diſcoveries have been regiſtered. It is but juſtice to ſay, that Dr. Thomſon has executed the taſk he has undertaken in a manner creditable to himſelf. His work preſents a more complete and accurate collection of facts than any previous publication of the kind. The arrangement is judicious, the quotations are candid, the reaſoning ſatisfactory and perſpicuous. We have no heſitation in recommending it as a moſt valuable performance.

The Chemical Pocket-Book, or Memoranda Chemica; arranged in a Compendium of Chemiſtry. By JAMES PARKINSON. 12mo. 1803.

It gives us pleaſure to announce that a third edition of this valuable little work has made its appearance, embracing all the new facts in chemiſtry that have been obſerved ſince the publication of the former editions. A conſiderable degree of care ſeems to have been beſtowed upon it, and ſeveral new and uſeful tables have been added in different parts.

A Syſtem of Theoretical and Practical Chemiſtry, &c. By FREDERICK ACCUM, *Teacher of Practical Chemiſtry, Pharmacy, and Mineralogy; and Chemical Operator in the Royal Inſtitution of Great Britain.* 2 Vols. 8vo. with Plates. 1803.

This work, juſt publiſhed, will prove highly acceptable to thoſe who wiſh to attain a practical acquaintance with the ſcience of which it treats. The author exhibits throughout the work a great degree of chemical knowledge; and his method of communicating knowledge to others is engaging. With the practical part of the various proceſſes in chemiſtry he ſeems to be well acquainted; and the accuracy and preciſion with which he deſcribes them, will render his work valuable not only to tyros, but to proficients in the ſcience. Many of the experiments, ſome of which are calculated to afford both inſtruction and amuſement, are quite new; and even in detailing experiments which are not new, particulars not to be met with elſewhere are often given, which will be uſeful to the practical chemiſt.

XXXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

SINCE our laſt report ſeveral meetings have been employed in reading a paper on tanning, by Mr. Davy, profeſſor of chemiſtry in the Royal Inſtitution of Great Britain. In this paper the author not only deſcribes the proceſſes uſual in the art of tanning, but enters into a chemical inveſtigation of the nature of the agents employed in the proceſs, and details a number of ingenious experiments undertaken expreſsly for the purpoſe of aſcertaining the mode of their operation. The reſults are intereſting, and cannot fail to be uſeful to thoſe engaged in the manufacture of leather.

ROYAL ACADEMY OF SCIENCES AT BERLIN.

On the 18th of January the academy held a public ſitting to celebrate the anniverſary of its eſtabliſhment. It was opened by the director Merian with an oration; after which M. de Verdy du Vernois read the ſketch of a plan for reviving the antient tournaments, and ſhowed the utility of again introducing this antient amuſement, formerly a ſchool for dexterity and intrepidity, and which might be attended with great advantage to the cavalry ſervice, and to all troops that fight on horſeback.

M. Erman then read a paper containing particulars reſpecting the reign of the elector Frederic William the Great, extracted from a manuſcript diary of marſhal Dieterich Sigiſmund von Buch.

M. Klaproth next read a diſſertation on ſtones and iron maſſes, and ſhowed that the fall of ſuch bodies from the heavens is a phænomenon totally different from all the other phænomena of nature; and that it needs therefore excite no wonder that doubts have been entertained reſpecting the truth of ſuch facts, but that hitherto they are not only ſupported by experience, but by chemical analyſes of the fallen bodies. As a contribution towards this ſubject, M. Klaproth gave an account of experiments, made by himſelf, with ſome of theſe productions which had fallen at different times and in different countries, particularly a ſpecimen of that which fell near Siena on the 16th of January 1794; and a fragment of the maſs of iron, weighing 71 pounds, which fell near Agram on the 26th of May 1751, comparing them with the analyſes lately publiſhed by Mr. Howard of the ſtones which fell at Benares in the Eaſt Indies on the 19th of December 1798, and on the 13th of December 1795 in Yorkſhire; and adding a ſhort hiſtory of theſe and ſome other bodies that have fallen from the clouds. They conſiſt either entirely of cellular maſſes of iron, or of ſtony ſubſtances mixed with grains of iron. In all of them the iron is of the ſame nature: it is ductile, exceedingly tough, appears white when filed, and almoſt always contains nickel. The ſtones are covered on the outſide with a black cruſt, are bright gray in the inſide with black ſpots, and interſperſed, beſides the ferruginous particles, with grains of pyrites. The principal part of the maſs conſiſts of oxide of iron, magneſia, and ſilex. The opinion of Dr. Chladni, that theſe meteoric productions are fragments of fire-balls which have burſt, may be conſidered as fully eſtabliſhed. Real native iron from the mines of Kamsdorff is diſtinguiſhed from the meteoric iron by not containing any nickel, and by containing a mixture of copper-lead.

FRENCH NATIONAL INSTITUTE.

Account of the labours of the Mathematical and Phyſical Claſs during the firſt quarter of the year 11.

MATHEMATICAL PART, read by C. Lacroix.

ASTRONOMY.

Obſervations on the Tranſit of Mercury over the Sun's Diſc, November 9, 1802.

If the theory of attraction, aided by the powerful means of analyſis, has enabled aſtronomers to give, almoſt at once, to the tables of the planets, and particularly to thoſe of the moon and ſun, a degree of exactneſs far ſuperior to that to which they could have been carried by the efforts of obſervers alone during a long ſeries of ages, time has not, on that account, loſt its right to the perfection of theſe reſults deduced from the meaſurement of its duration. Aſtronomers therefore, always attentive to the ſpectacle of the heavens, never ſuffer to eſcape but with regret opportunities of obſerving the ſimpleſt and moſt frequent phænomena, becauſe they know that, however preciſe they may be for the moment at which they are aſſigned, aſtronomical determinations have continual need of being rectified: with much more reaſon, therefore, they are anxious to take advantage of thoſe circumſtances where the poſition of the planets is leſs affected by the combination of their own motion and that of the earth. Such in regard to Mercury are his tranſits over the diſc of the ſun.

The firſt of theſe phænomena preſerved in the annals of aſtronomy was obſerved in 1631, at the college of France, by Gaſſendi, one of the moſt illuſtrious profeſſors of that celebrated ſchool. Since that time great attention has been paid to this phænomenon; and the obſervations of theſe tranſits, which follow each other pretty rapidly, have been multiplied. C. Lalande has employed himſelf in collecting and examining them with a care and aſſiduity for which he has been rewarded by the perfection he has thence been able to give to his tables of Mercury. After having ſucceſſively corrected each element, he waited for a confirmation of them by the tranſit announced for the 9th of November 1802, and hoped he ſhould then be able to be convinced that his tables, one of the moſt important reſults of his long labours, had reached the utmoſt perfection to which they could attain in the preſent ſtate of the ſcience. His wiſhes have been fully gratified by the obſervations we are about to detail.

The moments of interior and exterior contact of the two bodies on the egreſs of Mercury were determined as follows:

By	Lalande	$0^h\ 6'\ 29''$	$7'\ 56''$
	Meſſier	$0\ \ 6\ \ 49$	$8\ \ 20$
	Lalande jun.	$0\ \ 6\ \ 44$	$8\ \ 19$
	Bouvard	$0\ \ 6\ \ 54$	$8\ \ 19$
	Mechain	$0\ \ 6\ \ 45$	$8\ \ 30$
	Burckhardt	$0\ \ 6\ \ 45$	$8\ \ 20$

C. Lalande takes as the mean terms of the two contacts $0^h\ 6'\ 49''$, and $0^h\ 8'\ 19''$;

From which he deduces the egreſs of Mercury at

$0^h\ 7'\ 34''$;

The apparent diſtance of the centres at that moment,

$16'\ 8{\cdot}3''$;

And the ſame diſtance reduced to the centre of the earth,

$16'\ 10{\cdot}0''$.

By then comparing three differences of declination ſelected from twenty-five, which were meaſured by C. Meſſier during the tranſit, he deduces from them the ſhorteſt diſtance of the centres, namely $65''$: and combining it with the diſtance of the centres at the moment of the egreſs, he is enabled to find the difference in longitude and latitude of the two bodies, and the time elapſed between the middle of the tranſit and the egreſs. We ſhall not follow the detail of his proceſs any further, as our aſtronomical readers may eaſily ſupply what is here omitted; and to others it would be ſuperfluous. We only ſtate the reſults which C. Lalande obtained by this firſt method.

He found the true conjunction at

$9^h\ 2'\ 40''$ mean time;

The longitude, reckoned from the mean equinox for the moment,

$7^s\ 16°\ 17'\ 9''$;

The geocentric latitude,

$53''$ North;

The heliocentric latitude,

$1'\ 54''$;

The correction of the tables: — $13''$ in longitude.

C. Lalande re-commenced his calculations; ſubſtituting for the differences of declinations the obſervation of Mercury's paſſage of the meridian, made the ſame day by his nephew and C. Burckhardt; and by theſe means found $2''$ leſs in longitude, and $6''$ leſs for the latitude, at the moment of the egreſs.

He determines the geocentric latitude at the time of conjunction to be $56{\cdot}5''$, which gives the place of the node $1^s\,16^\circ\,0'\,37''$, further advanced by $1'\,46''$ than in the tables; and as C. Delambre had found by the paſſage of 1799 that this element ought to be increaſed by $1'$, C. Lalande adopts a mean correction of $+\,1'\,23''$, which makes the place of the node in 1801 to be

$$1^s\,15^\circ\,58'\,54''.$$

By comparing this determination with that which he deduced from the tranſit of 1677, he found for the annual motion of the node $43{\cdot}98''$ inſtead of $43{\cdot}3''$, which he had before.

To aſcertain whether the poſition which he gave to the aphelion of Mercury was exact, C. Lalande examined again the tranſit obſerved by C. Delambre in 1799, taking into account the perturbations calculated by M. Oriani, and the error in the tables of the ſun, and he found the ſame error of $13''$ as in that of this year, though the anomalies were very different. He conſequently thought that this error ought to be aſcribed only to the epoch of the mean motions: but after a communication from Dr. Maſkelyne in regard to a correction of $4''$ to be made in the right aſcenſions, which he aſſigned to the poſitions of the principal ſtars employed for determining the places of the ſun, the error in the tables of Mercury would be reduced to $9''$, and the epoch of the mean motions of this planet would conſequently be for 1801,

$$5^s\,11^\circ\,53'\,32''.$$

By diſtributing this error to the ſecular motion it would be reduced $6''$, and would become

$$2^s\,14^\circ\,4'\,4''.$$

In a word, the tranſit of November 9, this year, has fully confirmed the reſult which C. Delambre deduced from that of 1799 in regard to the diameter of Mercury. C. Delambre had obtained for this diameter as ſeen from the ſun $6{\cdot}3''$; and C. Lalande found $6{\cdot}2''$, while in his Aſtronomy it was $6{\cdot}9''$.

C. Meſſier communicated to the claſs a particular account of his obſervation of the tranſit of Mercury, during which he ſucceſſively determined twenty-five poſitions of that planet, comparing it either with the ſun's limb, or to a very conſiderable ſpot which appeared on that body, and of which the diameter was 25′ of a degree. He determines the moment of the egreſs of Mercury's centre at 7′ 6″ true time; and this planet's paſſage of the meridian 55″ before that of the ſun's centre.

C. Meſſier's memoir contains a detailed view of the determinations he obtained, and is accompanied with a drawing which repreſents the apparent route of Mercury on the ſun. It exhibits a luminous ring, with which Mercury ſeems to be ſurrounded. This circumſtance, peculiar to the obſervation of C. Meſſier, was remarked by him in the tranſit of 1799; it was obſerved alſo at Montpellier, in 1736, by Plantade; and at Upſal, in 1736, by M. Proſperin. This ring exhibited a very faint light, and of a colour different from that of the ſun. Its diameter towards the end of the tranſit was 1′ 19″, and that of Mercury 17″.

EXPERIMENTAL PHILOSOPHY.

On the Method of magnetizing to Saturation.

To apply with conſtancy to the ſame objects, and to vary their combinations in every manner poſſible, are the conditions on which nature conſents to diſcloſe her ſecrets to thoſe who interrogate her. It was by fulfilling them with an aſſiduity and ſagacity worthy of ſucceſs that C. Coulomb attained to his intereſting diſcoveries in electricity and magnetiſm, and by impoſing on himſelf the taſk of illuſtrating every part of theſe important branches of philoſophy. *Magnetizing*, the perfecting of which is of ſo much importance to navigators, could not eſcape his attention. Very vague notions had hitherto been acquired in regard to the intenſity of the magnetic force which ſteel is capable of acquiring in its different ſtates; and it was meaſured in general by the weight of the piece magnetized. But it is of the greateſt importance here to know the energy with which needles tend to return to their direction after they have been removed from it; and this C. Coulomb meaſures directly, by determining, according to Borda's views, and by means of very nice ſuſpenſion, how many oſcillations theſe needles perform in a given time.

After comparing the methods of magnetizing propoſed by Duhamel, Knight, and Æpinus, to give a needle all the directing force it is capable of receiving, the author decides in favour of that of Æpinus. He found that it is moſt advantageous to make the needles long and broad, but not thick. C. Jekel has eſtabliſhed a manufactory in order to make an uſeful application of theſe reſults for the uſe of the navy.

PHYSICAL PART, read by Lacroix.

On the Stones ſuppoſed to have fallen from the Clouds.

The attention of philoſophers has been again directed to the ſingular ſtones the origin of which is unknown, and which are ſuppoſed to have fallen from the clouds. The French chemiſts were deſirous of proving by experiments the identity of theſe ſtones, and of aſcertaining their component parts, already indicated by Mr. Howard.

C. Vauquelin procured ſome ſpecimens of the ſtones, analyſed by Mr. Howard, found at Benares in the Eaſt Indies, in Yorkſhire, at Siena in Italy, and at Bologna: to which he added ſome of thoſe which fell in France in 1789; at Barbotan near Roqueton, and Creon in the pariſh of Juliac, in 1790. He remarked, as Mr. Howard did, that theſe ſtones have ſuch a perfect reſemblance to each other that it is almoſt impoſſible to diſtinguiſh them. He was convinced by various analyſes that they all contain the ſame principles, namely, ſilex, manganeſe, iron, nickel, and ſulphur. Theſe reſults, analogous to thoſe obtained by Mr. Howard, and the work in which Dr. Chladni, known by his ingenious experiments on vibrating ſurfaces, has collected all the accounts publiſhed reſpecting the fall of theſe ſtones, concur to render it probable that their origin is foreign to our globe; for hitherto none of a ſimilar kind have been found in the interior parts of it.

The reading of this intereſting memoir gave riſe to a diſcuſſion the reſults of which muſt be here mentioned, as they afford new motives to induce philoſophers to examine and appreciate the different teſtimonies, in conſequence of which the ſtones in queſtion have been ſuppoſed to have fallen from the clouds. When a phænomenon is announced, if we were able to aſcertain, by a complete enumeration of the different phyſical agents, that none of them is capable of producing it, the impoſſibility of the phænomenon would be the evident reſult, and conſequently the falſity of the account.

But, on the other hand, when we find a cauſe which eſtabliſhes the probability of it, if ſound logic forbids us to aſcribe it excluſively to this cauſe, it commands us at the ſame time to ſubſtitute doubt for complete negation, and to employ every means poſſible of confirming the fact, becauſe it is not repugnant to the general laws of nature.

Chemiſts at preſent would be much embarraſſed to find in the atmoſphere the component principles which have been diſcovered by analyſis in the ſtones given to them as having fallen from the clouds; and on this account they would naturally reject ſuch events as abſurd. But C. Laplace has mentioned an explanation, which he announces not as the only one which could be given of them, and not to prove their exiſtence, but to ſhow that we ought not to reject them as abſurd, and to ſuſpend our opinion until time has procured ſome further illuſtration.

It is ſhown, by a very ſimple calculation, that a body projected from the moon would require only a velocity nearly quintuple that of the bullet of a 24-pounder diſcharged with a quantity of gunpowder equal to half its weight, to proceed to a diſtance from that ſatellite where the intenſity of its attraction would be the ſame as that of the earth. The body, when it had paſſed this point, being in the ſphere of the activity of our globe, would neceſſarily fall to its ſurface. The appearance of very conſiderable volcanoes obſerved on the ſurface of the moon does not render ſuch a conjecture improbable; but, independently of eruptions, which may be more or leſs uncommon, it will not often happen that the direction of the projection will be that which the combined motions of the earth and moon require in order that a projectile thrown from the latter body may meet with the former.

The atmoſphere of the moon, which is doubted by many aſtronomers, is ſo rare and of ſo little extent that it could oppoſe only a very feeble reſiſtance to thoſe bodies which might move in it. The caſe is not the ſame with the terreſtrial atmoſphere: it reduces almoſt to a tenth of its extent the largeſt range of a piece of ordnance; and the reſiſtance it oppoſes to rapid motion is ſuch, that to make a body projected from Veſuvius reach France, it would be neceſſary that it ſhould have a projection far greater than that which would carry a lunar body to the limits of the ſphere of the activity of that ſatellite.

It is therefore not poſſible to ſuppoſe that ſtones found at a diſtance from terreſtrial volcanoes are the product of the eruptions of theſe mountains: and mineralogy alſo oppoſes this explanation; for none of the volcanic productions known are analogous to thoſe ſuppoſed to have fallen from the clouds.

METEOROLOGY.

On the Variations of the State of the Heavens in the Mean Latitudes between the Equator and the Pole, and the eſſential Circumſtances by which they are accompanied.

The influence of meteors on the reſults of agriculture has rendered it of importance at all times that the law of the ſucceſſion of theſe phænomena ſhould be known; and the great advantages which would ariſe to mankind from the poſſibility of foretelling the nature of the ſeaſons, ſufficiently juſtifies all the fruitleſs attempts hitherto made to accompliſh that end.

Being much ſtruck with theſe advantages, C. Lamarck was diſcouraged by the bad ſucceſs of thoſe who had preceded him in this career; and from continuing to purſue it with ardour, after publiſhing in different works the cauſes which he aſſigns to the prevailing meteorological conſtitutions, he has given in a late memoir an examination of the variations of the ſtate of the heavens, that is to ſay, of the atmoſphere: he refers this ſtate,

1ſt, To the influence of the ſun's light:

2d, To that of the winds.

But he is of opinion that in the temperate zones the influence of the winds on the temperature of the ſtrata of the atmoſphere is much ſtronger than that of the light of the ſun; and that there thence reſult in the denſity and heat of theſe ſtrata, which naturally become rarer and colder the higher they go, inverſions to which he aſcribes the formation of the clouds.

The diſcordance of the effects which the ſame wind produces at different times ſeemed to oppoſe to the eſtabliſhment of a theory of theſe effects an inſurmountable obſtacle: but from numerous obſervations C. Lamarck is of opinion, that beſides the direction and nature of the winds alone, attention ought to be paid alſo to the height at which they blow in the atmoſphere.

Of the facts collected by the author, and which he conſiders as ſufficiently proved, we ſhall mention the following: "When a *north-weſt* and a *ſouth-eaſt* wind prevail ſimultaneouſly in two different ſtrata of the atmoſphere, if the *ſouth-eaſt* be the lower one, we may expect to ſee the weather become clear; but if this wind be the higher, the contrary will take place."

XXXIV. *Intelligence and Miſcellaneous Articles.*

ASTRONOMY.

We are indebted to a very accurate and much valued correſpondent for the following tables, which cannot but prove highly acceptable to ſuch of our readers as are deſirous of knowing in what part of the heavens the two new planets may be expected to be found. We believe no Ephemeris has yet been publiſhed containing their right aſcenſions and declinations. Baron Von Zach has publiſhed one in which their geocentric latitudes and longitudes are given; but this work being written in German is in the hands of but few people in this country.

	Geocentric Motion of Ceres Ferdinandea.		Geocentric Motion of Pallas.	
1803.	Right Aſcenſion.	Declin. South.	Right Aſcenſion.	Declin. North.
April 3	18^{h} 54^{m} 47^{s}	23° 6′	18^{h} 47^{m} 0^{s}	13° 33′
6	18 57 21	23 10	18 48 36	14 3
9	18 59 43	23 15	18 50 4	14 34
12	19 1 4	23 19	18 51 20	15 5
15	19 3 56	23 24	18 52 28	15 36
18	19 5 46	23 30	18 53 24	16 7
21	19 7 23	23 36	18 54 8	16 38
24	19 8 49	23 43	18 54 44	17 9
27	19 10 3	23 50	18 55 4	17 39
30	19 11 5	23 58	18 55 16	18 9

The above table, containing the geocentric motion of the two new planets, Ceres Ferdinandea and Pallas, for the enſuing month, is ſelected from an Ephemeris calculated by Mr. Gauſs, of Germany, of whoſe abilities as a calculator the world has already been fully convinced. Mr. Gauſs does not expect that the obſerved places of theſe planets when they ſhall be again found will agree very correctly with his Ephemeris, as there has not yet been a ſufficient number of obſervations made to ſettle the elements of their motions with ſuch a degree of preciſion; but he hopes his calculations will render them very eaſy to be found: thoſe gentlemen, therefore, who are provided with equatorials, or inſtruments for taking angles out of the meridian, will no doubt be ſoon able to find both, or at leaſt Pallas, whoſe ſituation is now become pretty favourable, being at a ſufficient diſtance from the ſun to be ſeen in the morning when the weather is clear. It does not appear likely that Ceres Ferdinandea, from her great ſouth declination, will be ſeen here till the latter end of April, or perhaps the beginning of May; but thoſe aſtronomers who are ſituated in more ſouthern latitudes will view her under more favourable circumſtances, and may perhaps be able to find her ſooner.

HUMBOLDT'S TRAVELS.

Where Alexander von Humboldt may be at preſent is not known even to his relations; but all the obſervations which he collected up to the beginning of March 1801 have lately arrived ſafe in Europe. He entruſted this valuable depoſit, which conſiſts of four bound volumes and three manuſcript bundles, a roll of plans, and a number of drawings of plants, to one of his friends at the Havannah, where he then was, to be ſent to Spain by a proper opportunity. They arrived in the beginning of September 1801, and have ſince been tranſmitted to Berlin.

FOSSIL BONES.

Two horns, with the head and ſeveral bones of an unknown animal of a monſtrous ſize, have lately been found in the Mologi circle, of the government of Jaroſlawi, on the banks of the Mologa, on an eſtate belonging to count Alexei Iwanſwitch Puſchkin. The length of the head is two ells Leipſic meaſure *, and the breadth on the forehead one ell eight inches, (two feet five inches.) The horns are like thoſe of an ox, are about four ells in length (ſix feet eleven inches), and in the thickeſt part are about an ell in circumference (one foot nine inches). Gmelin, as is well known, ſays that, beſides bones of the mammoth, (different however from thoſe of the American mammoth,) heads with horns of an extraordinary ſize are ſometimes found in Siberia. He himſelf carried with him to Peterſburgh a head of this kind, which in all probability is ſtill preſerved in the cabinet of curioſities belonging to the Academy of Sciences. It will be intereſting to natural hiſtory to compare this head with the one lately found, in order to aſcertain whether they belong to the ſame ſpecies of animal. At any rate, theſe heads ſeem to afford a new proof, in oppoſition to the opinion of Daubenton, Buffon, and other naturaliſts, that there exiſted formerly ſpecies of animals now become extinct.

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* About one yard ſix inches.

ANOTHER NEW METAL.

Profeſſor Tromſdorff, of Erfurt, has publiſhed the following notice:—"One of my mineralogical friends, whoſe name I ſhall make known at a proper time, found lately a particular ſubſtance which he tranſmitted to me for examination. Though the quantity was ſmall, I am however convinced that it is a *new metal* combined with ſulphur. The characteriſtic marks of this metal are: It belongs to the volatile metals: with ſulphur it forms a maſs which melts like wax, and which cryſtallizes; with ſulphureous acid it gives a reddiſh ſolution; with nitrous acid and nitro-muriatic acid it gives a yellowiſh ſolution; muriatic acid has no action on it warm: from acid ſolutions it is precipitated green by the pruſſiate of potaſh; of a ſteel gray by tincture of galls; chamois yellow by *hydrothion-ammonia* *; by carbonate of potaſh it is precipitated as a white oxide; it is not precipitated from its ſolution by cauſtic ammonia, and it is probable that it forms with it a double ſalt. I ſhall obſerve in the laſt place, that this new ſubſtance is found in Germany. A further account of it will be publiſhed in my journal."

Erfurt,
Jan. 1803.

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* By this term we conceive the author to mean a ſolution of carbonate of ammonia, in contradiſtinction to pure (cauſtic) ammonia, which he afterwards mentions.

XXXV. *Letter from Dr.* Barton *to Profeſſor* Zimmermann, *on the faſcinating Faculty which has been aſcribed to the Rattleſnake, and other American Serpents* *.

DEAR SIR, Philadelphia, Feb. 12, 1800.

I HAVE never yet received the tranſlation which you have made of my "Memoir concerning the faſcinating Faculty which has been aſcribed to the Rattleſnake, and other American Serpents." I feel flattered by your kind notice of that little production, one of my firſt eſſays in natural hiſtory, and one to which, I confeſs, I devoted a good deal of attention. By transferring my ſentiments on the ſubject which I have touched into the language of your country, you have, no doubt, contributed to draw the attention of the learned to the diſcuſſion of the queſtion, whether ſerpents are endued with the power of faſcinating other animals.

I did not, before this day, know that our learned friend profeſſor Blumenbach, of Göttingen, had publiſhed ſome Remarks on the Memoir, in Voigt's *Magazin für den neueſten zuſtand der Naturkunde*, part ii. I have not ſeen the magazine, but have met with a tranſlation of the profeſſor's paper in the Philoſophical Magazine †, publiſhed in London by Mr. Alexander Tilloch. In this letter I am going to trouble you with ſome further remarks, which you are at liberty to make uſe of in any way you may think proper.

I ſhall not purſue the profeſſor in the preciſe order of his remarks. I ſhall firſt take notice of his defence of that paſſage in his Manual of Natural Hiſtory ‡, which I particularly examined in my memoir, and which he ſeems to think I have criticiſed with ſomewhat of ſeverity. The following are Mr. Blumenbach's words, as I have tranſlated them in the memoir:—"That ſquirrels, ſmall birds, &c. voluntarily fall from trees into the jaws of the rattleſnake lying under them, is certainly founded in facts: nor is this much to be wondered at, as ſimilar phænomena have been obſerved in other ſpecies of ſerpents, and even in toads, hawks, and in cats, all of which, to appearance, can, under particular circumſtances, entice other ſmall animals by mere ſteadfaſt looks. Here the rattles of this ſnake (the rattleſnake) are of peculiar ſervice; for their hiſſing noiſe cauſes the ſquirrels, whether impelled by a kind of curioſity, miſunderſtanding, or dreadful fear, to follow it, as it would ſeem, of their own accord. At leaſt," continues Mr. Blumenbach, "I know, from well-informed eye-witneſſes, that it is one of the common practices among the younger ſavages to hide themſelves in the woods, and, by counterfeiting the hiſſing of the rattleſnake, to allure and catch the ſquirrels*."

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* Communicated by Dr. Barton.

† For December 1798.

‡ Handbuch der Naturgeſchichte.

On this paſſage I made ſome remarks, which were brought together under three diſtinct heads. I obſerved, 1ſt, That "the faculty of faſcinating is by no means peculiar to the rattleſnake, but is attributed as extenſively to the black ſnake, and other ſerpents, which are not furniſhed with the crepitaculum, or ſet of bells." 2dly, That "ſome perſons, who have ſeen the rattleſnake in the ſuppoſed act of charming, aſſure me that the reptile did not ſhake its rattles, but kept them ſtill;" and laſtly, I was inclined to think that there was no ſolid foundation for the ſtory upon which, in part at leaſt, Mr. Blumenbach has founded his theory.

It is certainly the prevailing opinion in this country, that the black ſnake and other ſerpents, as well as the rattleſnake, are endowed with the faculty of faſcinating or charming other animals. It is well known that none of the ſerpents except the different ſpecies of crotalus are furniſhed with the *crepitaculum caudæ*, or rattle, and no attempt has been made to point out the difference (if there be a difference) in the modes of faſcinating employed by theſe ſeveral ſerpents, with the exception of the remark made by a few of the Indians and Whites, that the rattleſnake charms with its rattle†. But we ſhall afterwards ſee that this is by no means the general opinion among the Indians or Whites.

Since the printing of my memoir, I have been aſſured by a very intelligent perſon ‡, who, living in a part of the country which abounds in rattleſnakes, has had many opportunities of attending to the manners of theſe ſerpents when watching for their prey, that at ſuch time the rattleſnake does not move his rattle, but lies ſtill. This is a confirmation of the fact mentioned in my memoir. "It is very probable," however, as M. Blumenbach obſerves, "that the caſe here may be altered by circumſtances;" that is, that the reptile whilſt endeavouring to obtain its prey ſometimes ſhakes its rattle and ſometimes keeps it ſtill. I am now, however, fully perſuaded that the latter is the general caſe; and the more ſo, becauſe it ſeems to be aſcertained that the rattleſnake ſeldom ſhakes his rattle unleſs when he is irritated.

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* See a Memoir, &c. pages 46 and 47.

† See a Memoir, &c. pages 14 and 15.

‡ Mr. Samuel Preſton. "Dr. Mead (ſays Mr. Preſton) ſuppoſes the rattle on their tail to be uſeful for that purpoſe (viz. charming); but he is much miſtaken, as I have actually ſeen them engaged in the proceſs a number of times. They do not make any noiſe with their rattle; they lie perfectly ſtill, with an open mouth and ſparkling eyes."—Letter to me, dated Stockport, Auguſt 7, 1798.

If then this ſerpent does not always, nor even generally, move his rattle at the time he is watching for his prey, it is, I think, ſomewhat unphiloſophical to aſcribe ſo much to the "hiſſing noiſe" of the inſtrument as Mr. Blumenbach has done. "This lazy animal, when lying on the ground, might certainly," ſays the profeſſor, "employ that ſingular organ for enticing animals, as well as the ceraſtes employs its horns for the ſame purpoſe, at leaſt according to common report."

I will readily allow, that if the ceraſtes of Egypt is capable of charming by means of the horns upon his head, the rattleſnake might, without much ſtretch of the imagination, be ſuppoſed capable of charming by means of the horny bells upon his tail. But here Mr. Blumenbach attempts the ſolution of one difficulty by having recourſe to another difficulty; or, rather, he ſeems diſpoſed to cut the knot of faſcination, as it reſpects the rattleſnake, by means of a ſtory which has never been proved to be a fact, and which, I cannot but think, is one of the many improbable tales in natural hiſtory. I know it is ſanctioned by Pliny* and Solinus. But, alas! how many hundred anile ſtories has Pliny told! and who does not know that Solinus is often the ſervile copier of Pliny?

In the diſcuſſion of curious queſtions like the preſent, I can feel pleaſure even in furniſhing my opponent with implements with which to defend himſelf. With this diſpoſition of mind I will here mention a ſuppoſed fact, which is a good deal ſimilar to that related of the ceraſtes. I do not doubt that it is as worthy of belief. There inhabits the lakes and rivers of many parts of North America a very curious ſpecies of eſox or pike, which is commonly known by the name of the gar fiſh or bill fiſh. The upper jaw is lengthened out into a long bony roſtrum, or protuberance, which has given it the name of *bill fiſh*. Although this fiſh is known to live almoſt entirely upon other fiſh, to which he proves very deſtructive, we are told that the gar hides himſelf in the reeds in ſuch a manner, that nothing but the curious roſtrum, which he thruſts out of the water in a perpendicular poſition, can be ſeen. Different kinds of birds, which come to reſt themſelves upon the reeds, miſtaking the fiſh's bill for a reed, or dry piece of wood, perch upon it alſo. He then opens his mouth, and generally makes an immediate prey of the miſguided bird.

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* "Ceraſtis corpore eminere cornicula ſæpe quadrigemina: quorum motu, reliquo corpore occultato, ſolicitent ad ſe aves." C. Plinii Secundi Naturalis Hiſtoriæ Lib. viii. cap. 23.

This ſtory is related by a very reſpectable writer, father Charlevoix, who adds a circumſtance which I muſt not omit to mention, eſpecially as it would ſeem to ſhow that there is ſome hidden (we will call it a *faſcinating*) virtue in the gar's bill. The Indians ſay, that the ſharp teeth which are diſtributed along the edges of this inſtrument are a "ſovereign remedy againſt the head-ach, and that pricking with one of theſe teeth where the pain is ſharpeſt, takes it away inſtantly *."

But to be ſerious: As moſt, if not all, animals are furniſhed with an organ of hearing, ſo it is not improbable that different ſounds, particularly the ſounds of muſical inſtruments, have ſomething charming or attractive to certain ſpecies of animals. Paſſing by the ſtories that are told of the *trichechus manatus*, or ſea-cow, the common mouſe, and ſome other animals, I will here mention a fact related by Dr. Odier on a very reſpectable authority, and I relate it the more confidently becauſe it has been confirmed to me by ſeveral perſons of credit. The iguana † of the Weſt Indies is ſaid to be ſo fond of muſic, that at the ſound of an inſtrument this ugly looking lizard becomes almoſt motionleſs, and is eaſily taken by a nooſe ‡.

But I muſt return to Mr. Blumenbach. "I know," ſays he, "from well-informed eye-witneſſes, that it is one of the common practices among the younger ſavages to hide themſelves in the woods, and, by counterfeiting the hiſſing of the rattleſnake, to allure and catch the ſquirrels." After quoting this paſſage, I obſerved in my memoir, that "I have inquired of Indians, and of perſons who have reſided for a conſiderable time among the Indians, and they appear to be as ignorant of the circumſtance as I am myſelf." I continued, "I am inclined to think that Mr. Blumenbach has been impoſed upon," or, perhaps, that a circumſtance which I have related may have given riſe to the ſtory *. Mr. Blumenbach has ſince informed me by letter, and now informs the public in his Remarks, that he received his "information from major Gardner, who, with his family, reſided many years in Eaſt Florida. He is," ſays the profeſſor, "a very intelligent naturaliſt, an accurate obſerver, and certainly would be very far from impoſing upon me."

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* A Voyage to North America, vol. i. pages 117 and 118. Engliſh tranſlation.

† Lacerta iguana of Linnæus.

‡ See Epiſtola Phyſiologica, Inauguralis, de Elementariis Muſicæ Senſationibus, Nota 32, Edinburgi 1770. It were much to be wiſhed that ſome ingenious man would favour us with a memoir concerning the influence of muſic upon different animals. Some intereſting materials for ſuch a work are to be found ſcattered through a number of writers both antient and modern. But new materials might readily be collected, ſince it would not be a difficult taſk to make experiments. Much curious phyſiological knowledge would reſult from ſuch an inquiry: and I greatly miſtake if the inquiry would not ſomewhat tend to diminiſh the quantity of our prejudices againſt animals that are unqueſtionably innocent. Should it have this effect, how great would be the gain to a benevolent mind! Nurtured among prejudices of different kinds, we are at once miſerable and unjuſt. I muſt confeſs that I have dropped ſome of my prejudices againſt the amphibia (See a Memoir, &c. p. 45, note) ſince I have learned that the iguana is pleaſed with the muſic of the Weſt Indians; and that a little garden lizard liſtened, "with a breathleſs attention," to the ſound of a lady's piano-forte.—For a very intereſting inſtance of this kind, ſee the Analytical Review for January 1789.

I knew not, when I printed my memoir, from whom Mr. Blumenbach received his information. It was not, however, unnatural for me to ſuppoſe, that he had been "impoſed upon," becauſe I well know that ſome of the moſt reſpectable naturaliſts and hiſtorians of Europe have often been moſt groſsly deceived by travellers who have viſited this country: hence the many tales and ſcandalous ſtories which crowd and deform ſome of the works of your moſt celebrated writers. Such tales and ſtories are the following:—That the Indians have no beards; that they have very ſmall appetites; that they are greatly addicted to the "antiphyſical vice;" that *none* of the tribes knew any thing of the uſe of ſalt before the Europeans came among them; that they cannot carry their arithmetic beyond the numeral three; &c. &c. &c.

I have lately made further inquiries of the Indians concerning the ſtratagem which, Mr. Blumenbach ſays, the younger ſavages employ to allure to them ſquirrels. I can learn nothing concerning it. I am ſtill diſpoſed to think that there is but a ſlender foundation for the ſtory. I am certain that it is not a *common* practice among the Indians. Perſons who have reſided for many years among our northern and weſtern Indians have never heard of it: neither have intelligent traders and interpreters from the very country in which major Gardner reſided. Anxious to aſcertain the truth, I ſhall extend my inquiries, and, ſhould I learn that the Indians do actually employ the ſtratagem, I ſhall endeavour to be the firſt to inform Mr. Blumenbach of the ſucceſs of my reſearch.

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* See a Memoir, &c. pages 48 and 49.

I ſhall conclude this part of my letter with two obſervations; viz. 1ſt, If it be a fact, as I have aſſerted, that the rattleſnake, while watching for his prey, ſeldom moves his rattle, the ſtory related by major Gardner muſt, independently on any other facts, appear highly improbable. 2dly, Even admitting the fact, that the rattleſnake does ſhake his rattle while employed in the ſuppoſed act of charming, I think the greater number of the favourers of the exiſtence of a faſcinating quality in this ſerpent ought, upon a careful conſideration of the ſubject, to give up Mr. Blumenbach's explanation of the buſineſs. They tell you that the bird or ſquirrel is often ſeen precipitating itſelf from the top of a lofty tree into the jaws of the ſerpent lying at the bottom. Now, is it likely that the noiſe of the rattle can be diſtinctly heard at the diſtance of ſixty, eighty, or a hundred feet, in a foreſt where all elſe is not ſilence; in a foreſt where rooks, and ravens, and jays, wood-peckers, and many other ſpecies of birds, utter their various cries or notes, which are mixed, and often confounded, with the noiſe of tree-frogs, locuſts, and a hundred other animals? I think, ſir, you will admit with me, that it is very improbable, if not impoſſible, that the rattle could affect the bird at ſuch a diſtance; and I am diſpoſed to believe, that, after reading the preceding obſervations, you will not think Mr. Blumenbach's ſyſtem is ſo capable of being maintained as that ingenious gentleman ſeems to ſuppoſe it is.

Mr. Blumenbach, in taking notice of my theory of accounting for the ſuppoſed faſcinating power of ſerpents, informs us, that with this method of explaining the phænomenon he has been acquainted ſince 1785, from an eſſay by profeſſor Michaëlis, in the Göttingen Magazine for January of that year. In juſtice to Dr. Michaëlis, I ſhall here quote his words, as they are given by Mr. Blumenbach:—" Others believe that it is owing merely to the care of the old ones for their young, which throw themſelves between the latter and their enemies, and by theſe means become a prey to them. One of my friends, Mr. David Colden, at Fluſhing, an amateur of natural hiſtory, and ſon of governor Colden, whoſe ſervice to ſcience is ſo well known, aſſured me that he had ſeveral times ſeen birds faſcinated by ſnakes, but always found the neſt of the bird either with eggs or young ones in the neighbourhood; which made the ſpectators give up the idea of faſcination. But," Mr. Michaëlis adds, " I know ſome inſtances where no neſt could be in the neighbourhood, and where, though the ſnake was at firſt at a great diſtance from the bird, it nevertheleſs fell towards it."

From this quotation it appears that I was not the firſt perſon who endeavoured to explain the ſuppoſed faſcinating power ſomewhat in the manner I have done. I have not aſſerted that I was. But I certainly neither did nor could borrow the explanation from Mr. Michaëlis, whoſe "valuable eſſay," as Mr. Blumenbach calls it, I have never yet ſeen. My theory, which every day's inquiry ſerves to ſtrengthen, was the reſult of a great deal of attention to the ſubject: and I have enjoyed as many opportunities of inveſtigating the truth as Mr. Michaëlis did. With reſpect to that ingenious gentleman, I ſhould have allowed him more merit had he adopted Mr. Colden's explanation; and I can allow him very little for rejecting it, merely becauſe, in ſome inſtances, no neſt could be found in the neighbourhood, and becauſe, "though the ſnake was at firſt at a great diſtance from the bird, it nevertheleſs fell towards it." It ſurely does not follow, becauſe no neſt could be found, that none exiſted. The ſchoolboy well knows the difficulty of diſcovering the neſts of many ſpecies of birds; and the naturaliſt, who ought to be acquainted with the arts employed by theſe animals to conceal, from man and other enemies, their neſts, ſhould make ſtill greater allowance for the difficulty of diſcovering theſe neſts. As to Mr. Michaëlis's other aſſertion, that, "though the ſnake was at firſt at a great diſtance from the bird, it nevertheleſs fell towards it," I will not poſitively deny it, until I learn whether that gentleman has himſelf witneſſed any thing of the kind. But, in the meanwhile, I muſt ſay, that I have no reaſons to think that I have been precipitate in advancing what I have advanced on this ſubject in my memoir.

I do not perceive that Mr. Blumenbach has made any other attempt to controvert my theory, except in ſo far as he has mentioned Mr. Michaëlis's two obſervations juſt noticed. On the contrary, in the new edition of his Manual, the profeſſor has quoted my memoir, and expunged the ſtory about the ringing of the ſerpent's tail *. Of Mr. Michaëlis's eſſay he makes no mention. And here, before I adduce any additional facts in ſupport of my explanation, I cannot forbear to obſerve, that I do not think Mr. Blumenbach has done juſtice to this part of my memoir. In particular, the fine fact communicated to me by the late Mr. Rittenhouſe, of which he has made no mention, is worth a whole volume of ſpeculations on the ſubject. I doubt not that my learned Göttingen friend had as high an opinion of the fact as I have.

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* Handbuch der Naturgeſchichte, p. 242; Göttingen 1797.

Before I proceed any further in my reply to Mr. Blumenbach, I beg leave to trouble you with ſome facts which are a good deal ſimilar to thoſe related in my memoir. They certainly favour the ſyſtem which I have advanced. Independently, however, of their connection with the ſubject of the memoir, they appear worthy of preſervation; for they ſerve to illuſtrate, in ſome degree, the hiſtory of the manners and inſtincts of ſerpents; a ſubject which has been too much neglected by naturaliſts.

A ſpecies of coluber, which is commonly called the chicken ſnake in the ſouthern parts of the United States, of which it is a native, frequently climbs up the loftieſt trees in purſuit of young birds. One of my friends [*], when he was in Georgia, ſeveral years ſince, had an opportunity of ſeeing one of theſe ſnakes in a ſituation which furniſhes a ſtriking argument in favour of my opinion. The active reptile having ſeized upon a young martin [†] (which had left its neſt for ſome days), upon a walnut-tree, at the height of about thirty feet, had not a little difficulty in ſwallowing the young bird. Having taken in the head firſt, as is commonly the caſe with our ſerpents, the bird made great reſiſtance by the flapping of its wings, ſo that the ſerpent could only ſwallow the head and neck. Whilſt the wings of the bird were in motion, numbers of the old martins collecting together flew about the ſnake and attacked him with their bills. Here, as in the inſtance related by Mr. Rittenhouſe, the old birds were actuated by the inſtinct of ſaving their young.

The following facts were communicated to me by Mr. John Heckewelder:—"In the ſummer of 1770," ſays this gentleman, "while I was fiſhing under the bank of Leheigh [‡], I heard, for the ſpace of near an hour, the ſound of a ground-ſquirrel, ſeemingly in diſtreſs, on the top of the bank. At length I went up to ſee what was the matter with the ſquirrel; when, to my utter aſtoniſhment, I diſcovered the animal about half way up a buſh, but running ſometimes higher up, ſometimes lower down, and a very large rattleſnake at the root of the buſh on which the ſquirrel was. Here I was immediately ſtruck with the idea that the ſnake was in the act of inchanting, and I hoped now to become fully convinced that the rattleſnake obtained its prey altogether in this manner, as I had often heard reported. I therefore ſat down quietly on a log about ſix yards diſtance, where I had a full view of both the ſnake and the ſqurirel. Sometimes I thought the ſquirrel going down for the laſt time, and to enter the jaws of the ſnake; but it would again return up the buſh with the ſame livelineſs it had run down. Finding, finally, no material alteration in the ſquirrel or its motions, and my patience being exhauſted, I determined on killing the ſnake, and examining into the caſe of the ſquirrel, viz. what ſtrength, &c. it yet retained after being charmed for ſo long a time; for, by this time, the ſuppoſed charm had laſted near three hours. I ſtruck at the ſnake with a long pole, but miſſed it; upon which it ran down the bank where I had been fiſhing. Remaining by the buſh on which the ſquirrel was, I hailed a man on the oppoſite ſide of the river, deſiring him to croſs in a canoe and kill the ſnake under the bank. To which he immediately complied; but likewiſe miſſing his ſtroke, on account of the buſhes, the ſnake took up the bank again, where I killed it. We now both joined to ſhake the ſquirrel down; but it had both ſenſe and ſtrength enough to climb to the very top, I ſuppoſe near twenty feet high. However, we brought it down to the ground; and though it had fallen about two yards from the buſh, it well knew its hole in which it dwelt, and this was at the root of the buſh, and exactly at the ſpot where the ſnake had lain. Here the myſtery was cleared up to us at once. We conjectured that the ſnake was either watching for the ſquirrel to come down to enter its hole, or for its companion or young, which were probably in the hole, to come out; all of which were ſufficient to cauſe anxiety in the ſquirrel on the buſh. The dexterity, however, of the ſquirrel in making its way into the hole, and at the very place where we ſtood, ſhowed plainly that it retained its full ſtrength and ſagacity, and had by no means ſuffered from the charm of the ſnake[*]."

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[*] The ingenious Mr. William Bartram.

[†] Hirundo purpurea.

[‡] The principal weſtern branch of the Delaware, which runs by Bethlehem.

"A ſimilar circumſtance, to which I was alſo an eye-witneſs, happened," ſays Mr. Heckewelder, "in the year 1771, near Wyaluſing, on Suſquehanna, where the cries of the chewink[†] drew my attention to the ſpot. The rattleſnake was juſt entering a heap of bruſh, in which the old ones had their neſt with young. I ſuppoſed that one or the other of the old ones, with the young, would have become its prey, had I not approached and relieved them by killing the ſnake[‡]."

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[*] Letter to me, dated Bethlehem, Auguſt 5th, 1796. Mr. Heckewelder has ſince informed me, that the ſnake, during the whole of the time he attended to it, never ſhook its rattle.

[†] The Fringilla erythrophthalma of Linnæus.

[‡] Letter to me, dated Auguſt 5th, 1796.

The following fact is ſimilar to one which I have related in my memoir*:—"In the year 1762, at Tuſcarawas, on Muſkingum, while going to fetch water out of the river, I obſerved," ſays Mr. Heckewelder, "a large black ſnake running out on a long limb of a large tree which ſtood on the water's edge. This limb was nearly horizontal over, and about twelve feet above the level of, the water. I could not, at firſt, conceive what the ſnake aimed at, until near the end of this limb I ſaw the animal ſtretch downwards, where I obſerved a hanging† bird's neſt, pretty well concealed between ſome ſmall boughs or leaves, into which the ſnake put its head, having ſtrung its tail, with part of its body, round the limb above. Determined on killing the ſnake, if poſſible, I ran to the houſe for my gun, and ſhot the reptile, which fell into the river, with a young bird in its jaws."

"I and another perſon once obſerved a ſnake of this kind run up a tree pretty high, and put its head into a woodpecker's neſt, where, as we ſuppoſed, it ſucked the bird's eggs, it being too early for the young birds to be hatched‡."

On theſe facts I ſhall not trouble you with any comments, but ſhall proceed with my examination of Mr. Blumenbach's Remarks.

[To be continued.]

XXXVI. *Obſervations on the different Theories of Philoſophers to explain the Phænomena of Combuſtion.* *By* CHARLES PORTAL, *Eſq.*§

THERE is no phænomenon in nature that has hitherto engaged the attention of philoſophers with leſs ſucceſs in the elucidation of its principles than that of combuſtion, and it at preſent affords one of the chief obſtacles to the forming of a clear and ſatisfactory theory of chemiſtry. It is not ſingular, that a ſubject of ſo ſtriking a nature, and abounding with a variety of ſuch important phænomena, ſhould have early attracted the attention of philoſophers, and we accordingly find that it was made the ſubject of inveſtigation by lord Bacon and Mr. Boyle.

Theſe two philoſophers were, however, more particularly confined in their inquiries to the nature of the unknown element called *fire*, nor did they reach ſufficiently far in ſuch inquiries as to frame any particular theory on the ſubject of combuſtion.

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* See pages 67 and 68.

† Oriolus Baltimore of Linnæus.

‡ Letter already referred to.

§ Communicated by the Author.

In referring back to the earlier periods of the hiſtory of chemiſtry, we ſhall find that Becher was the firſt philoſopher who withdrew that ſcience from the contracted limits of pharmacy and alchemy, and laid the foundation of the doctrine of phlogiſton.

G. Erneſt Stahl, whoſe genius was formed for the higheſt improvement of ſcience, ſucceeded him; and it is to this philoſopher that we are indebted for the firſt poſitive attempt to explain the phænomena, and to exhibit a regular theory of combuſtion.

The doctrine of phlogiſton, as expounded by this celebrated chemiſt, is too well known to require being deſcribed at any conſiderable length.

It proceeded on the aſſumption, that there was only one ſubſtance in nature capable of combuſtion, which he therefore called phlogiſton; and he held, that all bodies that were inflammable owed their combuſtibility only to the preſence of this principle. Combuſtion, therefore, he conſidered merely as its ſeparation in the form of light and heat; and ſuch bodies as were not inflammable were thought to be devoid of it: for during the combuſtion of ſubſtances he taught that their phlogiſton flies off, and the incombuſtible parts of them alone remain behind. Thus, if iron be expoſed to a ſufficiently ſtrong heat it will undergo combuſtion, a complete alteration will take place in its conſtituent parts, and a reſiduum will be found left of an incombuſtible nature.

Stahl explained theſe phænomena in the following way: Iron he conſidered as a peculiar earth united to a certain proportion of phlogiſton; when it was made to undergo combuſtion, the phlogiſton which formed a part of it, and to which it alone owed its combuſtibility, made its eſcape, and there was left behind only the baſe, which was found to be no longer inflammable. Now, as this ſeparation was always attended with the emiſſion of light and heat, phlogiſton was conſidered only as heat and light combined with ſome other ſpecies of matter in a peculiar and unknown way.

This doctrine was conſidered as deriving much ſupport from the fact, that a body, after having undergone combuſtion, (*i. e.* by the diſſipation of its phlogiſton in the form of light and heat,) was converted into a body that was no longer combuſtible, but which might again have its combuſtibility reſtored by the addition of any inflammable matter. Thus, in the example before adduced, if, after the iron has undergone the proceſs of combuſtion, and formed a reſiduum that is incombuſtible, we heat this latter ſubſtance with charcoal (an inflammable body), the metal will be again revived—phlogiſton is imparted to it from the charcoal, and we again procure a combuſtible ſubſtance. Thus, the light and heat which are evolved during combuſtion are ſuppoſed to proceed from the burning body, and to be occaſioned by the ſeparation of phlogiſton from the baſe to which it is united.

Such is the outline of that theory of combuſtion the foundation of which was firſt laid by Becher, and afterwards reduced into a regular ſyſtem by the immortal Stahl. The phlogiſtic doctrine of this chemiſt was univerſally adopted until the middle of the preſent century, when the diſcoveries of Mayow and Hooke, of Prieſtley and Lavoiſier, led the way to a new theory of combuſtion, which ſoon diſplaced the former, and eſtabliſhed itſelf on its ruins. Stahl, from being wholly occupied in demonſtrating the exiſtence of phlogiſton, ſeems to have paid no attention to the influence of air on combuſtion. The experiments of Boyle and of Hales had already pointed out the influence of this element on many of the phænomena of combuſtion, and the increaſe of weight that bodies acquire during ſuch a proceſs, is a fact totally irreconcileable with the theory of phlogiſton.

Many attempts, however, were made to overcome the weight of this objection by the diſciples of Stahl; and they even had recourſe to the ſuppoſition that phlogiſton was the principle of levity, and that, when abſtracted from any body, that body, by loſing ſo much abſolute levity, became heavier. So ſtrongly were they fettered by the tenets of their maſter, that, without ſubmitting to the labour of inveſtigation, they implicitly adopted any illuſtration that was conformable to his ideas; affording a ſtrong proof of the deſire that pervades the human mind to reduce every thing to firſt principles, and to adopt haſty generalizations, without having recourſe either to an extenſive collection of facts or the more certain evidence of accurate experiment.

Whilſt other chemiſts were intent on reconciling the various diſcoveries that had been recently made with the hypotheſis of Stahl, Lavoiſier (of a bold and creative genius) was led to call into queſtion the very exiſtence of phlogiſton itſelf. This aroſe from his having diſcovered that during the proceſs of combuſtion a portion of air conſtantly enters into union with the body which is made to undergo this operation, and that the weight of the air which diſappears in the proceſs is exactly equal to the increaſe of weight gained by the body that has undergone combuſtion. Theſe important facts led to the adoption of a new theory of combuſtion well known at preſent by the name of its illuſtrious author.

The theory of Lavoiſier is founded on the abſorption of oxygen by a combuſtible body, and proceeds upon the following principles:

1ſt, That combuſtion never takes place without the preſence of oxygen gas.

2dly, That in every combuſtion there is an abſorption of oxygen gas.

3dly, That there is an augmentation of weight in the products of combuſtion equal to the gas abſorbed; that the oxygen likewiſe imbibed by the combuſtible body may be again recovered from the compound formed, and the weight regained will be equal to the weight which diſappeared during combuſtion.

And, laſtly, That in all caſes of combuſtion there is a diſengagement of light and heat.

Theſe facts have been eſtabliſhed by the moſt accurate experiments, and are too well known to require any further illuſtration; they incontrovertibly prove the general principle, "that combuſtion is only a play of affinities between oxygen gas, caloric, and the baſe of the combuſtible body. It is a further part of this theory, that the light and caloric which are evolved during combuſtion proceed from the oxygen gas, and that they are not emitted, as ſhould follow from the phlogiſtic doctrine, from the combuſtible body.

As this theory, however beautiful, is not capable of explaining with clearneſs and accuracy many of the phænomena of combuſtion, Dr. Thomſon, of Edinburgh, has lately (ſee Nicholſon's Journal, New Series, for May and June 1802,) offered another which places this ſubject in a point of view ſomewhat different, and which certainly bids fair to enable us to eſtimate the phænomena of combuſtion with more ſucceſs than has hitherto been done. Chemiſts have been lately accuſtomed to give to the term combuſtion, according to the foregoing theory, a new meaning, and to make it ſtand for the general combination of a body with oxygen. Nothing, however, can be more evident than the difference that in numberleſs inſtances prevails between the act of oxygenation of bodies and that of combuſtion, inaſmuch as neither the phænomena attending them, nor the reſults ariſing therefrom, are the ſame.

It is probable that this error has ariſen from the conſideration that all bodies during their combuſtion combine with oxygen, without at the ſame time recollecting that this latter effect may be produced without any of the phænomena uſually attendant on combuſtion; and that, though certainly all combuſtion preſuppoſes the combination of oxygen with a baſe, yet this combination may be, and repeatedly is, effected where no combuſtion can poſſibly take place.

It is the object, therefore, of Dr. Thomſon's theory to point out the difference which in numberleſs inſtances prevails between the act of oxygenation of bodies and that of combuſtion, and particularly to account, in a more ſatisfactory manner than has hitherto been done, for the emiſſion that takes place during combuſtion of light and caloric. The two following are the leading poſitions of the doctor's theory:

1ſt, That during combuſtion all combuſtibles emit light, which previouſly formed a neceſſary ingredient to their own compoſition: and,

2dly, That the heat which is evolved during the proceſs of combuſtion proceeds from the decompoſition of oxygen gas.

It has been before obſerved, that by the phlogiſtic theory the light and heat are ſuppoſed to proceed from the combuſtible body; but that by the theory of Lavoiſier they are held to proceed from the decompoſition of oxygen gas, of which body they are conſidered as forming conſtituent parts.

In the infancy of chemical knowledge, and before the diſcovery of vital air, that the extrication of heat proceeded from the combuſtible body, was the only natural concluſion that could preſent itſelf to the mind; and as light and heat were conſidered as only modifications of the ſame ſubſtance, the ſuppoſition of courſe prevailed that both were evolved from the inflammable body.

Since, however, the later experiments of philoſophers, and particularly thoſe of Drs. Herſchel and Woolaſton, have diſproved this ſuppoſed identity, it evidently becomes no longer neceſſary to trace theſe two ſubſtances to the ſame ſource; and this, as is before ſtated, is the caſe with the theory we are now reviewing.

That the caloric which is evolved during combuſtion ſhould proceed from the decompoſition of the oxygen gas, is, amongſt a variety of other reaſons, rendered the more probable from the conſideration that bodies poſſeſs a greater ſhare of caloric in the gaſeous ſtate than in any other; and conſequently the probability is greatly ſtrengthened, that the heat which is evolved during the proceſs of combuſtion proceeds rather from the oxygen gas than from the inflammable matter, and that it is from the condenſation of oxygen gas that caloric is chiefly evolved; or, according to the explanation of Lavoiſier, that the oxygen of the gas, poſſeſſing a ſtronger affinity for the baſe of the combuſtible body than for caloric, is thereby attracted, and that the heat combined with it is conſequently ſet at liberty, and diffuſes itſelf among the adjacent bodies.

To the ſecond poſition of Dr. Thomſon, that the light invariably proceeds from the combuſtible body, and not from the oxygen gas, there are much ſtronger objections, and ſuch as, with our preſent collection of chemical facts, are, I apprehend, not readily to be explained.

It is a part of the theory of the doctor, that oxygen gas poſſeſſes no light: indeed this is ſo material a part, that, unleſs it can be ſubſtantiated, the theory is inadequate to elucidate the phænomena intended by it.

How ſhall we, however, reconcile the following facts on the above ſuppoſition? If nitric acid be expoſed to the light, it changes colour; it firſt grows yellow, afterwards green, and laſtly red, and oxygen gas is diſengaged; and on examining the acid we find that it is converted from nitric into nitrous acid.

It is evident that, as this decompoſition is of a chemical nature, the light that occaſions it either combines with the oxygen to form oxygen gas, or with the acid to form nitrous acid: that the latter is not the caſe we are juſtified in ſuppoſing, becauſe we find no difference between acid ſo procured and that gained by any other means, and we are neceſſitated to conclude that the light has combined with the oxygen, and that the latter is by this mean converted into oxygen gas. The ſame reaſoning may be applied to the converſion of oxygenated muriatic acid by light into ſimple muriatic acid. Unleſs theſe facts can be otherwiſe ſatisfactorily accounted for, the poſition of Lavoiſier ſtill remains with all its force, that light is a conſtituent part of oxygen gas. That light forms a neceſſary part in the conſtitution of many bodies, is a fact too well authenticated to be refuted. The experiments of the Dutch chemiſts, who heated together the filings of different metals with ſulphur under mercury; the phænomena of the pyrophori; the inflammation that takes place from the action of many of the acids on the oils; and the recent experiments of Dr. Hulme, illuſtrate this fact beyond the poſſibility of contradiction.

After a body has undergone combuſtion, we learn from this theory that it is deprived of light, and that it is only by means of a combuſtible body that light can be again transferred to the product of combuſtion. Thus water is conſidered by Dr. Thomſon as a product of combuſtion, and conſequently deprived of light. If, ſays he, we decompoſe this water by means of iron or zinc, we reſtore it to its former combuſtibility by occaſioning it to imbibe light, which is afforded it by the metal. If ſuch decompoſition is effected by means of iron, 73 parts of this latter unite with 27 of oxygen. Now, as every 27 parts of oxygen require about four of hydrogen to form water, of courſe theſe four parts of hydrogen are liberated during ſuch decompoſition: but, as it is not probable that combuſtibles are capable of combining with light in all proportions, it may be aſked, Whether the 73 parts of iron which are oxidated contain juſt light enough, and no more, to reſtore the combuſtibility of the four parts of hydrogen? for, if there be too much for that purpoſe, the ſuperabundant quantity ought to become viſible; and if too little, a part only of the hydrogen ſhould recover its combuſtibility, and be converted into gas. The ſame reaſoning may of courſe be urged with regard to the decompoſition of water by any other metal; for it is ſomewhat ſingular, that the combuſtible ſhould always contain and give out the preciſe quantity of light that is ſufficient to reſtore combuſtibility to the baſe of the product of combuſtion, and in no caſe either more or leſs. Theſe, as well as ſome other objections to the above theory, were pointed out by me in a paper entitled Remarks on Dr. Thomſon's Theory of Combuſtion, and inſerted in Nicholſon's Journal for the month of July 1802.

It ſhould appear, then, that we are not in poſſeſſion of a ſufficient collection of facts to enable us to form any theory of combuſtion that is perfectly free from objection, and calculated to explain all the phænomena attendant on it. Whether the light evolved during combuſtion is emitted from the combuſtible body or from the oxygen gas, muſt ſtill be conſidered as a matter of doubt; for the ſuppoſition ſeems perhaps the more probable, that it is partly derived from both. The ſubtile nature of light itſelf is highly adverſe to any inquiry into its real eſſence. The ſame obſervation will likewiſe apply to caloric. The theory laſt noticed is, however, poſſeſſed of a high degree of merit; it ſatisfactorily explains many phænomena that were before but little underſtood; it points out, with ſufficient perſpicuity, the difference that prevails between the act of oxygenation in bodies and that of combuſtion; and though certainly liable to ſome objections, it approximates the neareſt to a ſatisfactory explanation of the phænomena of combuſtion of any that have hitherto been offered.

CHARLES PORTAL.

⁂ Mr. Portal is probably not aware that the theory which he attributes to Dr. Thomſon as its author was firſt propoſed by Dr. Gren, whoſe doctrine is briefly as follows:

Oxygen gas is compoſed of *oxygen and caloric—Combuſtible bodies* conſiſt of *their reſpective baſes* in chemical combination with the *matter of light—Free light* conſiſts of the *matter of light* united to *caloric.*

Combuſtion, then, is a mere play of chemical affinities; the oxygen of the oxygen gas uniting with the baſe of the inflammable body to form an oxide or an acid, while the caloric of the gas unites with the matter of light of the combuſtible body, and forms free or viſible light. By this theory, no combuſtible ſubſtance can be a ſimple body—by Lavoiſier's, they may.—See Gren's *Principles of Modern Chemiſtry*, Engliſh edition, chap. ii. articles *Caloric, Light, Phlogiſton, Combuſtion.*—A. T.

XXXVII. *Hiſtory of Aſtronomy for the Year 1802. Read in the Athenæum of Paris December 30, by* JEROME LALANDE.

[Concluded from p. 179.]

ON the 7th of June I publiſhed the two laſt volumes of the large Hiſtory of the Mathematics by Montucla. The fourth is almoſt entirely devoted to the hiſtory of aſtronomy and navigation, which I communicated to the preſent period.

There has appeared alſo, in two volumes quarto, with 23 plates, *Hiſtoire de la Meſure du Temps par les Horloges*, by Ferdinand Berthoud, mechaniſt of the marine, member of the National Inſtitute, and of the Royal Society of London. It contains a deſcription of eſcapements, compenſation balances, moving ſpheres, and of the principal inventions of clock-work. The author ſpeaks of all the celebrated clockmakers, except Louis Berthoud, his nephew, to whom we are indebted for all the time-keepers made ſince 1784. I ſhall here take occaſion to obſerve, that Mr. Emery died at London, and that his widow has four time-keepers which ſhe wiſhes to diſpoſe of: they would be a valuable acquiſition.

We have received the 7th volume of the Tranſactions of the Royal Iriſh Academy, publiſhed in 1800, and in which Mr. Young examines the ſolution given by Newton of the problem reſpecting preceſſion of the equinoxes; alſo the 8th volume of the Iriſh Tranſactions, Dublin 1802.

On the 18th of July my ſmall tables, of logarithms appeared: they are the moſt convenient and the moſt correct yet given.

The third volume of the *Mécanique Céleſte* of C. Delaplace has been printed, and was publiſhed on the 29th of December. This work will form an epoch in phyſical aſtronomy. It exhibits a ſeries of important reſearches by that great geometrican, and the perturbations of each of the planets by the action of all the reſt; with a new theory of the moon, containing, beſides other things, the diſcovery of a new inequality the period of which is 180 years, and of which I have already ſpoken.

M. Haſſenfratz has publiſhed his *Cours de Phyſique Céleſte*, or *Leçons ſur l'Expoſition du Syſtême du Monde.* He makes no mention in it of my Aſtronomy, but he nevertheleſs has borrowed from it things which he could not find any where elſe. I have corrected ſome faults in it. (*Bibliotheque Françoiſe de Pougens.*)

The Board of Longitude has ſent to the printing-office obſervations made for two years with the new inſtruments of the obſervatory by Mechain and Bouvard, to be printed in folio like thoſe which Monnier publiſhed between 1751 and 1773, and thoſe of Greenwich in England, which were worthy of ſerving as a model.

On the 3d of July M. de Roſel, a naval officer, arrived at Paris with the journals of the voyage undertaken in ſearch of La Perouſe, under the command of captain d'Entrecaſteaux.

La Grandiere alſo has brought a journal of that voyage, which the Engliſh government had a year in their hands, but which they reſtored to him. At the depôt of the marine there are 58 charts conſtructed during that voyage by Beautemps-Beaupré, hydrographer of the expedition.

In the month of June I received intelligence from C. Bernier, who was at New Holland. Captain Baudin touched only in two places in an extent of 400 leagues which he traverſed on the weſtern coaſt. He was preparing to accompliſh the remaining part of the expedition to the north and ſouth: but it appears to me that the zeal of this aſtronomer has been thwarted by the indifference of the captain; and this voyage, on which we founded great hopes, will not be ſo productive as it ought to be.

The ſhip Lady Nelſon, ſent out with the Inveſtigator, captain Flinders, which arrived at New Holland about the middle of December 1801, fell in with the Geographe, commanded by Baudin. She then found at Port Jackſon the Naturaliſte, commanded by Hamelin, and which ſailed about the middle of May to go in ſearch of the Geographe, from which ſhe had been ſeparated by a gale of wind in the

Straits of Baſſe. Captain Flinders has ſent home his obſervations, and a chart of thoſe parts of New Holland which he viſited.

Captain Flinders made a diſcovery between Van Diemen's Land and New South Wales, of which information was received in March 1802: Governor King's Iſland, Cape Alban Otway, Portland Bay, Cape Northumberland.

Joſeph Joachim de Ferrer has ſent me ſeveral poſitions obſerved on the Miſſiſſippi and on the Ohio, which will be of the more importance as the French government is employed with the means of deriving benefit from that new colony, which may be attended with immenſe advantages, as Raynal has ſhown in his *Hiſtoire Philoſophique.*

The Voyage to the Northern Part of Aſiatic Ruſſia, the Frozen Ocean, the Sea of Anadir, and the Coaſts of America, from 1785 to 1794, by commodore Billings, tranſlated by Caſtera, in two volumes quarto, has made us acquainted with countries before undeſcribed or defined. The ſhores of the Kouſina have been traverſed: a chart by M. Bauer, and Arrowſmith, the Engliſh geographer, is added. The officers complain of the commander, who would not allow them to penetrate further north, notwithſtanding the inſtructions he had received to ſearch for a paſſage through the Frozen Ocean.

Travels into the interior Parts of Africa in 1797 and 1798, by Frederic Horneman, has been publiſhed. Horneman proceeded from Cairo to Mazoul. Another French edition, with additions by C. Langles, is in forwardneſs.

Geography has been enriched alſo by A Voyage to Senegal by C. Durand, one volume in quarto, with many plates. The author was a long time at Senegal; and I have mentioned his reſearches in a memoir on Africa.

M. Sutzen, a phyſician, accompanied by M. Jacobzen, a ſurgeon, is alſo preparing to penetrate into the interior parts of Africa. They have been furniſhed with obſervations by baron Von Zach, of Gotha. The duke of Gotha has given them a ſextant of ſeven inches radius, and a time-keeper by Emery; three artificial horizons, with their levels; an achromatic teleſcope of twenty inches, and two and a quarter inches aperture; a declination compaſs, a mariner's compaſs, and a camera obſcura.

M. Sutzen will proceed from Conſtantinople to the eaſtern coaſt of Africa, where he intends to join ſome of the caravans of Zinguebar or Monœmugi. He hopes to be four or five years abſent. (Von Zach's Journal for Auguſt 1802.)

On the 17th of July we ſaw don Domingo Badia, commiſſary of war in Spain, who travels by authority of the Prince of Peace, accompanied by Roixas, a naturaliſt. He will embark in England for Morocco, and will follow the route of the caravans of the interior.

Voyages à Madagaſcar, aux Indes Orientales, et à Maroc, by Alexis Rochon, a celebrated aſtronomer and geographer, have appeared in three volumes.

The general depôt of war at Paris continues its labours with ſurpriſing activity, as may be ſeen in the Moniteur of June 25 and October 16. Generals Calon, Clark, and Andreoſſy, who had the direction of it in ſucceſſion, have been ſucceeded by general Samſon.

The map of the country between the Adige and the Adda will ſoon be followed by maps of all Piedmont, of the Italian republic, and of the iſland of Elba: thoſe of Bavaria, Helvetia, of the Valais, and of the four departments of the Rhine, are continued. More than a hundred geographical engineers are employed, among whom are the celebrated Nouet, Henry, and Tranchot.

On the 1ſt of July M. Henry quitted Bavaria, diſguſted by the oppoſition of M. Bonne. The latter is ſtill occupied with the map of Bavaria; but we loſe the meaſurement of a degree, which M. Henry propoſed to make. However, we ſhall have from the Scheldt to the Adige, and from Breſt to Munich, an uninterrupted trigonometrical outline, which will ſoon be filled up with a complete topography. A map of Egypt in fifty ſheets, and thoſe of the Morea and neighbouring iſles, are now engraving. Materials are collecting for maps of Saint Domingo and of Louiſiana.

The beautiful map *des chaſſes*, a maſter piece of engraving, of which only five ſheets were terminated, will be carried to thirteen.

A plan is even in agitation for making a new and correct edition of the large map of France in 183 ſheets, which had been too much neglected. In the mean time, thirty are retouching.

The collection of manuſcript charts at the depôt, which already amounts to 7400, daily increaſes. C. Barbier-Dubocage, already known by his important labours, turns them to the beſt advantage poſſible; and the depôt of war, encouraged by the government, is about to produce to geography immenſe riches.

The firſt number of a topographical and military memorial, edited at the general depôt of war, has been publiſhed for November 1802. It contains a notice reſpecting the geographical charts; a treatiſe on geodeſic operations; tables for reducing the angles of one plane to another; the determination of heights by the barometer.

C. Nouet, who arrived from Egypt on the 5th of January, has already ſet out for Mount Blanc, where he will continue the laborious operations which he began in 1796.

The Ruſſians are going to conſtruct a map of Eſthonia and Volhynia.

M. Weiſs has terminated his map of Swiſſerland in ſixteen ſheets.

A beautiful chart of the ſouthern part of the geometrical ſurvey, by captain Mudge and Mr. Dalby, has been engraved in England in four ſheets.

M. Ciccolini has made a tour to the coaſts of the Adriatic, where he has verified the poſitions of a great number of points. In regard to Pezaro there was an error of 30″ in time.

The travels of baron Von Zach, ſo uſeful to the geography of Germany, are detailed in the excellent journal which he publiſhes every month.

M. Mentelle has undertaken for the Firſt Conſul a terreſtrial globe a metre in diameter, which will contain all the new diſcoveries collected with great care and erudition.

On the 26th of March I obtained permiſſion to found at the Inſtitute a prize, to be aſſigned to the perſon who, in the courſe of the year, ſhall make the moſt curious obſervation, or publiſh the moſt uſeful memoir, in regard to aſtronomy.

C. Chaptal, whom we have the good fortune to have for miniſter of the interior, and whoſe labours as a philoſopher announce his zeal as a miniſter, has attached calculators to the Board of Longitude; he has granted gratifications to C. Flaguergues, an aſſiduous obſerver, and to C. Pons, keeper of the obſervatory of Marſeilles, who diſcovered a ſecond comet.

When the conſultation eſtabliſhed for diſtributing rewards, and which was very uſeful in 1793 and 1794, was ſuppreſſed, the Inſtitute was charged to take up the ſame buſineſs, and to propoſe to government the gratifications it would be neceſſary to give. The Inſtitution has not yet exerciſed this right, but I have propoſed to it to fulfil its obligation.

M. Jacotot, profeſſor of aſtronomy at Dijon, has obtained from C. Giraudet, prefect of La Côte-d'or, the repairs neceſſary for the obſervatory damaged by a fire. I hoped I ſhould have been able to obſerve there the eclipſe of the ſun on the 28th of Auguſt: but the ſky was abſolutely cloudy, as it was at Paris. We have received obſervations of that eclipſe from C. Thulis at Marſeilles, and from C. Flaguergues at Viviers.

Prince Henry of Wirtemberg, brother of the empreſs dowager of Ruſſia, who reſides at Hamburgh, has purchaſed the beautiful inſtruments made by Megnié, of Paris.

The ſpeculum for a teleſcope, which Dr. Herſchel ſent to Peterſburgh, has been ſent back to him for the purpoſe of being mounted. Kramp ſolicits for the eſtabliſhment of an obſervatory at Cologne.

The academy of Turin requeſts the re-eſtabliſhment of its obſervatory; and M. Henry has offered to take the direction of it.

Fathers Canovai and Delricco, who have at Florence the obſervatory of father Ximenes, have ſent us a liſt of their inſtruments, to prove that they cannot exert themſelves with any advantage for aſtronomy; but they have publiſhed tables of logarithms.

The king of Etruria has announced that he ſtill entertains a deſign of placing an aſtronomer in his *Cabinet de Phyſique*, where he has already ſome excellent inſtruments.

General Chabert, who has returned from England after an abſence of ten years, has been aſſociated with the Board of Longitude, to which office he was juſtly entitled by his geographical labours; and his age of ſixty-eight years does not prevent him from being preſent at the ſittings, and from being uſeful by his advice and experience.

The youngeſt ſon of our celebrated aſtronomer C. Mechain has been appointed by the Board of Longitude ſecretary of the obſervatory, which affords him an opportunity of devoting himſelf to aſtronomy. Auguſtine Francis Mechain was born at Paris, March 5, 1786.

He has ſucceeded his brother Iſaac, who having returned from Egypt has been appointed commiſſioner of foreign relations at Galaſia, in Moldavia.

C. Lechevalier has conſtructed in the Hotel of Foreign Relations a ſmall obſervatory, where he can gratify the taſte and curioſity of which he gave proofs during his ſtay at Conſtantinople.

C. Louis Berthoud has conſtructed for the obſervatory a clock worth a thouſand crowns. The pivots turn in rubies, and we hope it will equal that given by the Firſt Conſul to the obſervatory of Milan.

C. Pons, a watchmaker of great ſkill and ingenuity, made an experiment at my obſervatory of a clock which beats half ſeconds with a free ſcapement, and which goes with wonderful regularity.

C. Levêque has publiſhed, in the fourth volume of the Memoirs of the Inſtitute, a learned paper on the longitude, and particularly on Maignon's charts for reducing the moon's diſtances obſerved at ſea.

C. Richer has made a new trigonometric compaſs, or compaſs for reducing the moon's diſtances from the ſtars. It has ingenious inventions for dividing into unequal parts the rules containing the diſtance, the ſum, and the difference of the heights. In my ſhort Treatiſe of Navigation I have given a deſcription of the inſtrument which gained the prize in 1791; and in the *Connoiſſance des Temps* for the year 4, the demonſtration of the formula of M. de la Grange, which ſerves as a foundation for this inſtrument; which, however, has been much improved: the only inconvenience is, that it will coſt 600 francs.

Mr. Troughton has finiſhed the model of Menoza's circle, which gives the double of the multiple of Borda's circle, even retaining the ſmalleſt fixed mirror.

A curious detail reſpecting the calendar of the Indians has been publiſhed in the letters of the abbé Sevin.

C. Girard read in the Inſtitute a long memoir on the Egyptian milometer, the value of which he determined in the iſland Elephantine, on the very ſpot, 19 feet 5·6 lines; which ſhows that the antient meaſurement of the earth by Eratoſthones was very correct.

The abbé Teſta has publiſhed at Rome a diſſertation on the zodiac found at Dendara (in Greek, Tenthyris) in Egypt. He undertakes to prove that it is not older than 300 years before the vulgar æra. The Hiſtory of Herodotus, tranſlated by Larcher, edition of 1802, contains a ſally againſt the unbelievers, who carry back the period of one of the zodiacs of Tentyra, at preſent Dendera, and of Eſné, or Henne, to 6000 years; and the author's only reaſon is, that it would be 217 years before the creation.

He adds a notice by Viſconti, who ſays that the firſt ſign of the great zodiac is Leo; that Libra, the ſymbol of the equinox, is in its place; and that the reſemblance of the greater parts of the ſigns to the Grecian proves that this zodiac was conſtructed at a time not ſo remote as the earlieſt periods of the Greek aſtronomy: he is almoſt convinced that this work belongs to the firſt century of the vulgar æra.

The exterior cornice exhibits a large Greek inſcription, which may decide the queſtion; but another Greek inſcription contains Roman names, and announces a Cæſar, who could be only Auguſtus or Tiberius. In a word, M. Viſconti ſays that the architecture of the temple of Tentyra, though in the Egyptian taſte, and though ſome hieroglyphics are carved out on the walls, has an evident relation to the arts of Greece. For my part, I have remarked by the engraving of M. Denon that Cancer is inſcribed in two lines, at the beginning of the deſcending ſigns and at the end of the aſcending; which proves that the ſolſtice was towards the middle of Cancer; and this carries it back 3000 years. But I have ſhown in my Aſtronomy that Eudoxus, who wrote 370 years before our æra; and Aratus, who followed Eudoxus, deſcribed the ſphere according to an older tradition, which goes back twelve or thirteen hundred years before the vulgar æra, and which came from Egypt or India. Petau, Whiſton, Freret, and Legentil, found nearly the ſame date: it is therefore natural to believe that it ſhould be found in the zodiac of Tenthyris, which conſequently, in this reſpect, may be conſidered as a work of the Greeks.

C. Villoiſon, well known by his Greek erudition, has celebrated aſtronomy in Latin verſes, which announce both his talent for Latin poetry and his taſte for aſtronomy. He addreſſed them to me on my birth-day, and publiſhed them in the *Magazin Encyclopédique.* In the notes he ſpeaks of the labours of the uncle, the nephew, and niece; and of citizen Burckhardt, their moſt learned co-operator and their moſt intimate friend.

C. Boulage, of Troye, has written a beautiful epiſtle to Piazzi on the diſcovery of his planet: it has been inſerted in the fourth number of the Memoirs of the Academical Society of the department of Aube.

Aſtronomy this year has loſt the reſpectable Auguſtin Darquier, member of the National Inſtitute. He was born at Toulouſe, November 23, 1718, and died on the 10th of January 1802. He devoted himſelf early to aſtronomy through natural inclination. Though in a town diſtant from the capital, he applied to it with a zeal and activity which were not leſſened by age. He purchaſed inſtruments, eſtabliſhed an obſervatory in his own houſe, and cauſed two volumes of obſervations to be printed at his own expenſe. His tranſlation of Lambert's Coſmologic Letters has been printed at Amſterdam. He inſtructed pupils, paid calculators, and being in no need of the aſſiſtance of government he was indebted for every thing to himſelf. I have printed his laſt obſervations in my *Hiſtoire Céleſte*; they go as far as the 19th of March 1798; and he had ſome more to ſend me, though 80 years of age. The Lyceum of Toulouſe will give us ſome further particulars reſpecting his long and intereſting career.

Aſtronomy loſt alſo in France, on the 1ſt of March, M. Lemery, whoſe ſingular turn for calculation I diſcovered thirty years ago. Being then attached to the marquis De Puſieux, he employed all his leiſure time in calculation. I made him calculate a great many places of the moon, which were publiſhed in 1777 in the *Connoiſſance des Temps* for 1779: and for fifteen years he made all the calculations of that work with equal care and aſſiduity.

Daniel Bogdanich died at Peſt, in Hungary, on the 31ſt of January, at the age of 37. He was occupied with the geography of Hungary. (See Von Zach's Journal for April 1802.)

M. George Frederic Kordenbuſch, aſtronomer of Nuremberg, died on the 3d of April, at the age of 71.

In the month of September baron De Vega was drowned in the Danube, and it is ſuppoſed threw himſelf into that river. We are indebted to him for the large edition of Vlacq's tables of logarithms in folio, containing the numbers to 100,000; and the ſines from ten to ten ſeconds, which had become very rare, and in which he corrected many faults.

Meteorology has this year exhibited very extraordinary phænomena. In the month of January an extraordinary inundation: the Seine roſe 22 feet and a half above the height of 1719.

On the 25th of May a froſt, which did immenſe hurt to the productions of the earth.

On the 14th of May ſnow fell at Munich, and continued for forty-five hours; the trees were ſtripped of their leaves, and overturned.

In the months of July and Auguſt an exceſſive heat took place, and continued forty days. The thermometer at the obſervatory ſtood at 97° F., which is very uncommon at Paris.

The need of having meteorological rules founded on obſervation, induced our learned miniſter to eſtabliſh a correſpondence on that ſubject, and an office to conduct it.

Meteorological obſervations have been made in thirty places, from the convent of Mount Cenis as far as the borders of the ſea.

John Dominic Beraud, born in 1741, formerly a draftſman at Coni, and who for twenty years has reſided at Turin, continues to employ himſelf in making meteorological obſervations, and ſends us regularly his reſults.

Lafon, geographical engineer in Louiſiana, has ſent us meteorological obſervations made at New Orleans; and we have received ſome made at Guadaloupe by C. Hapel Lachenaye between the years 5 and 9. The fourth volume of the Memoirs of the Inſtitute contains ſome alſo.

C. Coulomb read in the Inſtitute experiments on the method of magnetizing needles to ſaturation; with a comparative view of the methods of Knight, Duhamel, and Æpinus. He has pointed out to navigators the ſureſt means of obtaining the beſt needles. The broadeſt and largeſt are preferable, but they muſt not be thick. This philoſopher has publiſhed in the fourth volume of the Inſtitute a curious memoir on the dipping needle.

The declination of the magnetic needle at Paris was obſerved by C. Bouvard at the obſervatory, on the 2d of May, to be 22° 3′, and on the 22d of July 21° 45′.

C. Lenoir, at the Garden of the *Depôt de la Marine*, found it on the 20th of June to be 22° 6′ with a compaſs, on which he employed every care poſſible. But the changes which take place at the different hours of the day, and in different months of the year, amount to more than 10′. We can therefore only ſay that the declination is 22°, and it was found to be the ſame in 1792 and 1800; ſo that at preſent it appears to be ſtationary, whereas ten years ago it increaſed from 6′ to 8′ annually.

Forty years ago I obſerved it to be 18¼°. (See *Connoiſſance des Temps* for 1762.)

M. Simonin, profeſſor of Croiſic, has ſent us the reſult of a thouſand obſervations on the tides, with the tables neceſſary for keeping an account of the variations produced by the ſun and moon according to their altitude and diſtance.

M. Romme, profeſſor of Rochfort, has ſent to the Inſtitute obſervations made every three minutes from low water to full moon to make known the irregularities, which are very ſingular; and a new table of the retardation of the tides, deduced from an immenſe number of obſervations.

He has preſented alſo a general table of the tides, the currents, and winds, obſerved in all the ſeas of the earth; the publication of which will form an important ſupplement to the large Treatiſe on the Flux and Reflux of the Sea, which I publiſhed in 1781.

Mr. Grenier, an officer known by his diſcoveries in the Indian ſeas, has written a conſiderable work on the winds and currents in all the ſeas; with a theory which renders the explanation of them eaſier. He propoſes to lay it ſoon before the public.

XXXVIII. On

XXXVIII. *On the Diſappearance of Saturn's Ring in the Year* 1803. *By Profeſſor* J. E. BODE*.

THE plane of Saturn's ring, as is well known, during the whole of his revolution of thirty years round the ſun, retains its paralleliſm, and interſects the plane of the ecliptic in 17 degrees of Piſces and of Virgo at an angle of 31½°. The neceſſary conſequence, therefore, is, that the northern ſide of the ring is illuminated for fifteen years by the oblique rays of the ſun, and then the ſouthern for the ſame period. It muſt alſo happen, that in the above places the plane of the ring paſſes twice through the ſun in each revolution, at which time the edge of the ring only is illuminated. But the edge or thickneſs of the ring being too ſmall to admit of its being ſeen from the earth on account of the great diſtance of Saturn, the ring becomes inviſible. For fifteen years the earth and the ſun are on the ſame ſide of the ring, which is the illuminated ſide, and therefore the ring during that period is always viſible; but a little before, and at that period when the plane of the ring paſſes through the ſun, or when Saturn's heliocentric place is in 17° of ♓ or of ♍ in the aſcending or deſcending nodes of the ring, the earth and the ſun may firſt be on one ſide and then on the other ſide of the ring. In the firſt caſe, as long as its plane does not paſs through the ſun it will be viſible from the earth; but in the other caſe it will be inviſible. The earth, therefore, can then paſs ſeveral times through the plane of the ring, at which time the ring will appear like a right line, or be entirely inviſible; the conſequence of which is, that the ring will be alternately viſible and inviſible.

This alternation of diſappearing and reappearing will be exhibited by the ring in the year 1803. The ſun, having illuminated the ſouth ſide of the ring ſince October 1789, will paſs through the plane of it in the month of June 1803, and will illuminate the northern ſide, when Saturn paſſes heliocentrically the aſcending node of his ring; and in the mean time the plane of the ring will paſs three times through the earth, which will be firſt on the ſide illuminated by the ſun, and then on the oppoſite or darkened ſide; in conſequence of which the ring ſeen from the earth will be twice viſible and inviſible in the courſe of a few months.

Theſe phænomena will be better comprehended by inſpecting the annexed figure, which ſuppoſes the obſerver to be placed in Saturn. This figure, then, exhibits the exact inclination of the plane of the ring to the ecliptic; the apparent motion of the earth and the ſun, as ſeen from Saturn, together with the time of the paſſage of theſe planets through the aſcending node of his ring. (See Plate V. fig. 2.)

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* From his *Aſtronomiſch Jahr Buch* for 1803.

AB is the ecliptic ſeen from Saturn in the ſign of Piſces, and divided into degrees. The aſcending node of Saturn's ring ſeen from thence is placed in 17° 6′, in which point the plane of the ring interſects the ecliptic at an angle of 31° 20′. The eye is then in that plane. Thus the ring appears as a ſtraight line; *nn* is the north ſide, and *zz* the ſouth ſide of it. The dotted line CD is the ſolar orbit ſeen from Saturn; and the ſun's place is marked in it on the firſt day of each month, from November 1802 to October 1803. Theſe places of the ſun are directly oppoſite to the heliocentric places of Saturn: the longitude thus differs ſix ſigns, and the northern latitude of Saturn ſeen from thence changes into the ſouthern of the ſun. In this manner, the ſun in November, December, January, and to the month of June, illuminates the ſouth ſide of the ring; but always in a fainter manner, the nearer he comes to the plane of it. On the 15th of June he paſſes through the plane in *e*. The ring at that time is illuminated on the edge, and can be ſeen only by powerful teleſcopes as a fine luminous line.

The longitude of the point *e*, reduced to the ecliptic, falls in *f*, or 20° 42′ of Piſces. The ſun, then, about the middle of June begins to illuminate the northern ſide of the ring; in July, Auguſt, September, October, &c. he recedes more and more from the plane of the ring, and the illumination of it then becomes ſtronger.

The elliptical line is the earth's orbit as ſeen from Saturn, according to its direct and retrograde motion from the 1ſt of November 1802 to the 1ſt of October 1803, and its place is marked on the firſt of each month. Its longitude is ſix ſigns leſs or more than the geocentric longitude of Saturn; and the northern latitude of Saturn is changed into the ſouthern of the earth.

Now, as the figure clearly ſhows, the earth in November and December 1802 is repreſented on the ſouth ſide, or turned towards the ſun, and therefore on the illuminated ſide of the ring. But afterwards the earth approaches more and more to the plane of the ring; and, as the ring receives the ſolar rays in a more and more oblique manner, the ring not only becomes narrower but even decreaſes in light. On the 13th of December, as may be ſeen by this ſmall figure, the earth enters the plane of the ring, and paſſes to the northern ſide of it, which is turned from the ſun. The ring then, for the firſt time, becomes to us inviſible: but this inviſibility is not of long continuance; for, as may be ſeen by the figure, the earth on the 1ſt of January, by its Saturniocentric apparent retrograde motion, paſſes a ſecond time through the plane of the ring, and goes to the ſouthern or illuminated ſide, and the ring then begins to appear again as a ſtraight line. In February, March, and April, the earth recedes more and more from the plane, and the ring becomes broader and more perceptible. But the ſun during theſe months approaches nearer to the plane of the ring, and by theſe means it is illuminated in a more oblique direction. In the month of May the earth is at its greateſt diſtance from the plane of the ring, while the ſun approaches it more and more till the middle of June, when he paſſes through the plane in *e*, and the ring for the ſecond time becomes totally inviſible to the earth, which is then on its dark ſide. From this period, during the month of July and till Auguſt, the ring will remain in this ſtate. About the 18th of Auguſt the earth will paſs, for the third time, through the plane of the ring, as it will then be on the northern, which is now the illuminated ſide of the ring: it will again appear as a ſtraight line, and be viſible when viewed through good teleſcopes: it will increaſe in light and breadth in September and October in proportion as the earth and the ſun recede from the plane.

Between the 1ſt of November 1802 and the 1ſt of October 1803 the ring of Saturn twice diſappears and twice reappears. At the firſt diſappearance on the 13th of December 1802, Saturn in the morning will be in the eaſtern part of the heavens; and the firſt reappearance will be about the 7th of January 1803; Saturn in the night-time being on the ſame ſide of the heavens.

At the ſecond diſappearance in the middle of June, Saturn in the night will be in the weſt, and, as he ſets half an hour after midnight, the phænomenon may be obſerved. But at the ſecond and laſt diſappearance in Auguſt, Saturn will be after the ſun, and therefore the phænomenon cannot be ſeen. In the month of October, when Saturn emerges from the ſun's rays, he may be ſeen before ſun-riſe in the eaſt, and the ring then will be very viſible.

In regard to the above calculation of the times of the diſappearance and reappearance of Saturn's ring, the accuracy of them, and the correſpondence of them with the phænomena, muſt depend on the accuracy of the tables and the theory hitherto adopted in regard to the poſition of the ring.

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The ſtate of the atmoſphere alſo, and the different power of the teleſcopes and of the eyes of the obſervers, may occaſion ſome variation. The diſappearance and reappearance of the ring, which are occaſioned by the earth paſſing through its plane, can be better and more conveniently obſerved than thoſe which take place in conſequence of the paſſage of the ſun through that plane, becauſe the earth, on account of its quicker motion, paſſes ſooner through the plane of the ring than the ſun.

As the ſix firſt ſatellites of Jupiter move in the ſame plane with the ring, when the ring diſappears, or becomes like a luminous line, theſe ſatellites muſt appear on both ſides of Saturn in a right line, and can then be more readily diſcovered and diſtinguiſhed from fixed ſtars.

XXXIX. *On the Diſappearance of the Ring of Saturn. By* VAN BEECK CALKOEN, *Profeſſor of Aſtronomy at Leyden**.

THE plane of Saturn's ring, according to aſtronomical obſervations, has always a direction parallel to itſelf, ſo that it interſects the ecliptic in 17° 18′ of ♍ and ♓, at an angle of 31½°, and the orbit of Saturn at 20° 42′ of ♍ and ♓. Now, if Saturn as ſeen from the ſun be in longitude 20° 42′ of ♓, the plane of the ring will paſs through the centre of the ſun; by which the edge of the ring being illuminated, while the northern and ſouthern ſurface receive no light, it muſt become inviſible, and can be ſeen only by the moſt powerful teleſcopes as a fine luminous line. On the 15th of June next Saturn will be in this ſituation; and as this planet employs about thirty years in its revolution round the ſun, this phænomenon takes place only once in 15 years. Other phænomena, however, in conſequence of which the ring before the 15th of June will alternately diſappear and reappear, are connected with this ſtate of the planet. Theſe phænomena ariſe from the different poſitions of the earth, which in the courſe of its revolution is at one time above and at another below the plane of the ring; ſo that from the 1ſt of November 1802 to October 1803 the ring will be twice inviſible and twice viſible; firſt the ſouthern and then the northern, then the ſouthern and afterwards the northern ſurface will be alternately ſeen from the earth within the courſe of ten months. For determining and calculating theſe phænomena with more accuracy, Lambert invented a very ingenious kind of projection, a repreſentation of which was given by profeſſor Bode, of Berlin, in his Aſtronomical Almanac for 1789, in which year theſe phænomena took place in the ſame manner as they will do in 1803; and this repreſentation has been repeated in the ſame work for the preſent year: but as this diagram, in my opinion, is too difficult for thoſe not well verſed in aſtronomy, I flatter myſelf that the following figure will render the phænomenon eaſier to be underſtood by amateurs; though Lambert's method is more exact, and better ſuited to aſtronomers.

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* From *Algemeene Konſt en Letter-Bode*, No. 13, for 1803.

The circle (Plate IV. fig. 1.) repreſents the earth's orbit, and exhibits the points where the earth is placed at the commencement of each month. The vernal equinox is marked o ♈, the line SZ drawn through the ſun; and the place of Saturn on the 15th of June is the direction of the plane of the ring. Saturn, at the four periods announced, is in the lines drawn parallel to SZ, though at a diſtance from the ſun five times greater than is here repreſented for want of room. The plane of the paper is that of the ecliptic, with which the one-half of Saturn's ring makes an angle of 31°.

I have already obſerved that the ring twice diſappears, and then twice becomes again viſible. This phænomenon ariſes partly from the ſituation of Saturn in regard to the ſun, and partly from that of the earth; and each of theſe cauſes muſt be conſidered ſeparately to have a clear idea of theſe phænomena. In regard to the firſt, it is evident that that ſide only of Saturn's ring which is turned towards the ſun can be illuminated; and this illumination is ſtrongeſt when the planet is in 20° of ♊ and 20° of ♐, the whole ſurface of the ring being then oppoſite to the ſun. Now as the direction of the ring is always the ſame, that is to ſay, parallel to itſelf, the rays of the ſun, as the planet approaches to 20° of ♍ and ♓, muſt fall more obliquely on the ring, by which means it will be more faintly lighted, and at the ſame time appear ſmaller; and when Saturn has attained to this longitude, no part of the upper ſurface, but merely the edge of the ring, the plane of which paſſes through the ſun, will be illuminated; ſo that the ring will then have the appearance of a thin luminous line, but will not be viſible without the help of good glaſſes. For the laſt ſeven years the ring has been obſerved to decreaſe in light and in breadth; and this decreaſe will continue till the 15th of June, after which both will again increaſe. During the laſt fifteen years the ſouthern ſurface of the ring has been illuminated, but in the next fifteen the northern ſurface alone will receive the rays of the ſun. In whatever part of our ſolar ſyſtem this phænomenon be viewed, the appearance will be the ſame. The ring till the 15th of June will continually become ſmaller, and on that day will be converted into an almoſt imperceptible line of light. The diſappearance then of Saturn's ring on the 15th of June, or rather its decreaſe till that period and its ſubſequent increaſe, ariſes alone from the relative poſition of Saturn and the ſun. As to the other cauſes of diſappearance, which are the conſequence of Saturn's poſition in regard to the earth, we muſt firſt keep in view the ſlow motion of Saturn compared to that of the earth, as this planet advances ſcarcely 12° in the time that the earth performs its revolution around the ſun. On the 15th of June, at which time Saturn will be in 20° of ♓, and the plane of the ring will paſs through the ſun, the interſection of that plane with the ecliptic, or the line ZS in the figure, paſſes through the middle of the earth's orbit. During the laſt fifteen years, while Saturn advanced from ♈ to ♍, this line of the plane of the ring was without, and above the earth's orbit, and during that period the ſouthern and luminous part of the ring was always viſible; but about the middle of December 1802 this line approached the earth's orbit, and on the 13th of December paſſed through it exactly in the point where the earth then was. The earth being then in the plane of the ring, the thickneſs of the ring only could be viſible to us; and thus, for the firſt time in fifteen years, diſappeared on the 13th of December 1802. On the other hand, as the earth moved ſtraight towards Saturn, or parallel with that line, while Saturn advanced, it was neceſſary that the ſouthern ſurface of the ring hitherto viſible ſhould become inviſible, and the northern, which was not illuminated, being turned towards the earth, the whole ring would become inviſible. But as the earth proceeded in its orbit it would ſoon reach the line of the plane, and on the 7th of January 1803 would again paſs through it. At that period, the ſouthern ſurface of the ring being illuminated by the ſun would again be viſible from the earth; but as the earth on the 21ſt of June muſt in its revolution be again on the other ſide, Saturn in his courſe falling in with the earth in the plane of the interſection of the ring and the ecliptic, will again paſs through it on the 18th of Auguſt, at which time the ring will appear once more as a fine ſtripe of light. After that day the northern ſurface of the ring will be viſible from the earth, and, as the line of the plane will then be without the earth's orbit, will, during the next fifteen years, remain always viſible.

From the 7th January to the 21ſt of June, the ſouthern ſurface of the ring being illuminated, the ring will be viſible;

but from the 21ſt of June to the 18th of Auguſt no part of it will be ſeen, as its darkened ſide during that period will be turned towards the earth. This diſappearance and reappearance of Saturn's ring will be of importance to aſtronomers, as it will enable them to determine the direction of the plane of the ring, and will afford a curious ſpectacle to amateurs who are provided with good teleſcopes. The ring, which already appears as a broad ſtripe of light, will continually become ſmaller till the 15th of June; after which time it will diſappear till the 15th of Auguſt, when it will appear as a fine ſtripe of light, which will increaſe in breadth till the aperture between it and the planet will increaſe during the firſt ſeven years. As Saturn till the month of June ſets at midnight, the decreaſe of the ring may always be obſerved; but when he ſets earlier it will be poſſible to ſee him only a ſhort time in Auguſt, as he will be loſt in the ſun's rays.

Leyden,
March 10, 1803.

XL. *Memoir on the Foſſil Caout-Chouc, or Elaſtic Bitumen of Derbyſhire.* *By* FAUJAS-SAINT-FOND *.

ABOUT twenty years ago ſome inſulated portions of a kind of blackiſh bitumen, compreſſible and even elaſtic, which had ſome ſort of reſemblance to old leather, were found in the natural fiſſures of one of the mines of Caſtleton. The young miners having ſet fire to ſome of this matter, it burnt with a bright flame, emitting an odour which did not appear to be diſagreeable.

At that period mineralogy was not ſo well known as at preſent in England, and particularly in Derbyſhire: no one paid attention to this foſſil, or endeavoured to aſcertain its nature.

Twelve years after, a heavy ſtorm of rain having produced a deep ravine on the ſide of one of the hills which ſurround the village of Caſtleton, a ſimilar kind of bitumen was found between the foſſil ſtrata of argillaceous ſchiſt which exiſts at the bottom of that hill.

More care was then taken to collect this matter, ſome of which was ſent to Derby, Edinburgh, and London; and ſo great a relation was found between it and the *caout-chouc*, known under the vulgar name of *elaſtic gum*, that mineralogiſts did not heſitate to conſider it as the ſame as that brought from Guiana and Peru. This opinion was ſoon confirmed by chemical analyſis; and this ſingular foſſil was the more ſought after, and conſidered as a curious object of natural hiſtory, as it was more rare; and as it was thought an aſtoniſhing circumſtance, that a ſubſtance which diſtils from exotic trees which grow only in the torrid zone ſhould be found between ſtrata of argillaceous ſchiſt in the boſom of the mountains in the northern part of England.

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* From *Journal du Muſeum National*, No. 4.

Lametherie mentioned this diſcovery in the *Journal de Phyſique*, without giving any details reſpecting the place where it was found: he only inſiſted on the analogy between this foſſil bitumen and the caout-chouc.

At that period ſome varied fragments of this ſubſtance were ſent to me, together with the matrices in which they had been found; and as I was acquainted with the places and the mountain where the diſcovery had been made, I mentioned this important fact every year in my geological lectures at the Muſæum, when I treated of the exotic wood and plants found in a foſſil ſtate in countries abſolutely oppoſite to thoſe where theſe plants and trees grow at preſent: but I publiſhed nothing on the ſubject, as has been obſerved by Mr. Mawe, proprietor of the principal mines of ſparry fluor of the neighbourhood of Caſtleton, and author of *The Mineralogy of Derbyſhire*, lately publiſhed; a work which will be exceedingly uſeful to naturaliſts who intend to viſit that county, ſo abundant in a variety of mineral productions *.

Mr. Mawe, when laſt at Paris, brought me ſome very remarkable ſpecimens which were wanting in my collection of foſſil caout-chouc; and it is my duty to acknowledge here my gratitude to him. He was ſo kind as to give me alſo ſome information on this ſubject; and he ſhowed me at the ſame time a piece of foſſil caout-chouc, diſcovered not long ago, conſiſting of an oblong maſs two feet and a half in circumference and two inches and a half in thickneſs, weighing about three pounds. It is ſoft, exceedingly compreſſible, and of a black and ſomewhat olive colour; ſolid, hard, and ſhining fragments of a bitumen nearly ſimilar are enveloped by, and adhere as it were to, the elaſtic maſs, both in the inſide and on the exterior faces. They are not larger than a common almond; ſome of them are *black like jet*, others of a *brown liver* colour. They may be eaſily detached from the compreſſible caout-chouc where theſe hard fragments are not abundant.

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* The Mineralogy of Derbyſhire, with a Deſcription of the moſt intereſting Mines in the North of England, in Scotland and Wales. By John Mawe. London 1802. 8vo. one volume, with plates.

Mr. Mawe has made known in his Mineralogy of Derbyſhire the principal varieties of the foſſil caout-chouc; but as he has attended rather to thoſe ſelect ſpecimens fitteſt for ornamenting collections, than to a ſyſtematic deſcription of theſe pieces as well as of the matrices which accompany them, and as he thought it of no uſe in his plan to enter into details reſpecting the nature and depth of the places where the foſſil caout-chouc is found, I ſhall ſupply the deficiency in this part, which is ſo intimately connected with geology.

In order to give a topographical idea of the place, I ſhall obſerve, that in going from Derby to Caſtleton you are obliged to aſcend a pretty rapid acclivity to a large plain in the form of a mountain, which extends for ſeveral leagues in every direction. All this elevated, rugged, and rocky diſtrict is known by the name of High Peak. It is in general calcareous, and even abundant in ſhells, except ſome argillaceous ſtrata, and particularly ſeveral veins of trapp which interſect tranſverſely the calcareous ſtrata, or which, diſpoſed ſometimes in banks, proceed nearly parallel with beds of limeſtone. I ſhall ſay nothing of the veins of lead ore, of the beautiful cryſtals of ſparry fluor, of the calamine and other minerals found in this diſtrict. For an account of theſe I ſhall refer to the Deſcription of Derbyſhire, Whitehurſt on the Formation of the Earth *, and Ferber's Oryctographia of Derbyſhire †, or that which I gave myſelf of the environs of Buxton and of Caſtleton in my Tour through England and Derbyſhire ‡.

Caſtleton is not ſeen till you are, as I may ſay, above it, and till you arrive at the edge of a rapid and ſteep declivity, which interſects, in an abrupt and ſudden manner, this part of the mountain ſurrounding the baſon, at the bottom of which the ſmall village in queſtion is ſituated as in the middle of a funnel.

The revolution which divided this mountain, by tearing aſunder its ſides, aroſe, no doubt, from one of thoſe grand natural events ſimilar traces of which are to be found even in the High Alps; but here this terrible commotion, by opening the boſom of the earth, has uncovered ſeveral mineral riches which without this accidental circumſtance would never have been known.

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* Inquiry into the Original State and Formation of the Earth, &c. by John Whitehurſt. London, 4to. with plates.

† Eſſay on the Oryctography of Derbyſhire, by Mr. Ferber, tranſlated from the German.

‡ Voyage en Angleterre, en Ecoſſe et aux Iles Hébrides, &c. 2 vols. 8vo. avec fig.

One of the precipices of this mountain is known by the name of Hay Cliff: it is calcareous, and contains ſhells and other marine productions in a ſtate of petrification. The other is called Man-Tor: it is alſo calcareous, and towards the foot there are ſome galleries of a mine, on a vein which terminates near the ſurface, and of which the matrix is a milky calcareous ſpar containing large-grained galena, but in ſmall quantity. There are found alſo towards the bottom of the ſame precipice, as well as in ſome other parts, fiſſile ſtrata of argillaceous ſchiſt, very often marly, which imbibe water in the time of rain, ſplit, and occaſion the ſolid and ſtony maſſes which reſt above them to crumble down*.

Some of the ſchiſtous ſtrata in queſtion have a certain hardneſs; but all in general have a character of alluvion which an experienced eye cannot miſtake, and yet their formation is of the higheſt antiquity. It is at a depth which, without fear of being deceived, may be eſtimated at leaſt at four hundred and fifty feet below the upper ſtratum, that the foſſil caout-chouc has been found, and particularly the large fragment in the poſſeſſion of Mr. Mawe. In general it is in ſmall cavities, in a kind of neſts between the fiſſile ſtrata, that this bituminous matter is contained; and as in this ſtate it is ſheltered from the action of the air, it needs excite no ſurpriſe that it ſhould have experienced only very little alteration, and that its nature is not changed.

In the laſt place, one of the cavities, twenty-two inches in length and five inches in height, contained a great deal of caout-chouc. By theſe means very fine ſpecimens of it may be obtained for collections, at a very moderate price; and it is to Mr. Mawe that we are indebted for this obligation. This gentleman, who with his father-in-law Mr. Brawn poſſeſſes a large manufactory of ſparry fluor, of which they make elegant vaſes and other ornaments, carries on trade in a very honourable manner with the minerals of Scotland, Derbyſhire and other parts of England, a depôt of which he propoſes to eſtabliſh at Paris, which muſt, no doubt, be of advantage to the progreſs of mineralogy.

I divide the foſſil caout-chouc of Derbyſhire into elaſtic or compreſſible bitumen, and ſolid, hard, and brittle bitumen.

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* "The earth and ſtones which crumble down from this mountain," ſays Ferber, who viſited theſe places ſeveral years before I did, "form in ſeveral places ſmall hills, the ſize of which daily increaſes, and which the people conſider as one of the ſeven wonders of the Peak."—*Eſſai d'Oryctographie du Derbyſhire*, par Ferber, *Trad. Francoiſe*, p. 21.

SECTION I.

Compreſſible Bitumen.

Variety I. Elaſtic bitumen of a blackiſh brown and ſomewhat olive colour, ſoft, exceedingly compreſſible, unctuous and ſomewhat fat, of a ſlightly aromatic ſmell, but having a little of the inſipid odour of natural caout-chouc; inflammable, and burning with a bright brilliant flame, leaving a black oily reſiduum which does not become dry. The fragments of this variety are ſometimes an inch and a half in thickneſs and four inches in breadth. They are the largeſt except the fragment in poſſeſſion of Mr. Mawe, which is conſiderably larger.

Variety II. Foſſil caout-chouc, ſplit and cracked in the whole exterior ſurface, which was in contact with the matrix from which it has been detached. It is dry in that part, but nevertheleſs compreſſible; black on the ſurface expoſed to the contact of the air; but remarkable in this reſpect, that when a lamina of about a line in thickneſs is cut from this caout-chouc with a very ſharp knife, it appears in the inſide of a yellowiſh white colour: when cut in a very thick part, where the action of the air has not been able to render it black, like certain balls of the elaſtic gum brought ſometimes from Para, it has the ſame appearance as the uſual caout-chouc. Another circumſtance worthy of attention is, that in the part recently cut a kind of acid matter is obſerved to ooze out, which is not of a diſagreeable taſte, and which appears to be of the nature of the pyrolignic acid. This caout-chouc is pellucid on the edges, and almoſt of a hyacinth red colour.

Scarcely has the air exerciſed its action on the part recently cut, which is white, when it aſſumes a ſlight reddiſh tint, which daily becomes darker, and acquires a dark mahogany and then a blackiſh brown colour; at length, at the end of a month, if expoſed to the air, it becomes of a deep black.

Variety III. The ſame caout-chouc as the preceding, but of a ſomewhat firmer texture and a ligneous appearance. When viewed with a magnifying glaſs, it is obſerved that the fibrous undulations ſeen in this variety are merely the effect of the milky ſubſtance which flowed from the tree more or leſs ſlowly or at different times, and which did not acquire conſiſtence till it had loſt the mucilaginous water which held the matter of caout-chouc ſuſpended or in ſolution. I particularly mention this fact, to ſhow that this variety has no real relation to the wood of the tree which produced at that period the elaſtic bitumen.

placed in Saturn. This figure, then, exhibits the exact inclination of the plane of the ring to the ecliptic; the apparent motion of the earth and the ſun, as ſeen from Saturn, together with the time of the paſſage of theſe planets through the aſcending node of his ring. (See Plate V. fig. 2.)

AB is the ecliptic ſeen from Saturn in the ſign of Piſces, and divided into degrees. The aſcending node of Saturn's ring ſeen from thence is placed in 17° 6′, in which point the plane of the ring interſects the ecliptic at an angle of 31° 20′. The eye is then in that plane. Thus the ring appears as a ſtraight line; *nn* is the north ſide, and *zz* the ſouth ſide of it. The dotted line CD is the ſolar orbit ſeen from Saturn; and the ſun's place is marked in it on the firſt day of each month, from November 1802 to October 1803. Theſe places of the ſun are directly oppoſite to the heliocentric places of Saturn: the longitude thus differs ſix ſigns, and the northern latitude of Saturn ſeen from thence changes into the ſouthern of the ſun. In this manner, the ſun in November, December, January, and to the month of June, illuminates the ſouth ſide of the ring; but always in a fainter manner, the nearer he comes to the plane of it. On the 15th of June he paſſes through the plane in *e*. The ring at that time is illuminated on the edge, and can be ſeen only by powerful teleſcopes as a fine luminous line.

The longitude of the point *e*, reduced to the ecliptic, falls in *f*, or 20° 42′ of Piſces. The ſun, then, about the middle of June begins to illuminate the northern ſide of the ring; in July, Auguſt, September, October, &c. he recedes more and more from the plane of the ring, and the illumination of it then becomes ſtronger.

The elliptical line is the earth's orbit as ſeen from Saturn, according to its direct and retrograde motion from the 1ſt of November 1802 to the 1ſt of October 1803, and its place is marked on the firſt of each month. Its longitude is ſix ſigns leſs or more than the geocentric longitude of Saturn; and the northern latitude of Saturn is changed into the ſouthern of the earth.

Now, as the figure clearly ſhows, the earth in November and December 1802 is repreſented on the ſouth ſide, or turned towards the ſun, and therefore on the illuminated ſide of the ring. But afterwards the earth approaches more and more to the plane of the ring; and, as the ring receives the ſolar rays in a more and more oblique manner, the ring not only becomes narrower but even decreaſes in light. On the 13th of December, as may be ſeen by this ſmall figure, the earth enters the plane of the ring, and paſſes to the northern ſide of it, which is turned from the ſun. The ring then, for the firſt time, becomes to us inviſible: but this inviſibility is not of long continuance; for, as may be ſeen by the figure, the earth on the 1ſt of January, by its Saturniocentric apparent retrograde motion, paſſes a ſecond time through the plane of the ring, and goes to the ſouthern or illuminated ſide, and the ring then begins to appear again as a ſtraight line. In February, March, and April, the earth recedes more and more from the plane, and the ring becomes broader and more perceptible. But the ſun during theſe months approaches nearer to the plane of the ring, and by theſe means it is illuminated in a more oblique direction. In the month of May the earth is at its greateſt diſtance from the plane of the ring, while the ſun approaches it more and more till the middle of June, when he paſſes through the plane in *e*, and the ring for the ſecond time becomes totally inviſible to the earth, which is then on its dark ſide. From this period, during the month of July and till Auguſt, the ring will remain in this ſtate. About the 18th of Auguſt the earth will paſs, for the third time, through the plane of the ring, as it will then be on the northern, which is now the illuminated ſide of the ring: it will again appear as a ſtraight line, and be viſible when viewed through good teleſcopes: it will increaſe in light and breadth in September and October in proportion as the earth and the ſun recede from the plane.

Between the 1ſt of November 1802 and the 1ſt of October 1803 the ring of Saturn twice diſappears and twice reappears. At the firſt diſappearance on the 13th of December 1802, Saturn in the morning will be in the eaſtern part of the heavens; and the firſt reappearance will be about the 7th of January 1803; Saturn in the night-time being on the ſame ſide of the heavens.

At the ſecond diſappearance in the middle of June, Saturn in the night will be in the weſt, and, as he ſets half an hour after midnight, the phænomenon may be obſerved. But at the ſecond and laſt diſappearance in Auguſt, Saturn will be after the ſun, and therefore the phænomenon cannot be ſeen. In the month of October, when Saturn emerges from the ſun's rays, he may be ſeen before ſun-riſe in the eaſt, and the ring then will be very viſible.

In regard to the above calculation of the times of the diſappearance and reappearance of Saturn's ring, the accuracy of them, and the correſpondence of them with the phænomena, muſt depend on the accuracy of the tables and the theory hitherto adopted in regard to the poſition of the ring.

The ſtate of the atmoſphere alſo, and the different power of the teleſcopes and of the eyes of the obſervers, may occaſion ſome variation. The diſappearance and reappearance of the ring, which are occaſioned by the earth paſſing through its plane, can be better and more conveniently obſerved than thoſe which take place in conſequence of the paſſage of the ſun through that plane, becauſe the earth, on account of its quicker motion, paſſes ſooner through the plane of the ring than the ſun.

As the ſix firſt ſatellites of Jupiter move in the ſame plane with the ring, when the ring diſappears, or becomes like a luminous line, theſe ſatellites muſt appear on both ſides of Saturn in a right line, and can then be more readily diſcovered and diſtinguiſhed from fixed ſtars.

XXXIX. *On the Diſappearance of the Ring of Saturn. By* VAN BEECK CALKOEN, *Profeſſor of Aſtronomy at Leyden* *.

THE plane of Saturn's ring, according to aſtronomical obſervations, has always a direction parallel to itſelf, ſo that it interſects the ecliptic in 17° 18′ of ♍ and ♓, at an angle of $31\frac{1}{2}$°, and the orbit of Saturn at 20° 42′ of ♍ and ♓. Now, if Saturn as ſeen from the ſun be in longitude 20° 42′ of ♓, the plane of the ring will paſs through the centre of the ſun; by which the edge of the ring being illuminated, while the northern and ſouthern ſurface receive no light, it muſt become inviſible, and can be ſeen only by the moſt powerful teleſcopes as a fine luminous line. On the 15th of June next Saturn will be in this ſituation; and as this planet employs about thirty years in its revolution round the ſun, this phænomenon takes place only once in 15 years. Other phænomena, however, in conſequence of which the ring before the 15th of June will alternately diſappear and reappear, are connected with this ſtate of the planet. Theſe phænomena ariſe from the different poſitions of the earth, which in the courſe of its revolution is at one time above and at another below the plane of the ring; ſo that from the 1ſt of November 1802 to October 1803 the ring will be twice inviſible and twice viſible; firſt the ſouthern and then the northern, then the ſouthern and afterwards the northern ſurface will be alternately ſeen from the earth within the courſe of ten months. For determining and calculating theſe phænomena with more accuracy, Lambert invented a very ingenious kind of projection, a repreſentation of which was given by profeſſor Bode, of Berlin, in his Aſtronomical Almanac for 1789, in which year theſe phænomena took place in the ſame manner as they will do in 1803; and this repreſentation has been repeated in the ſame work for the preſent year: but as this diagram, in my opinion, is too difficult for thoſe not well verſed in aſtronomy, I flatter myſelf that the following figure will render the phænomenon eaſier to be underſtood by amateurs; though Lambert's method is more exact, and better ſuited to aſtronomers.

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* From *Algemeene Konſt en Letter-Bode*, No. 13, for 1803.

The circle (Plate IV. fig. 1.) repreſents the earth's orbit, and exhibits the points where the earth is placed at the commencement of each month. The vernal equinox is marked 0 ♈, the line SZ drawn through the ſun; and the place of Saturn on the 15th of June is the direction of the plane of the ring. Saturn, at the four periods announced, is in the lines drawn parallel to SZ, though at a diſtance from the ſun five times greater than is here repreſented for want of room. The plane of the paper is that of the ecliptic, with which the one-half of Saturn's ring makes an angle of 31°.

I have already obſerved that the ring twice diſappears, and then twice becomes again viſible. This phænomenon ariſes partly from the ſituation of Saturn in regard to the ſun, and partly from that of the earth; and each of theſe cauſes muſt be conſidered ſeparately to have a clear idea of theſe phænomena. In regard to the firſt, it is evident that that ſide only of Saturn's ring which is turned towards the ſun can be illuminated; and this illumination is ſtrongeſt when the planet is in 20° of ♊ and 20° of ♐, the whole ſurface of the ring being then oppoſite to the ſun. Now as the direction of the ring is always the ſame, that is to ſay, parallel to itſelf, the rays of the ſun, as the planet approaches to 20° of ♍ and ♓, muſt fall more obliquely on the ring, by which means it will be more faintly lighted, and at the ſame time appear ſmaller; and when Saturn has attained to this longitude, no part of the upper ſurface, but merely the edge of the ring, the plane of which paſſes through the ſun, will be illuminated; ſo that the ring will then have the appearance of a thin luminous line, but will not be viſible without the help of good glaſſes. For the laſt ſeven years the ring has been obſerved to decreaſe in light and in breadth; and this decreaſe will continue till the 15th of June, after which both will again increaſe. During the laſt fifteen years the ſouthern ſurface of the ring has been illuminated, but in the next fifteen the northern ſurface alone will receive the rays of the ſun. In whatever part of our ſolar ſyſtem this phænomenon

non be viewed, the appearance will be the ſame. The ring till the 15th of June will continually become ſmaller, and on that day will be converted into an almoſt imperceptible line of light. The diſappearance then of Saturn's ring on the 15th of June, or rather its decreaſe till that period and its ſubſequent increaſe, ariſes alone from the relative poſition of Saturn and the ſun. As to the other cauſes of diſappearance, which are the conſequence of Saturn's poſition in regard to the earth, we muſt firſt keep in view the ſlow motion of Saturn compared to that of the earth, as this planet advances ſcarcely 12° in the time that the earth performs its revolution around the ſun. On the 15th of June, at which time Saturn will be in 20° of ♓, and the plane of the ring will paſs through the ſun, the interſection of that plane with the ecliptic, or the line ZS in the figure, paſſes through the middle of the earth's orbit. During the laſt fifteen years, while Saturn advanced from ♈ to ♍, this line of the plane of the ring was without, and above the earth's orbit, and during that period the ſouthern and luminous part of the ring was always viſible; but about the middle of December 1802 this line approached the earth's orbit, and on the 13th of December paſſed through it exactly in the point where the earth then was. The earth being then in the plane of the ring, the thickneſs of the ring only could be viſible to us; and thus, for the firſt time in fifteen years, diſappeared on the 13th of December 1802. On the other hand, as the earth moved ſtraight towards Saturn, or parallel with that line, while Saturn advanced, it was neceſſary that the ſouthern ſurface of the ring hitherto viſible ſhould become inviſible, and the northern, which was not illuminated, being turned towards the earth, the whole ring would become inviſible. But as the earth proceeded in its orbit it would ſoon reach the line of the plane, and on the 7th of January 1803 would again paſs through it. At that period, the ſouthern ſurface of the ring being illuminated by the ſun would again be viſible from the earth; but as the earth on the 21ſt of June muſt in its revolution be again on the other ſide, Saturn in his courſe falling in with the earth in the plane of the interſection of the ring and the ecliptic, will again paſs through it on the 18th of Auguſt, at which time the ring will appear once more as a fine ſtripe of light. After that day the northern ſurface of the ring will be viſible from the earth, and, as the line of the plane will then be without the earth's orbit, will, during the next fifteen years, remain always viſible.

From the 7th January to the 21ſt of June, the ſouthern ſurface of the ring being illuminated, the ring will be viſible;

but from the 21ſt of June to the 18th of Auguſt no part of it will be ſeen, as its darkened ſide during that period will be turned towards the earth. This diſappearance and reappearance of Saturn's ring will be of importance to aſtronomers, as it will enable them to determine the direction of the plane of the ring, and will afford a curious ſpectacle to amateurs who are provided with good teleſcopes. The ring, which already appears as a broad ſtripe of light, will continually become ſmaller till the 15th of June; after which time it will diſappear till the 15th of Auguſt, when it will appear as a fine ſtripe of light, which will increaſe in breadth till the aperture between it and the planet will increaſe during the firſt ſeven years. As Saturn till the month of June ſets at midnight, the decreaſe of the ring may always be obſerved; but when he ſets earlier it will be poſſible to ſee him only a ſhort time in Auguſt, as he will be loſt in the ſun's rays.

Leyden,
March 10, 1803.

XL. *Memoir on the Foſſil Caout-Chouc, or Elaſtic Bitumen of Derbyſhire.* *By* FAUJAS-SAINT-FOND*.

ABOUT twenty years ago ſome inſulated portions of a kind of blackiſh bitumen, compreſſible and even elaſtic, which had ſome ſort of reſemblance to old leather, were found in the natural fiſſures of one of the mines of Caſtleton. The young miners having ſet fire to ſome of this matter, it burnt with a bright flame, emitting an odour which did not appear to be diſagreeable.

At that period mineralogy was not ſo well known as at preſent in England, and particularly in Derbyſhire: no one paid attention to this foſſil, or endeavoured to aſcertain its nature.

Twelve years after, a heavy ſtorm of rain having produced a deep ravine on the ſide of one of the hills which ſurround the village of Caſtleton, a ſimilar kind of bitumen was found between the foſſil ſtrata of argillaceous ſchiſt which exiſts at the bottom of that hill.

More care was then taken to collect this matter, ſome of which was ſent to Derby, Edinburgh, and London; and ſo great a relation was found between it and the *caout-chouc*, known under the vulgar name of *elaſtic gum*, that mineralogiſts did not heſitate to conſider it as the ſame as that brought from Guiana and Peru. This opinion was ſoon confirmed by chemical analyſis; and this ſingular foſſil was the more ſought after, and conſidered as a curious object of natural hiſtory, as it was more rare; and as it was thought an aſtoniſhing circumſtance, that a ſubſtance which diſtils from exotic trees which grow only in the torrid zone ſhould be found between ſtrata of argillaceous ſchiſt in the boſom of the mountains in the northern part of England.

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* From *Journal du Muſeum National*, No. 4.

Lametherie mentioned this diſcovery in the *Journal de Phyſique*, without giving any details reſpecting the place where it was found: he only inſiſted on the analogy between this foſſil bitumen and the caout-chouc.

At that period ſome varied fragments of this ſubſtance were ſent to me, together with the matrices in which they had been found; and as I was acquainted with the places and the mountain where the diſcovery had been made, I mentioned this important fact every year in my geological lectures at the Muſæum, when I treated of the exotic wood and plants found in a foſſil ſtate in countries abſolutely oppoſite to thoſe where theſe plants and trees grow at preſent: but I publiſhed nothing on the ſubject, as has been obſerved by Mr. Mawe, proprietor of the principal mines of ſparry fluor of the neighbourhood of Caſtleton, and author of *The Mineralogy of Derbyſhire*, lately publiſhed; a work which will be exceedingly uſeful to naturaliſts who intend to viſit that county, ſo abundant in a variety of mineral productions *.

Mr. Mawe, when laſt at Paris, brought me ſome very remarkable ſpecimens which were wanting in my collection of foſſil caout-chouc; and it is my duty to acknowledge here my gratitude to him. He was ſo kind as to give me alſo ſome information on this ſubject; and he ſhowed me at the ſame time a piece of foſſil caout-chouc, diſcovered not long ago, conſiſting of an oblong maſs two feet and a half in circumference and two inches and a half in thickneſs, weighing about three pounds. It is ſoft, exceedingly compreſſible, and of a black and ſomewhat olive colour; ſolid, hard, and ſhining fragments of a bitumen nearly ſimilar are enveloped by, and adhere as it were to, the elaſtic maſs, both in the inſide and on the exterior faces. They are not larger than a common almond; ſome of them are *black like jet*, others of a *brown liver* colour. They may be eaſily detached from the compreſſible caout-chouc where theſe hard fragments are not abundant.

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* The Mineralogy of Derbyſhire, with a Deſcription of the moſt intereſting Mines in the North of England, in Scotland and Wales. By John Mawe. London 1802. 8vo. one volume, with plates.

Mr. Mawe has made known in his Mineralogy of Derbyſhire the principal varieties of the foſſil caout-chouc; but as he has attended rather to thoſe ſelect ſpecimens fitteſt for ornamenting collections, than to a ſyſtematic deſcription of theſe pieces as well as of the matrices which accompany them, and as he thought it of no uſe in his plan to enter into details reſpecting the nature and depth of the places where the foſſil caout-chouc is found, I ſhall ſupply the deficiency in this part, which is ſo intimately connected with geology.

In order to give a topographical idea of the place, I ſhall obſerve, that in going from Derby to Caſtleton you are obliged to aſcend a pretty rapid acclivity to a large plain in the form of a mountain, which extends for ſeveral leagues in every direction. All this elevated, rugged, and rocky diſtrict is known by the name of High Peak. It is in general calcareous, and even abundant in ſhells, except ſome argillaceous ſtrata, and particularly ſeveral veins of trapp which interſect tranſverſely the calcareous ſtrata, or which, diſpoſed ſometimes in banks, proceed nearly parallel with beds of limeſtone. I ſhall ſay nothing of the veins of lead ore, of the beautiful cryſtals of ſparry fluor, of the calamine and other minerals found in this diſtrict. For an account of theſe I ſhall refer to the Deſcription of Derbyſhire, Whitehurſt on the Formation of the Earth [*], and Ferber's Oryctographia of Derbyſhire [†], or that which I gave myſelf of the environs of Buxton and of Caſtleton in my Tour through England and Derbyſhire [‡].

Caſtleton is not ſeen till you are, as I may ſay, above it, and till you arrive at the edge of a rapid and ſteep declivity, which interſects, in an abrupt and ſudden manner, this part of the mountain ſurrounding the baſon, at the bottom of which the ſmall village in queſtion is ſituated as in the middle of a funnel.

The revolution which divided this mountain, by tearing aſunder its ſides, aroſe, no doubt, from one of thoſe grand natural events ſimilar traces of which are to be found even in the High Alps; but here this terrible commotion, by opening the boſom of the earth, has uncovered ſeveral mineral riches which without this accidental circumſtance would never have been known.

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[*] Inquiry into the Original State and Formation of the Earth, &c. by John Whitehurſt. London, 4to. with plates.

[†] Eſſay on the Oryctography of Derbyſhire, by Mr. Ferber, tranſlated from the German.

[‡] Voyage en Angleterre, en Ecoſſe et aux Iles Hébrides, &c. 2 vols. 8vo. avec fig.

One of the precipices of this mountain is known by the name of Hay Cliff: it is calcareous, and contains ſhells and other marine productions in a ſtate of petrification. The other is called Man-Tor: it is alſo calcareous, and towards the foot there are ſome galleries of a mine, on a vein which terminates near the ſurface, and of which the matrix is a milky calcareous ſpar containing large-grained galena, but in ſmall quantity. There are found alſo towards the bottom of the ſame precipice, as well as in ſome other parts, fiſſile ſtrata of argillaceous ſchiſt, very often marly, which imbibe water in the time of rain, ſplit, and occaſion the ſolid and ſtony maſſes which reſt above them to crumble down[*].

Some of the ſchiſtous ſtrata in queſtion have a certain hardneſs; but all in general have a character of alluvion which an experienced eye cannot miſtake, and yet their formation is of the higheſt antiquity. It is at a depth which, without fear of being deceived, may be eſtimated at leaſt at four hundred and fifty feet below the upper ſtratum, that the foſſil caout-chouc has been found, and particularly the large fragment in the poſſeſſion of Mr. Mawe. In general it is in ſmall cavities, in a kind of neſts between the fiſſile ſtrata, that this bituminous matter is contained; and as in this ſtate it is ſheltered from the action of the air, it needs excite no ſurpriſe that it ſhould have experienced only very little alteration, and that its nature is not changed.

In the laſt place, one of the cavities, twenty-two inches in length and five inches in height, contained a great deal of caout-chouc. By theſe means very fine ſpecimens of it may be obtained for collections, at a very moderate price; and it is to Mr. Mawe that we are indebted for this obligation. This gentleman, who with his father-in-law Mr. Brawn poſſeſſes a large manufactory of ſparry fluor, of which they make elegant vaſes and other ornaments, carries on trade in a very honourable manner with the minerals of Scotland, Derbyſhire and other parts of England, a depôt of which he propoſes to eſtabliſh at Paris, which muſt, no doubt, be of advantage to the progreſs of mineralogy.

I divide the foſſil caout-chouc of Derbyſhire into elaſtic or compreſſible bitumen, and ſolid, hard, and brittle bitumen.

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[*] "The earth and ſtones which crumble down from this mountain," ſays Ferber, who viſited theſe places ſeveral years before I did, "form in ſeveral places ſmall hills, the ſize of which daily increaſes, and which the people conſider as one of the ſeven wonders of the Peak."—*Eſſai d'Oryctographie du Derbyſhire*, par Ferber, *Trad. Francoiſe*, p. 21.

SECTION I.

Compreſſible Bitumen.

Variety I. Elaſtic bitumen of a blackiſh brown and ſomewhat olive colour, ſoft, exceedingly compreſſible, unctuous and ſomewhat fat, of a ſlightly aromatic ſmell, but having a little of the inſipid odour of natural caout-chouc; inflammable, and burning with a bright brilliant flame, leaving a black oily reſiduum which does not become dry. The fragments of this variety are ſometimes an inch and a half in thickneſs and four inches in breadth. They are the largeſt except the fragment in poſſeſſion of Mr. Mawe, which is conſiderably larger.

Variety II. Foſſil caout-chouc, ſplit and cracked in the whole exterior ſurface, which was in contact with the matrix from which it has been detached. It is dry in that part, but nevertheleſs compreſſible; black on the ſurface expoſed to the contact of the air; but remarkable in this reſpect, that when a lamina of about a line in thickneſs is cut from this caout-chouc with a very ſharp knife, it appears in the inſide of a yellowiſh white colour: when cut in a very thick part, where the action of the air has not been able to render it black, like certain balls of the elaſtic gum brought ſometimes from Para, it has the ſame appearance as the uſual caout-chouc. Another circumſtance worthy of attention is, that in the part recently cut a kind of acid matter is obſerved to ooze out, which is not of a diſagreeable taſte, and which appears to be of the nature of the pyrolignic acid. This caout-chouc is pellucid on the edges, and almoſt of a hyacinth red colour.

Scarcely has the air exerciſed its action on the part recently cut, which is white, when it aſſumes a ſlight reddiſh tint, which daily becomes darker, and acquires a dark mahogany and then a blackiſh brown colour; at length, at the end of a month, if expoſed to the air, it becomes of a deep black.

Variety III. The ſame caout-chouc as the preceding, but of a ſomewhat firmer texture and a ligneous appearance. When viewed with a magnifying glaſs, it is obſerved that the fibrous undulations ſeen in this variety are merely the effect of the milky ſubſtance which flowed from the tree more or leſs ſlowly or at different times, and which did not acquire conſiſtence till it had loſt the mucilaginous water which held the matter of caout-chouc ſuſpended or in ſolution. I particularly mention this fact, to ſhow that this variety has no real relation to the wood of the tree which produced at that period the elaſtic bitumen.

Variety IV. Foſſil caout-chouc, compreſſible, and having a relation to the firſt variety, but of a darker colour, adhering to gray calcareous ſpar mixed with ſome grains of galena.

Variety V. Caout-chouc of a liver brown colour, much leſs unctuous to the touch than the firſt variety, but compreſſible, and having the aſpect of the real natural elaſtic gum; and the more remarkable, as it is found to become ſolid, as we may ſay, in ſome parts, where it acquires a much greater hardneſs, and even becomes brittle, and acquires a vitreous ſplendour. This gradual change is ſo ſtriking, that we cannot conſider this hard bitumen, which in the preſent ſpecimen is of a yellowiſh colour, as a hard bituminous matter accidentally enveloped by caout-chouc.

SECTION II.

Solid and Brittle Foſſil Caout-Chouc.

No. VI. Black ſolid caout-chouc, hard and brittle like jet, exceedingly brilliant on its fracture, which is conchoid, and ſometimes finely marked with ſtriæ diſpoſed in the form of rays, proceeding from the point of fracture, and expanding in the manner of a fan; electric by friction, opake throughout the whole maſs, but pellucid at the edges, and particularly when viewed in a ſtrong light: its colour is then a red, almoſt as bright and agreeable as that of the hyacinth, and analogous to the colour obſerved on the edges of the elaſtic caout-chouc of the ſecond variety, ſeen in the light which ſeems to announce their identity.

No. VII. Another variety of hard caout-chouc, ſimilar to that of No. VI. in hardneſs and ſplendour, but which is of a liver brown colour. It is entirely pellucid by the light of a lamp even throughout its whole ſubſtance, though opake in common day-light, and its colour is then ſimilar to that of the hyacinth.

No. VIII. The ſame variety as the above, but of a paler liver colour. It has beſides the ſame characters and the ſame properties as the other hard and brittle kinds of caout-chouc; but it is remarkable for ſtill adhering to its matrix, conſiſting of ſemi-tranſparent milky calcareous ſpar, with thick brilliant laminæ of galena. The caout-chouc here not only adheres to the calcareous ſpar but to ſulphurated lead, and is intimately mixed with both.

Mr. Mawe, in his Mineralogy of Derbyſhire, mentions the following very curious fact:—"A variety, the only one I poſſeſs of the kind, is elaſtic bitumen in a petrified marine ſhell incloſed in the rock;" alſo "another ſpecimen no leſs rare is obſcure caout-chouc, but tranſparent in the light, incloſed in cryſtallized fluor." Foſſil caout-chouc, according to Mr. Mawe, is found alſo in ſulphated barytes.

If it be conſidered that trees and other vegetables, which produce natural caout-chouc in ſuch abundance that the matter can flow down and accumulate at the root of them when the wind or any other accident lacerates the bark or breaks the branches, are all exotic, it will be allowed that this is a curious geological fact, which coincides with that of foſſil amber, which has been found, and is ſtill found, in ſome coal mines, and in turf ſoil of an antique origin, and which differs from that formed in the common turf moſſes. We are not yet acquainted with any vegetable productions which furniſh caout-chouc in abundance, but the following:

1ſt, The *vabea*, a ſpecies of apocinea, which grows in Madagaſcar, and of which Lamarc has given a figure in his *Illuſtrations de Botanique.*

2d, The *urceola elaſtica* of Sumatra and of Pullo-Pinang, diſcovered by Mr. Howiſon, an Engliſh ſurgeon at Pullo-Pinang, and deſcribed in the fifth volume of the *Aſiatic Reſearches* by Dr. Roxburgh [*]. This plant is of the family of the apocineæ.

3d, The *hevea Guianenſis*, deſcribed and exhibited in a figure by Aublet in his *Plantes de la Guiane*, is a large tree of the family of the euphorbia. It riſes to the height of more than forty feet; its trunk is ſometimes above two feet in diameter; and the natives of Para make bottles, boots, and other articles of the caout-chouc which diſtils from it. It is the ſame as that mentioned by M. de la Condamine in the Memoirs of the Academy of Sciences for 1736, which grows alſo in the province of Eſmeralda, in Peru, which the Maina Indians call *caout-chouc*, and of which they make bottles by means of earthen moulds: they uſe it alſo for torches to give them light.

4th, The *artocarpus integrifolia* of South America, a tree which approaches near to the mulberry and fig-tree.

5th, The *ficus religioſa.*

6th, The *ficus Indica.*

7th, The *hippomane biglanduloſa* (manchineal tree).

8th, The *cecropia peltata;* the two laſt among the euphorbia produce alſo a milky juice analogous to the caout-chouc.

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[*] See Philoſophical Magazine, vol. vi.

XLI. *A general View of the Coal Mines worked in France, of their different Products, and the Means of circulating them.* By C. Lefebvre, *Member of the Council of Mines, of the Philomatic Society, &c. &c.*

[Continued from p. 164.]

Department of La Côte d'Or.

SOME indications of coal have been announced here, and particularly in the communes of Areſnè, Turcey, Montbard, and Chevauney. They deſerve to be examined; and Champeaux, engineer of mines, now employed in that diſtrict, will no doubt give ſome accurate information on the ſubject: but hitherto no coal mines have been worked in this department; it can receive its coals from Blanzi by the canal of Charollois, and by going up the Saône as far as Saint-Jean-de-Lône, where the canal of Burgundy begins.

Department of the Côtes-du-Nord.

No coals have yet been found in this department. Indications of coal have been announced near Lannion and Quimper-Gaezence, in the neighbourhood of Pontrieux; but no attention has been paid to them. It is ſupplied therefore only by means of its ſea-ports, which may receive the coal brought from the mines of Litry, in the Calvados, and thoſe which abound in the northern departments, which may be conveyed by land to the canals which terminate at the ſea.

Department of La Creuſe.

Several coal mines (14) are worked in this country, which is ſtill little known in a mineralogical point of view, and which appears worthy of being carefully examined. The communes where coal mines in a ſtate of working are found, are thoſe of Couchezotte, Boſmoraud, Vavory, St. Palais, and Fautmazuras.

Though their production, according to the information obtained by the Council of Mines, is eſtimated in general at 126,000 myriagrammes, it certainly exceeds that quantity, becauſe ſeveral of the mines have not yet been worked with proper activity. The mean price of the coals at the pit is 10 cents per myriagramme.

Theſe mines are not much worked, becauſe there are very few means of conſumption.

If the Creuſe, which is ſaid to be navigable as far as Gueret, could be rendered navigable for boats to Ahun, it would open a very uſeful circulation for the coals of the mines in the neighbourhood of that commune, and would facilitate the tranſportation of them as far as the Vienne, to which it joins in the department of Indre-et-Loire. Theſe mines, then, would ſupply in part the conſumption of the department of Indre.

On the other hand, if it were poſſible to render the Thirion, which paſſes Bourganeuf, navigable from that town to its junction with the Vienne above Limoges, and to enſure the navigation of the Vienne from Limoges to Châtelleraux, where it begins to bear boats, an extenſive conſumption would be opened for the coals which are on the north and ſouth of Bourganeuf, and make this fuel circulate in the departments of La Haute-Vienne and of La Vienne. This circulation would be exceedingly uſeful, and increaſe manufactories and induſtry.

Department of La Dyle.

This department poſſeſſes no coal mines; but it borders on that of Jemappes, the numerous mines of which ſupply more coals than it has occaſion for. It is of importance, therefore, that this fuel, the uſe of which is general in that country, may be obtained at a price ſuited to the different places where it is conſumed; and that the roads be repaired and carefully maintained in a proper ſtate. Without this attention, the dearneſs of carriage would throw the inhabitants of La Dyle into a deplorable ſtate of diſtreſs in regard to fuel, and produce a hurtful ſtagnation in the mines of the department of Jemappes.

Department of La Dordogne.

The cantons of Cranſac and Teraſſon (15) afford collections and ſtrata of coals of a good quality and exceedingly rich. Theſe mines would be an object worthy being worked with activity, if the navigation of the Veſere were rendered more certain, and eaſier.

At preſent they are worked only at the ſurface by ſome proprietors of land, and ſupply only the local conſumption. The product of theſe coal mines is not known; but it is certain that the working of them would be eaſy, and that they are capable of affording great reſources for a long time.

Department of Doubs.

Several indications of coals have been announced in this department (16). Reſearches even have been made in different parts, but hitherto no coals have been dug up.

According to the general meaſures which have been taken by the government, this department is one of thoſe where an engineer of mines will be employed; and we have reaſon to hope that reſearches will be made, and followed with ſucceſs.

A very conſiderable maſs of foſſil bituminous wood has been diſcovered at Grand-Denis, in the neighbourhood of Ornans, in the commune of Flanchebouche. This ſubſtance has not entirely the ſame qualities as coals; but in the ſtate in which it is found at Grand-Denis it may be advantageouſly employed for ſeveral purpoſes, and particularly under boilers. The managers of the ſalt-works, who have cauſed trials to be made with it, are going to employ it for evaporating the water of the ſalt-work of Montmorot. This will produce a great ſaving in the wood employed in that manufactory.

Department of La Drôme.

The diſcovery of coal mines in this department (17) has often been announced; but, according to the reports of the engineers of mines who have viſited them, it appears that theſe indications were nothing elſe than bituminous foſſil wood, which is frequently found in ſtrata of ſand, and particularly in the environs of Creſt in the diſtrict of that name, and in the territory of ſeveral communes in the environs of Nions.

This foſſil wood has been dug up, and particularly at Creſt; and though it cannot be applied to the ſame purpoſes as coals, it is ſtill uſeful in theſe countries in the manufactories for ſpinning ſilk.

The department of La Drôme may receive abundance of coals, at leaſt for ſupplying the communes which lie near to the banks of the Rhone, as thoſe of the departments of Ardeche and La Loire can be conveyed to them on that river.

Department of the Scheldt.

The mines of the departments of the North and of Jemappes furniſh abundance of coals to the inhabitants of this department, in which no coal mines are worked.

Department of Eure.

In this department no coal mines are known. It cannot procure this fuel but by going down the Seine to its mouth, or unleſs ſome arrive by ſea at Honfleur. This port may be ſupplied from the coal mines of Litry, in Calvados, or from thoſe of the departments of the North.

Department

Department of Eure-et-Loire.

As this department, like the preceding, has no mines in a ſtate of being worked, it can procure no coals but thoſe conveyed down the Loire as far as Orleans, which ſupply its ſouthern part, and thoſe circulated on the Seine, which ſupply the northern. But the land carriage there muſt render this fuel very dear.

Department of Finiſterre.

There are no coal mines in this department (18) which can be conſidered in a ſtate of productive working: however, in conſequence of old indications, ſome reſearches have been within theſe few years reſumed in the neighbourhood of Quimper.

Theſe reſearches have afforded ſome hopes. Small veins of coal have even been found; and as a mine of this kind would be of great importance to the port of Breſt and the other ſea ports and arſenals of this department, the miniſter of the marine has cauſed theſe reſearches to be continued near Quimper with great activity, according to plans approved by the Council of Mines.

Several other indications have been announced at Cleden and at the bottom of the Bay of Dinan. C. Berth had given in the year 6 ſome information in regard to this diſtrict; in conſequence of which he was allowed to continue the reſearches he had begun, and to put himſelf in a condition to obtain a grant when his diſcovery ſhould be fully eſtabliſhed. C. Berth ſet out for Egypt, and it does not appear that attention has ſince been paid to his labours: a coal mine in this point would be of the more importance, as it would be within reach of the harbour of Breſt.

Department of Forêts.

No coal mines are worked in this department, which may receive in the ſouthern part the product of the abundant mines in the environs of Saarbruck, and the coals which may be conveyed on the Moſelle.

Department of Gard.

This is one of the departments of the ſouth of France (19) where this kind of fuel is moſt abundant. On the north of Alais the mines of Cendras, of Portes, of the Foreſt of Abilon, La Grand-Combe, and Pradel, furniſh about 2,200,000 myriagrammes of coals per annum.

The coal mines of Banes, Robillac, Meranes, Saint-Jean-de-Valeriſque, furniſh at leaſt 900,000 myriagrammes.

Several ſtrata of coal are ſtill worked in the environs of Pont-Saint-Eſprit, and towards Laudun; but it is inferior in quality to the before-mentioned. This is the more unfortunate, as the ſituation of theſe mines on the banks of the Rhone would render them of great importance on account of the facility with which they could be tranſported.

If the coal mines in the environs of Alais poſſeſſed the ſame advantages in regard to tranſportation, the quantity dug up might be increaſed to ten times the preſent quantity, without any fear of the mines being exhauſted in many years. But they cannot be exported from the country, in conſequence of the dearneſs of carriage.

The mean price of the large coals at theſe mines is 7 cents per myriagramme, and that of the ſmall coals from 4 to 5 cents.

It appears that a canal is now conſtructing from Nimes to Saint-Gilles. The mineral riches and abundant reſources in fuel, which the mines in the neighbourhood of Alais would furniſh for ages, are of ſo much importance, that a communication ought to be opened with that country by means of a counter-canal to ſupply the place of the bed of the Gardon, which is not navigable.

In this department there are alſo ſome ſtrata of coal worked in the environs of the commune of Vigan. At preſent the working is not carried on with much activity; ſome of the pits have even been abandoned in conſequence of law-ſuits, which are on the point of being decided.

The common amount of the product of this canton may be eſtimated at leaſt at 200,000 myriagrammes.

Theſe mines cannot find much conſumption except by the Herault; but this river is not navigable in the neighbourhood of Vigan.

Department of La Haute-Garonne.

In this department there are no coal mines worked. Some ſoundings have been made in the environs of Toulouſe, in conſequence of ſome ſuppoſed indications. Theſe ſoundings have not confirmed the hopes which had been conceived.

Foſſil wood in the ſtate of very fine jet has been found in the Foreſt of Montbrun towards Monteſquieu and Rieux. Specimens of it may be ſeen in the collection of the Council of Mines.

The department of La Haute-Garonne may be ſupplied with coals in the eaſtern part by the mines of Carmeaux, in the department of Tarn. The products of theſe mines are embarked on the Tarn, and enter with that river into the northern part of the department of La Haute-Garonne. But the difficulties which occur in the navigation of the Tarn greatly increaſe the price of the coals of Carmeaux. This inconvenience might be removed without much expenſe. We muſt therefore hope that meaſures will be purſued to remove theſe obſtacles, which are equally prejudicial to induſtry, to commerce, and to all the conſumers of theſe departments. The coals deſtined for Toulouſe and the ſouthern part of the department are depoſited at Port Saint-Sulpice, from which they are tranſported by land carriage.

Department of Gers.

This department, in which no coal mines are known, can obtain this fuel only by land carriage from the entrepôts of Toulouſe, or from ſome of the ports of La Garonne in its northern part.

Department of La Gironde.

Collections of bituminous foſſil wood depoſited in ſtrata of ſand have been diſcovered in ſeveral places in the neighbourhood of Bourdeaux. As this foſſil wood ſeems to be diſperſed in great abundance, it would be of advantage to purſue the reſearches which have been made, with more conſtancy than has hitherto been the caſe.

Theſe reſearches might not conduct to the diſcovery of a coal mine; but the foſſil wood might be uſed with great advantage as fuel in different operations for which wood is employed.

There are no coal mines worked in this department, but it receives by the Garonne the coals of Carmeaux. This fuel might alſo be procured with more eaſe from the abundant coal mines which are known between Teraſſon and Bergerac, on the banks of the Veſere and the Dordogne, if the navigation of theſe rivers were rendered eaſier.

This department might receive alſo by the mouth of the Gironde the products of the rich mines of the departments of the North. An exchange of the coals, iron works, and of a variety of other articles manufactured in theſe departments, for the wines and brandy of the Bordelais, which are in requeſt in the whole North, might be made by the French naval trade as well as the Engliſh. It is even probable that we ſhould obtain great advantages for the ironmongery trade in India, in conſequence of the low price at which the manufactories of the north of France could furniſh theſe articles, of which Bourdeaux would be the entrepôt either for America or for the Eaſt Indies.

Department of Herault.

Coal mines are frequently met with in this department (20). The canton of Bedarieux affords ſome immenſely rich, ſuch as thoſe of Saint-Gervais, of Camplong, of Bouſſaque, and Graiſſeſſac; towards the ſouth, in the canton of Roujan, thoſe of Bouſquet, in the commune of Neſſies; towards the ſouth-weſt, in the canton of Saint-Chiniau, the mines of Ceſſenon; and towards the ſouth, near the canal of the two ſeas, thoſe of Azillanet. Some have been found alſo in different places in the environs of Montpellier. A leaſe is about to be granted for working thoſe of Saint-Gely-du-Feſq.

Though the greater part of the coals procured from theſe mines is not of the beſt quality, they are a great relief, in conſequence of the dearneſs of wood, and of the numerous manufactories in which they are employed.

But in general, the means of circulation in the interior of this department are neither numerous nor eaſy. The coals of the mines in the neighbourhood of Bedarieux, which might furniſh a great many, cannot conveniently be tranſported. Some individuals are ſoliciting for grants ſubject to the condition of making a road, which would ſoon give more importance to theſe mines by facilitating the carriage of them to Bedarieux. It would be neceſſary alſo to have a communication with the canal of the two ſeas, leſs expenſive than that by land. It appears that the river Ourbe might eaſily be rendered navigable from Bedarieux to Bezieres. It would then anſwer very well the propoſed object.

The price of coals at theſe mines is 15 cents per myriagramme. But this price is doubled when they have been tranſported as far as the canal.

The coal mines which are neareſt, ſuch as thoſe of Azillanet, are attended with ſome advantages in this reſpect.

The whole product of the different mines of this department may be eſtimated at 1,800,000 myriagrammes.

In conſequence of the ſuperior quality of the coals of Carmeaux, in the department of Tarn, they are received by the conſumers along the canal, in preference to thoſe already mentioned, though the price is nearly double.

Department of Jemappes.

A very large portion of this department (21), and particularly its ſouthern part, may be conſidered as an immenſe maſs of coals, in ſome places ſcarcely covered by ſtrata more modern than the depôts of that mineral.

The geologue, in traverſing the interior of theſe mines, is aſtoniſhed at the varied phænomena exhibited by the numerous and ſucceſſive ſtrata of coals, the inflection, curvatures, or turns of which, in an inverſe direction and paralleliſm, open a vaſt but difficult field for conjectures reſpecting their formation, and the revolutions of the globe which muſt have produced theſe reſults: but to enter here into a conſideration of theſe wonderful effects, a deſcription of which muſt be reſerved for particular treatiſes on the operations of nature, would lead me too far from my preſent object.

I ſhall therefore content myſelf with giving ſome idea of the reſources which this country furniſhes, not only to its inhabitants, but to thoſe of ſeveral other departments; by which it will be ſeen how much its productions in coal might be increaſed; and that they would be ſufficient to ſupply for many years all thoſe parts of France which receive coals by ſea from foreign countries.

More than 3000 pits are worked in the environs of Jemappes, Mons, and Charleroi. They are far from being carried to their maximum of activity. The ſum of their products, however, amounts, at the leaſt, to 220,000,000 myriagrammes annually; and this product might eaſily be doubled, if the conſumption increaſed in the ſame proportion.

In regard to the quality of theſe coals, it is exceedingly various: theſe numerous mines furniſh coals of every kind, and the price at the pits differs according to the quality from 5 to 9 cents per myriagramme.

The places where they are conſumed are, the departments of the Sambre and Meuſe, La Dyle, the Scheldt, the Deux-Nethes, and Batavia, in competition with the Engliſh coals.

The means of circulation are the river Haine, the Scheldt, the canals which communicate with theſe rivers, the Sambre and the Meuſe, and the large roads from Mons and Charleroi to Bruſſels.

It may be readily conceived of how much importance it would be to this department, which employs a great number of hands in working its coals, and alſo to thoſe countries to which theſe coals might be tranſported, if the canals and roads were kept in a ſtate of better repair.

Every degradation of the roads which retards conveyance, or which renders more horſes neceſſary, multiplies expenſe and increaſes the price of the coals; which ought to be kept

as low as poſſible, both on account of their utility in our manufactories, and of the neceſſity of maintaining a competition with the Engliſh coals.

The carriage by the cauſeways from Charleroi and Mons to Bruſſels adds greatly to the price of theſe coals. A canal has been projected for uniting the Sambre towards Thuin with the ſmall river of Senne; which would convey the coals of the mines in the neighbourhood of Charleroi to Bruſſels, and would thus form a communication with the Scheldt and Batavia, ſo as to prevent the neceſſity of land carriage.

This canal, valuable in this point of view, would be of utility alſo to the export trade of theſe countries in regard to the glaſs and nails ſent to Holland and to our own ports, and which are thence conveyed to the Indies.

It would be of utility alſo to government for tranſporting the timber of the Foreſt of Souane, through which it would paſs.

To point out theſe improvements will, no doubt, be ſufficient to induce government to pay ſerious attention to them; but until new means of circulation be provided, it is of importance that the exiſting roads ſhould be properly repaired.

I ſhall terminate this article by obſerving, that it is the intereſt of France to facilitate its commercial intercourſe with Holland, and particularly that which relates to the importation of our coals into that country: conſequently, inſtead of laying a duty on their exportation to the amount, as is ſaid, of 20 per cent., this branch of commerce ought to be entirely free. It would be good policy alſo to encourage by bounties the exportation of the coals of this department, eſpecially when deſtined for French ports in parts of our territory which cannot receive this fuel from the mines of the interior, and which if not ſupplied by our mines would procure them from foreign countries.

[To be continued.]

XLII. *On a new Kind of Painting with the Serum or watery Part of the Blood.* *By* F. Carbonel, *M.D. of Barcelona* *.

THE advantages and utility of painting have at all times excited the emulation of profeſſors and of artiſts. The progreſs of this art, which imitates nature, has fortunately correſponded to their wiſhes; but its advancement would cer-

tainly have been more rapid if artiſts had been ſooner made acquainted with the new chemiſtry. This aſſertion is proved by the preſent diſcovery.

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* From the *Journal de Phyſique*, Ventoſe, an. 11.

The neceſſity of painting ſurfaces of different kinds, their various ſituations, and the complex circumſtances which attend the application of colours, have given riſe to the different kinds of painting which are known; ſuch as painting in freſco, painting in ſize, oil-painting, painting with milk, painting on porcelain, painting with wax, painting on glaſs, &c.

The journey of the king of Spain to Barcelona, and the preparation of the edifices deſtined for the uſe of their majeſties, afforded me an opportunity of ſolving a new problem in regard to this art. It was neceſſary to diſcover ſome kind of painting of the common colour of ſtone which might be able to reſiſt rain, which ſhould be unalterable in the ſun, eaſy to be applied on wood, and which ſhould dry ſoon and exhale no bad odour; in a word, a painting equally durable with that in ſize, and which ſhould have the property of being able to withſtand the inclemency of the weather. The ſolubility of gelatin in water, the bad ſmell of oil, the weak adheſion of milk of lime to wood, and its ready alteration by heat, made me ſenſible that it would be impoſſible to have recourſe to any of the known vehicles for ſecuring the colouring matters. After ſome examination I found that albumen * was the only ſubſtance which could anſwer the required conditions, on account of its inſolubility in water, and the conſiſtence it acquires by combining with calcareous earths. This ſubſtance, prepared by the vital power in the animal organs, is found almoſt pure in the white of an egg or albumen, and is found in combination in different animal ſubſtances. But the great viſcidity of the white of an egg, the clots which are formed by its combination with lime, and particularly its dearneſs, made me give up all idea of employing it for painting a whole edifice.

I then conceived that the ſerum of blood, the principal part of which conſiſts of albumen, would be attended with all the advantages of white of egg for the propoſed end, without having any of its inconveniences. Chemical analyſis of animal ſubſtances, indeed, ſhows that the ſerum of blood, commonly called the water of the blood, or, improperly, the lymph of the blood, is a kind of animal mucilage compoſed of albuminous or gelatinous ſubſtances in different propor-

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* This painting was much wanted for the bridge which joins the *hotel de la Douane* to the palace of the captain-general, as no other kind could be employed for fear of injuring the queen's health.

tine, diſſolved in an indeterminate quantity of water, ſaturated partly with pure ſoda, which renders the albuminous ſubſtance of an almoſt ſaponaceous taſte. There is found alſo in the ſerum muriate of ſoda, of lime, and of ammonia. But theſe ſaline ſubſtances are not eſſential to this combination, ſince they not only may vary in their proportions, but be wanting either in whole or in part; and even more in the ſerum of other fluids, ſuch as the muriate of potaſh, the phoſphate of magneſia, &c., without the albuminous, alkaline, or gelatinous ſubſtance of the ſerum being changed, or exhibiting different characteriſtic properties.

This doctrine, received by all the celebrated modern chemiſts, induced me to think that the albuminous ſubſtance of ſerum, conſidered in its combination with gelatin and ſoda, ought to form with calcareous earth a compoſition which would retain no more of that viſcidity and tenacity afforded by the mixture of the white of an egg and lime, than what might be neceſſary for anſwering the propoſed end.

Theſe reaſons, and the low price of this liquid in large cities, ſoon induced me to try the effects of the ſerum of blood with pulverized quicklime; and I found, with much ſatisfaction, that my firſt trials correſponded in every reſpect to the hopes I had conceived.

Practice and Application of the new Kind of Painting.

The preparation of this new kind of paint is eaſy, and requires very little time. Nothing is neceſſary but to pulverize the lime, and to dilute it in the ſerum of blood until it forms a liquid of ſufficient thickneſs for painting; which is then to be applied in the uſual manner. The colour which this compoſition acquires is whitiſh, and more or leſs ſhining, according to the purity of the ſerum and the whiteneſs of the lime. This earth may be employed either in the ſtate of quick or cauſtic lime reduced to powder, or well ſlaked with a ſmall quantity of water to leſſen the adheſion of its integrant parts. It muſt then be reduced to a fine powder, and be ſifted through a cloſe ſieve. If kept for ſome days, it is neceſſary to preſerve it in a cloſe tub or earthen veſſel, to prevent its combination with the carbonic acid and water of the atmoſphere. The ſerum is ſeparated from the curdled blood by decantation: but care muſt be taken to ſhake the matter as little as poſſible, and to ſuffer it each time to remain at reſt.

In this compoſition two things only are to be obſerved: the firſt is, that ſerum being a fluid highly ſuſceptible of corruption, it muſt be employed the ſame day it is ſeparated; or

at furtheſt the day following, provided it be kept in a cool place ſheltered from the contact of the air, eſpecially during the ſummer, when it alters much more eaſily.

In caſe of doubt, inſpection of the fluid alone may ſerve to determine its quality; for the ſigns of its fermentation are very evident. This obſervation is applicable alſo to the colouring liquor, but in a leſs degree. It follows from this circumſtance, that the veſſels which have been uſed for containing, preparing, or applying paint made with ſerum ought to be waſhed every day. The ſecond precaution to be taken is, that as the re-action of the ſerum and lime increaſes in the courſe of time, as well as the denſity of the mixture, in order that it may be preſerved in the neceſſary ſtate of fluidity, or to diminiſh or increaſe it according to the nature of the ſubſtances to be painted and the number of ſtrata to be applied, another veſſel filled with ſerum muſt be provided, to give to the colouring liquor the neceſſary fluidity. It thence follows that the mixture muſt be prepared as it is uſed.

Having aſſured myſelf of the ſucceſsful and conſtant effects of this kind of painting, I tried to mix it with different colouring ſubſtances, to render the application of the invention more general.

The well known re-action of animal ſubſtances and of lime on the metallic oxides, and particularly on thoſe in which oxygen is united to the metal with little force, made me ſuſpect an alteration of ſome colours when mixed with this liquid; an alteration naturally produced by the ſeparation of a part of the oxygen from the metallic oxides. On the other hand, the analogy which coloured chalks have with calcareous earth induced me to believe that their combination would be attended with the happieſt reſult. The oxides of lead, copper, and arſenic, indeed, undergo a ſingular alteration in their reſpective colours, when they are united with ſerum and calcareous earth: but this is not the caſe with cobalt green; which on that account appears to me to be very proper for being uſed as a colouring matter in this new kind of painting. Thoſe earths, however, called red, green, black, or yellow, which are of an argillo-calcareous nature, when mixed with theſe ſubſtances, may furniſh different agreeable colours of a durability equal to the blueiſh colour produced merely by the mixture of lime and of ſerum.

I have however remarked, that the durability of this painting is conſiderably altered when there is mixed with it a very large quantity of colouring earth, ſuch as the black earth uſed for painting an iron colour, or the repreſentation of white marble with black ſpots. To remedy this inconvenience, notions, diſſolved in an indeterminate quantity of water, conſtantly united to pure ſoda, which reduces the albuminous ſubſtance to an almoſt ſaponaceous ſtate. There is found alſo in the ſerum phoſphate of ſoda, of lime, and of ammonia. But theſe ſaline ſubſtances are not eſſential to this combination, ſince they not only may vary in their proportions, but be wanting either in whole or in part; and even unite in the ſerum to other ſalts, ſuch as the muriate of potaſh, the phoſphate of magneſia, &c., without the albuminous, alkaline, or gelatinous ſubſtance of the ſerum being changed, or exhibiting different characteriſtic properties.

This doctrine, received by all the celebrated modern chemiſts, induced me to think that the albuminous ſubſtance of ſerum, modified by its combination with gelatin and ſoda, ought to form with calcareous earth a compoſition which would retain no more of that viſcidity and tenacity aſſumed by the mixture of the white of an egg and lime, than what might be neceſſary for anſwering the propoſed end.

Theſe reaſons, and the low price of this liquid in large cities, ſoon induced me to try the effects of the ſerum of blood with pulverized quicklime; and I found, with much ſatisfaction, that my firſt trials correſponded in every reſpect to the hopes I had conceived.

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plaſter and glue, to prevent its re-action on the colouring parts of the painting.

The hardneſs of this ſubſtance, and its ſuſceptibility of receiving a poliſh, induce me to believe that it might be employed on ſtucco, and for imitating it on walls and ceilings.

It may be uſed alſo for uniting water-conduits, and for cementing ſtones of all dimenſions, and eſpecially thoſe which are put into wood caiſſons for building under water.

It appears from different works, that the antients had conceived the idea of employing blood in the compoſition of their cements and mortar; but the uſe of it in painting was before unknown. Beſides, there was too great a diſtance between this uſe of blood and that of ſerum freed from the fibrous part and the colouring matter, to admit of any doubt that the principle of my diſcovery is entirely my own.

It follows from what has been here ſaid, that this new kind of painting may be added to thoſe already known, and that it may be called painting *in ſerum*; that it is as eaſily applied as prepared; that the elements of it are common, and of little value; and that it unites in it all the advantages of painting in oil and in diſtemper, without having any of their inconveniences.

XLIII. *On the Kind of Air-Furnaces employed in Iron Foundries for caſting large Pieces of Ordnance, Shafts for Mills, Cylinders, and other heavy Articles.* By Mr. DAVID MUSHET, *of the Calder Iron Works**.

THE furnaces about to be deſcribed are employed for melting pig-iron with the flame of pit-coal. Furnaces of this kind are conſtructed of various ſizes according to circumſtances. The ſmall ſizes will run down from ſeven to ten hundred weight, and are uſed in ſmall foundries for what the trade call *jobbing*.

Fig. 1. (Plate IV.) a ground plan of two large air-furnaces, and chimney for melting pig- or caſt-iron with the flame of pit-coal.

The letters A B C D point out the exterior dimenſions of the ſtalk or chimney, which is firſt erected, leaving two openings or arches into which the fore-part of the furnaces are afterwards built. The breadth of the chimney at the particular place which the plan exhibits is 16 feet from A to B, and from A to D or from B to C 6 feet 6 inches. The plan is drawn at that elevation where the flame enters the chimney

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* Communicated by the Author.

by the flue or throat, narrowed on purpoſe to throw back part of the flame, and keep the furnace equally hot throughout, as may be more particularly viewed in the vertical ſection, fig. 2.

E E, the furnace bars on which the coals reſt, and where the combuſtion is maintained.

F F, openings called teaſing-holes, by which the coals are introduced to repair the fire.

G G, fire-brick buildings called bridges. Theſe are meant to concentrate the flame, that it may act as violently on the metal as poſſible. Upon the height of the bridge much depends in fuſing the metal ſpeedily, and with little loſs. The height of this may be ſeen in the vertical ſection, fig. 2. G.

H H, the charging doors, by which the metal is introduced in the ſhape and ſtate of pig-iron, lumps, ſcraps, &c. &c. The iron generally occupies the furnace acroſs to I, called the back wall, and is never meant to approach the bridge nearer than the dotted line, leſt the metal in melting ſhould run back into the grates, in place of deſcending into the general reſervoir or cavity below. The corners or notches, *b*, *b*, *b*, *b*, receive a ſtout caſt-iron frame lined with fire-bricks. This is hung by means of a chain and pulley, and can be raiſed and depreſſed at pleaſure. This frame is, properly ſpeaking, the charging door, and is always carefully made air-tight by means of moiſtened ſand.

K K, the flues or openings by which the flame enters the chimney. Theſe are 15 inches by 10. On maintaining theſe openings of a proportionate ſize to the other parts depend in a great meaſure the powers and œconomy of the furnace.

L L, lading doors, by which ladles are introduced, in the caſe of ſmall furnaces, to lift out the metal and diſtribute it to the various moulds.

M M M M, binding bolts to limit within proper bounds the expanſion which takes place in the building when the furnace is highly heated.

Fig. 2. vertical ſection of one of the furnaces, and its appropriate ſtalk or chimney.

E, the grates.

F, the teaſing-hole.

G, the bridge.

H, the charging door.

K, the flue or opening into the chimney.

L, the lading door.

M M, the binder or binding bolt.

N, the interior of the ſtalk or chimney, 30 inches ſquare.

O O, the fire brick-work, 9 inches thick.

P P, ſpace

PP, ſpace of 2 inches for ſtuffing with ſand.

QQ, common brick building.

RR, caſt-iron lintels, over which are thrown double 9-inch arches, ſo that at any time the inferior building can be taken down to make repairs, without ſhaking or in the leaſt injuring the chimney.

S. The dotted lines here are meant to repreſent what is called the tapping-hole. When a large piece of goods is to be caſt, lifting the metal with ladles would be impracticable. A ſharp-pointed bar is driven up this opening. The iron then flows freely out into a large baſon of ſand made for its reception. It is then conducted, by collateral channels, into the mould.

The ſpace under the curved dotted line from G to L, by S, is filled with a mixture of ſand and aſhes. When the furnace is prepared to melt, the whole of the bottom receives a ſtratum of ſharp clean ſand about two inches thick. This is broken up at night, and freſh ſand is ſubſtituted for it before the fire is kindled in the morning.

Fig. 3. is a horizontal ſection of the chimney or ſtalk, taken where the flues aſſume a perpendicular direction. The letters in this figure correſpond to thoſe in the vertical ſection, fig. 2. The height of the chimney ought not to be leſs than 45 feet; if 50 feet, the effect will be ſooner and of courſe better produced.

Remarks upon the Melting of Iron in theſe Furnaces.

The effect wiſhed to be produced in air furnaces is the fuſion of a certain portion of pig- or caſt-iron for the purpoſe of being poured or run into moulds to form articles of almoſt every deſcription.

The preparation previous to melting is as follows:—After the bottom of the furnace is laid, and ſmoothed with freſh ſand, and all the openings made air-tight, the furnace-man introduces a kindling at the teaſing-hole, accompanied with new pit-coal. In a few minutes a conſiderable volume of dark flame mixed with ſmoke is produced. The fire quickly gathers ſtrength; more coal is introduced; and the furnace now becomes filled with a yellow-coloured flame. By continuing this operation for an hour, or an hour and a quarter, the furnace and flame will have become completely white; the latter ſteady, and at times apparently without motion. The furnace-man now judges the bottom to have been ſufficiently hardened for receiving the pig-iron without any riſk of ſinking. The charging-door is now opened, and the pig-metal thrown carefully and regularly upon that part of the bottom formerly deſcribed as being appropriated for its reception. The door is again cloſed and made air-tight, and the operation of firing continued with unremitting care and attention.

The time of melting depends entirely upon the quantity of metal introduced. The furnaces deſcribed above are capable of melting from 50 to 60 hundred weight of metal each, and when there is a moderate circulation of air they will perform this work in 2½ or 3 hours. In half an hour after the metal is introduced it aſſumes a blackiſh red colour. It then begins to brighten with every additional fire, and in about one hour appears white, and begins to loſe ſhape, and reſemble a wreath of ſnow.

An eye accuſtomed to ſuch heats will now diſcern the metal beginning to drop, and run down the inclined plane in very beautiful ſtreamlets reſembling quickſilver. Eight or ten of theſe are viſible at a time, and after proceeding half way down begin to form junctions with each other, and flow connected into the general cavity or reſervoir. By-and-by this becomes filled, and literally forms a beautiful molten mirror, in which ſometimes part of the interior furnace is reflected.

The furnace-man, by ſearching at the bridge with his fire-iron or teaſer, judges when the metal is nearly all gone. Of this he is certain by looking up from the peep-hole of the lading-door. If the ſtreamlets of the running metal have ceaſed, then the whole is melted, and ready for running out.

In the operation of melting, the three following circumſtances ought to be particularly attended to: the thinneſs or hotneſs of the metal; the waſte or loſs ſuſtained in melting; and the quantity of coals employed.

The firſt is of the utmoſt importance, as many articles in the foundry buſineſs require the metal in a ſtate of the greateſt diviſion; otherwiſe they will be found imperfect when taken from the ſand, and unfit for ſale. The furnace-man, therefore, is always on the watch to replace the fire as it decays, and keep a large and ſharp volume of flame conſtantly paſſing over the metal.

The waſte or loſs of real metal is alſo an object of great importance. This always bears a relation to the quality of the iron, the ſtrength and cleanneſs of the coals, and the judgment and attention of the melter. Strong iron is found always more difficult to fuſe; this neceſſarily expoſes it for a long period in contact with the flame. The reverſe happens with metal that is more fragile, and eaſier broken in the pig. The length of the expoſure in fuſing depends on this; and other circumſtances being alike, the loſs or waſte of metal will alſo be in the ſame ratio.

There are, however, other facts not unworthy of notice. No. I. pig-iron, or richly carbonated metal, when run from an air-furnace, will be found in point of quality little better than No. II. or carbonated iron. This is owing to a quantity of its carbon being deſtroyed during the fuſion. The loſs in melting No. I. iron, therefore, chiefly conſiſts of carbon; and the deficiency of metal ought never, with a clean bottom, to exceed 1 cwt. in 20.

Carbonated or No. II. iron alſo becomes deprived of a conſiderable portion of its carbonaceous mixture in fuſion; and when run from the air-furnace is ſeldom better than No. III. metal. The loſs ſuſtained in melting may be averaged at 7½ per cent.

No. III. pig-iron is, after melting in an air-furnace, found whitiſh or mottled. It is ſeldom ſuſceptible of the ſame nice degree of diviſion as the ſuperior qualities, and loſes in fuſion a much larger proportion of metal, ſeldom under 10 per cent. and frequently 12½ or 15.

The quantity of coals requiſite to melt a given quantity of iron is various, as much depends upon the quality and fuſibility of the metal. If the furnace goes one heat a day with No. I. or II. iron, the quantity of coals will be from 20 to 25 cwt. for a ton of iron. If two or three heats a day, or as many tons of iron are melted at one kindling, the proportion of coals will be nearly weight for weight of the iron melted when the coals are mixed with a fair proportion of ſmall: with ſtrong large ſplint coals, one ton of good pig-iron may be completely reduced with from 12 to 15 cwt. including the previous heating of the furnace.

XLIV. *Obſervations on the Freezing of Water and the Nature of Snow: in a Letter from Profeſſor* DRIESSEN *to Dr.* VAN MARUM[*].

AS the preſent ſeaſon has afforded us an opportunity to make experiments on the freezing of water and the nature of ſnow, I flatter myſelf that it will give you ſome ſatisfaction to be made acquainted with the reſults I have obtained in regard to this important ſubject; and I give you full liberty to publiſh them, if you think they are likely to be of any utility.

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[*] From *Algemeene Konſt en Letter-Bode*, No. 14, 1803.

At

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[To be continued.]

XLII. *On a new Kind of Painting with the Serum or watery Part of the Blood. By* F. CARBONEL, *M.D. of Barcelona* [*].

THE advantages and utility of painting have at all times excited the emulation of profeſſors and of artiſts. The progreſs of this art, which imitates nature, has fortunately correſponded to their wiſhes; but its advancement would certainly

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tainly have been more rapid if artiſts had been ſooner made acquainted with the new chemiſtry. This aſſertion is proved by the preſent diſcovery.

The neceſſity of painting ſurfaces of different kinds, their various ſituations, and the complex circumſtances which attend the application of colours, have given riſe to the different kinds of painting which are known; ſuch as painting in freſco, painting in ſize, oil-painting, painting with milk, painting on porcelain, painting with wax, painting on glaſs, &c.

The journey of the king of Spain to Barcelona, and the preparation of the edifices deſtined for the uſe of their majeſties, afforded me an opportunity of ſolving a new problem in regard to this art. It was neceſſary to diſcover ſome kind of painting of the common colour of ſtone which might be able to reſiſt rain, which ſhould be unalterable in the ſun, eaſy to be applied on wood, and which ſhould dry ſoon and exhale no bad odour; in a word, a painting equally durable with that in ſize, and which ſhould have the property of being able to withſtand the inclemency of the weather. The ſolubility of gelatin in water, the bad ſmell of oil, the weak adheſion of milk of lime to wood, and its ready alteration by heat, made me ſenſible that it would be impoſſible to have recourſe to any of the known vehicles for ſecuring the colouring matters. After ſome examination I found that albumen * was the only ſubſtance which could anſwer the required conditions, on account of its inſolubility in water, and the conſiſtence it acquires by combining with calcareous earths. This ſubſtance, prepared by the vital power in the animal organs, is found almoſt pure in the white of an egg or albumen, and is found in combination in different animal ſubſtances. But the great viſcidity of the white of an egg, the clots which are formed by its combination with lime, and particularly its dearneſs, made me give up all idea of employing it for painting a whole edifice.

I then conceived that the ſerum of blood, the principal part of which conſiſts of albumen, would be attended with all the advantages of white of egg for the propoſed end, without having any of its inconveniences. Chemical analyſis of animal ſubſtances, indeed, ſhows that the ſerum of blood, commonly called the water of the blood, or, improperly, the lymph of the blood, is a kind of animal mucilage compoſed of albuminous or gelatinous ſubſtances in different propor-

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This doctrine, received by all the celebrated modern chemiſts, induced me to think that the albuminous ſubſtance of ſerum, modified by its combination with gelatin and ſoda, ought to form with calcareous earth a compoſition which would retain no more of that viſcidity and tenacity aſſumed by the mixture of the white of an egg and lime, than what might be neceſſary for anſwering the propoſed end.

Theſe reaſons, and the low price of this liquid in large cities, ſoon induced me to try the effects of the ſerum of blood with pulverized quicklime; and I found, with much ſatisfaction, that my firſt trials correſponded in every reſpect to the hopes I had conceived.

Practice and Application of the new Kind of Painting.

The preparation of this new kind of paint is eaſy, and requires very little time. Nothing is neceſſary but to pulverize the lime, and to dilute it in the ſerum of blood until it forms a liquid of ſufficient thickneſs for painting; which is then to be applied in the uſual manner. The colour which this compoſition acquires is whitiſh, and more or leſs ſhining, according to the purity of the ſerum and the whiteneſs of the lime. This earth may be employed either in the ſtate of quick or cauſtic lime reduced to powder, or well ſlaked with a ſmall quantity of water to leſſen the adheſion of its integrant parts. It muſt then be reduced to a fine powder, and be ſifted through a cloſe ſieve. If kept for ſome days, it is neceſſary to preſerve it in a cloſe tub or earthen veſſel, to prevent its combination with the carbonic acid and water of the atmoſphere. The ſerum is ſeparated from the curdled blood by decantation: but care muſt be taken to ſhake the matter as little as poſſible, and to ſuffer it each time to remain at reſt.

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In caſe of doubt, inſpection of the fluid alone may ſerve to determine its quality; for the ſigns of its fermentation are very evident. This obſervation is applicable alſo to the colouring liquor, but in a leſs degree. It follows from this circumſtance, that the veſſels which have been uſed for containing, preparing, or applying paint made with ſerum ought to be waſhed every day. The ſecond precaution to be taken is, that as the re-action of the ſerum and lime increaſes in the courſe of time, as well as the denſity of the mixture, in order that it may be preſerved in the neceſſary ſtate of fluidity, or to diminiſh or increaſe it according to the nature of the ſubſtances to be painted and the number of ſtrata to be applied, another veſſel filled with ſerum muſt be provided, to give to the colouring liquor the neceſſary fluidity. It thence follows that the mixture muſt be prepared as it is uſed.

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The well known re-action of animal ſubſtances and of lime on the metallic oxides, and particularly on thoſe in which oxygen is united to the metal with little force, made me ſuſpect an alteration of ſome colours when mixed with this liquid; an alteration naturally produced by the ſeparation of a part of the oxygen from the metallic oxides. On the other hand, the analogy which coloured chalks have with calcareous earth induced me to believe that their combination would be attended with the happieſt reſult. The oxides of lead, copper, and arſenic, indeed, undergo a ſingular alteration in their reſpective colours, when they are united with ſerum and calcareous earth: but this is not the caſe with cobalt green; which on that account appears to me to be very proper for being uſed as a colouring matter in this new kind of painting. Thoſe earths, however, called red, green, black, or yellow, which are of an argillo-calcareous nature, when mixed with theſe ſubſtances, may furniſh different agreeable colours of a durability equal to the blueiſh colour produced merely by the mixture of lime and of ſerum.

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The well known re-action of animal ſubſtances and of lime on the metallic oxides, and particularly on thoſe in which oxygen is united to the metal with little force, made me ſuſpect an alteration of ſome colours when mixed with this liquid; an alteration naturally produced by the ſeparation of a part of the oxygen from the metallic oxides. On the other hand, the analogy which coloured chalks have with calcareous earth induced me to believe that their combination would be attended with the happieſt reſult. The oxides of lead, copper, and arſenic, indeed, undergo a ſingular alteration in their reſpective colours, when they are united with ſerum and calcareous earth: but this is not the caſe with cobalt green; which on that account appears to me to be very proper for being uſed as a colouring matter in this new kind of painting. Thoſe earths, however, called red, green, black, or yellow, which are of an argillo-calcareous nature, when mixed with theſe ſubſtances, may furniſh different agreeable colours of a durability equal to the blueiſh colour produced merely by the mixture of lime and of ſerum.

I have however remarked, that the durability of this painting is conſiderably altered when there is mixed with it a very large quantity of colouring earth, ſuch as the black earth uſed for painting an iron colour, or the repreſentation of white marble with black ſpots. To remedy this inconvenience, nothing

thing will be neceſſary but to mix a little white of an egg with the ſerum before it is united with the lime or the black earth. I muſt here add, that wood painted in this manner may be poliſhed, and made to acquire a certain degree of luſtre. In this caſe I have obſerved that freſh ſpermaceti may be ſubſtituted with advantage for the tallow or oil in which the rags employed for rubbing wood before it is poliſhed are commonly dipped. This kind of paint may be applied alſo with the ſame ſucceſs to earthen ware, lead, iron, cut ſtone, &c.

The circumſtances which induced me to employ this new kind of painting, obliged me ſo make an extenſive uſe of it, without allowing me time to carry my operations any further. More than 300 arrobes* of ſerum were conſumed in the courſe of a few days. In this manner we were able to paint not only the paſſage and bridge of the king's palace, but alſo the windows and doors of the ſame edifice, thoſe of the magnificent building of the Exchange, the *Place de Taureau*, and many private houſes. The happy effects it produced, when expoſed to the eyes of the public, are a ſufficient proof that practice here perfectly correſponds with theory.

It is hardly neceſſary to obſerve, that the ſurfaces to be painted muſt be ſmooth; that the wood muſt be covered with a ſtratum of plaſter; and that this painting muſt not be applied over painting with oil or with turpentine, becauſe the heat would make it crack.

I propoſe to continue the ſmall number of trials I have already made, to render the uſe of my diſcovery more general. The following is the reſult of thoſe which I have hitherto been able to attempt.

In conſequence of the viſcidity, adheſion, and ſpeedy deſiccation of this compoſition, it may be employed in the ſame manner as bitumen, to cement with great ſolidity broken articles of every kind. In this caſe it oppoſes a ſtrong reſiſtance to the action of fire, water, many of the gaſes, and to vapour.

As ſerum contains a large quantity of albuminous matter, having myſelf ſeparated from it nearly half its weight, it may be employed with advantage for clarifying ſugar, wine, and other ſubſtances, in a more œconomical manner than by the white of an egg.

This new compoſition may be employed alſo for covering the ſurface of damp walls intended to be painted in oil colours; taking care, however, to cover it with a coating of plaſter and glue, to prevent its re-action on the colouring parts of the painting.

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* An arrobe weighs 25 or 26 pounds.

The hardneſs of this ſubſtance, and its ſuſceptibility of receiving a poliſh, induce me to believe that it might be employed on ſtucco, and for imitating it on walls and ceilings.

It may be uſed alſo for uniting water-conduits, and for cementing ſtones of all dimenſions, and eſpecially thoſe which are put into wood caiſſons for building under water.

It appears from different works, that the antients had conceived the idea of employing blood in the compoſition of their cements and mortar; but the uſe of it in painting was before unknown. Beſides, there was too great a diſtance between this uſe of blood and that of ſerum freed from the fibrous part and the colouring matter, to admit of any doubt that the principle of my diſcovery is entirely my own.

It follows from what has been here ſaid, that this new kind of painting may be added to thoſe already known, and that it may be called painting *in ſerum*; that it is as eaſily applied as prepared; that the elements of it are common, and of little value; and that it unites in it all the advantages of painting in oil and in diſtemper, without having any of their inconveniences.

XLIII. *On the Kind of Air-Furnaces employed in Iron Foundries for caſting large Pieces of Ordnance, Shafts for Mills, Cylinders, and other heavy Articles.* By Mr. DAVID MUSHET, *of the Calder Iron Works*[*].

THE furnaces about to be deſcribed are employed for melting pig-iron with the flame of pit-coal. Furnaces of this kind are conſtructed of various ſizes according to circumſtances. The ſmall ſizes will run down from ſeven to ten hundred weight, and are uſed in ſmall foundries for what the trade call *jobbing*.

Fig. 1. (Plate IV.) a ground plan of two large air-furnaces, and chimney for melting pig- or caſt-iron with the flame of pit-coal.

The letters ABCD point out the exterior dimenſions of the ſtalk or chimney, which is firſt erected, leaving two openings or arches into which the fore-part of the furnaces are afterwards built. The breadth of the chimney at the particular place which the plan exhibits is 16 feet from A to B, and from A to D or from B to C 6 feet 6 inches. The plan is drawn at that elevation where the flame enters the chimney by the flue or throat, narrowed on purpoſe to throw back part of the flame, and keep the furnace equally hot throughout, as may be more particularly viewed in the vertical ſection, fig. 2.

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[*] Communicated by the Author.

E E, the furnace bars on which the coals reſt, and where the combuſtion is maintained.

F F, openings called teaſing-holes, by which the coals are introduced to repair the fire.

G G, fire-brick buildings called bridges. Theſe are meant to concentrate the flame, that it may act as violently on the metal as poſſible. Upon the height of the bridge much depends in fuſing the metal ſpeedily, and with little loſs. The height of this may be ſeen in the vertical ſection, fig. 2. G.

H H, the charging doors, by which the metal is introduced in the ſhape and ſtate of pig-iron, lumps, ſcraps, &c. &c. The iron generally occupies the furnace acroſs to I, called the back wall, and is never meant to approach the bridge nearer than the dotted line, leſt the metal in melting ſhould run back into the grates, in place of deſcending into the general reſervoir or cavity below. The corners or notches, *b*, *b*, *b*, *b*, receive a ſtout caſt-iron frame lined with fire-bricks. This is hung by means of a chain and pulley, and can be raiſed and depreſſed at pleaſure. This frame is, properly ſpeaking, the charging door, and is always carefully made air-tight by means of moiſtened ſand.

K K, the flues or openings by which the flame enters the chimney. Theſe are 15 inches by 10. On maintaining theſe openings of a proportionate ſize to the other parts depend in a great meaſure the powers and œconomy of the furnace.

L L, lading doors, by which ladles are introduced, in the caſe of ſmall furnaces, to lift out the metal and diſtribute it to the various moulds.

M M M M, binding bolts to limit within proper bounds the expanſion which takes place in the building when the furnace is highly heated.

Fig. 2. vertical ſection of one of the furnaces, and its appropriate ſtalk or chimney.

E, the grates.

F, the teaſing-hole.

G, the bridge.

H, the charging door.

K, the flue or opening into the chimney.

L, the lading door.

M M, the binder or binding bolt.

N, the interior of the ſtalk or chimney, 30 inches ſquare.

O O, the fire brick-work, 9 inches thick.

P P, ſpace

PP, ſpace of 2 inches for ſtuffing with ſand.

QQ, common brick building.

RR, caſt-iron lintels, over which are thrown double 9-inch arches, ſo that at any time the inferior building can be taken down to make repairs, without ſhaking or in the leaſt injuring the chimney.

S. The dotted lines here are meant to repreſent what is called the tapping-hole. When a large piece of goods is to be caſt, lifting the metal with ladles would be impracticable. A ſharp-pointed bar is driven up this opening. The iron then flows freely out into a large baſon of ſand made for its reception. It is then conducted, by collateral channels, into the mould.

The ſpace under the curved dotted line from G to L, by S, is filled with a mixture of ſand and aſhes. When the furnace is prepared to melt, the whole of the bottom receives a ſtratum of ſharp clean ſand about two inches thick. This is broken up at night, and freſh ſand is ſubſtituted for it before the fire is kindled in the morning.

Fig. 3. is a horizontal ſection of the chimney or ſtalk, taken where the flues aſſume a perpendicular direction. The letters in this figure correſpond to thoſe in the vertical ſection, fig. 2. The height of the chimney ought not to be leſs than 45 feet: if 50 feet, the effect will be ſooner and of courſe better produced.

Remarks upon the Melting of Iron in theſe Furnaces.

The effect wiſhed to be produced in air furnaces is the fuſion of a certain portion of pig- or caſt-iron for the purpoſe of being poured or run into moulds to form articles of almoſt every deſcription.

The preparation previous to melting is as follows:—After the bottom of the furnace is laid, and ſmoothed with freſh ſand, and all the openings made air-tight, the furnace-man introduces a kindling at the teaſing-hole, accompanied with new pit-coal. In a few minutes a conſiderable volume of dark flame mixed with ſmoke is produced. The fire quickly gathers ſtrength; more coal is introduced; and the furnace now becomes filled with a yellow-coloured flame. By continuing this operation for an hour, or an hour and a quarter, the furnace and flame will have become completely white; the latter ſteady, and at times apparently without motion. The furnace-man now judges the bottom to have been ſufficiently hardened for receiving the pig-iron without any riſk of ſinking. The charging-door is now opened, and the pig-metal thrown carefully and regularly upon that part of the bottom formerly deſcribed as being appropriated for its reception. The door is again cloſed and made air-tight, and the operation of firing continued with unremitting care and attention.

The time of melting depends entirely upon the quantity of metal introduced. The furnaces deſcribed above are capable of melting from 50 to 60 hundred weight of metal each, and when there is a moderate circulation of air they will perform this work in 2½ or 3 hours. In half an hour after the metal is introduced it aſſumes a blackiſh red colour. It then begins to brighten with every additional fire, and in about one hour appears white, and begins to loſe ſhape, and reſemble a wreath of ſnow.

An eye accuſtomed to ſuch heats will now diſcern the metal beginning to drop, and run down the inclined plane in very beautiful ſtreamlets reſembling quickſilver. Eight or ten of theſe are viſible at a time, and after proceeding half way down begin to form junctions with each other, and flow connected into the general cavity or reſervoir. By-and-by this becomes filled, and literally forms a beautiful molten mirror, in which ſometimes part of the interior furnace is reflected.

The furnace-man, by ſearching at the bridge with his fire-iron or teaſer, judges when the metal is nearly all gone. Of this he is certain by looking up from the peep-hole of the lading-door. If the ſtreamlets of the running metal have ceaſed, then the whole is melted, and ready for running out.

In the operation of melting, the three following circumſtances ought to be particularly attended to: the thinneſs or hotneſs of the metal; the waſte or loſs ſuſtained in melting; and the quantity of coals employed.

The firſt is of the utmoſt importance, as many articles in the foundry buſineſs require the metal in a ſtate of the greateſt diviſion; otherwiſe they will be found imperfect when taken from the ſand, and unfit for ſale. The furnace-man, therefore, is always on the watch to replace the fire as it decays, and keep a large and ſharp volume of flame conſtantly paſſing over the metal.

The waſte or loſs of real metal is alſo an object of great importance. This always bears a relation to the quality of the iron, the ſtrength and cleanneſs of the coals, and the judgment and attention of the melter. Strong iron is found always more difficult to fuſe; this neceſſarily expoſes it for a long period in contact with the flame. The reverſe happens with metal that is more fragile, and eaſier broken in the pig. The length of the expoſure in fuſing depends on this; and other circumſtances being alike, the loſs or waſte of metal will alſo be in the ſame ratio.

There are, however, other facts not unworthy of notice. No. I. pig-iron, or richly carbonated metal, when run from an air-furnace, will be found in point of quality little better than No. II. or carbonated iron. This is owing to a quantity of its carbon being deſtroyed during the fuſion. The loſs in melting No. I. iron, therefore, chiefly conſiſts of carbon; and the deficiency of metal ought never, with a clean bottom, to exceed 1 cwt. in 20.

Carbonated or No. II. iron alſo becomes deprived of a conſiderable portion of its carbonaceous mixture in fuſion; and when run from the air-furnace is ſeldom better than No. III. metal. The loſs ſuſtained in melting may be averaged at 7½ per cent.

No. III. pig-iron is, after melting in an air-furnace, found whitiſh or mottled. It is ſeldom ſuſceptible of the ſame nice degree of diviſion as the ſuperior qualities, and loſes in fuſion a much larger proportion of metal, ſeldom under 10 per cent. and frequently 12½ or 15.

The quantity of coals requiſite to melt a given quantity of iron is various, as much depends upon the quality and fuſibility of the metal. If the furnace goes one heat a day with No. I. or II. iron, the quantity of coals will be from 20 to 25 cwt. for a ton of iron. If two or three heats a day, or as many tons of iron are melted at one kindling, the proportion of coals will be nearly weight for weight of the iron melted when the coals are mixed with a fair proportion of ſmall: with ſtrong large ſplint coals, one ton of good pig-iron may be completely reduced with from 12 to 15 cwt. including the previous heating of the furnace.

XLIV. *Obſervations on the Freezing of Water and the Nature of Snow: in a Letter from Profeſſor* DRIESSEN *to Dr.* VAN MARUM*.

AS the preſent ſeaſon has afforded us an opportunity to make experiments on the freezing of water and the nature of ſnow, I flatter myſelf that it will give you ſome ſatisfaction to be made acquainted with the reſults I have obtained in regard to this important ſubject; and I give you full liberty to publiſh them, if you think they are likely to be of any utility.

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* From *Algemeene Konſt en Letter-Bode,* No. 14, 1803.

At

tion, the ſpongy appearance which water acquires by freezing *in vacuo*, in conſequence of its greater expanſion, which every time that the glaſs burſt was unaccompanied with the appearance of air-bubbles, but immediately on the introduction of air they ſpread themſelves through the whole maſs. Hence there was no doubt that this increaſed dilatation was entirely the conſequence of the aëriform water, the expanſion of which was not impeded by the preſſure of the atmoſphere. We repeated this experiment for the laſt time on the ſame day, when the froſt was perceptibly leſs intenſe. A beer-glaſs capable of containing eight ounces was two-thirds filled with diſtilled water as much freed from air as poſſible: it was immediately placed under the receiver of an air-pump, and the air was exhauſted as ſpeedily as poſſible. At the end of ſome hours we ſaw a phænomenon which appeared ſo ſtrange and ſingular, that I was at firſt incapable of ſaying what it might be. The freezing water had raiſed itſelf up in a ſtraight column nearly three inches above the glaſs, being in thickneſs in the middle above a line, and at the bottom two lines. The water was equally frozen at the ſurface and throughout the whole maſs: the cellular or ſpongy appearance exiſted only under the column, and extended to the middle of the baſe. No air-bubbles were any where to be ſeen. It is probable that the ſlowneſs of the freezing may have contributed to produce this ſingular phænomenon.

We conjectured, and with great probability, that this column was hollow, and had ſerved for diſcharging the aëriform water. As ſoon as the air was admitted under the receiver we ſaw a movement in the pipe like that of air which penetrates into a wet glaſs tube, and in the mean time a great many air-bubbles appeared in the cavities at the bottom of the column. By introducing a ſtiff briſtle into the aperture, and employing a magnifying glaſs, we could eaſily trace the hollow part of the tube to the cavity where the air-bubbles were.

The air, therefore, on this occaſion had no ſhare in the expanſion of the ice; and as our water before freezing never contains ſo much air, it appears that during the freezing it could contribute little or nothing towards the expanſion.

Expanſion is a phænomenon common during all cryſtallization, and is a conſequence of the peculiar regular form which the moleculæ of bodies adhering mutually to each other aſſume, and by which means larger vacuities are formed. Expanſion is increaſed, and particularly during the cryſtallization of water, by the aëriform ſtate to which a part of it is brought by the diſengaged caloric.

ceptacle for a very large quantity of combined caloric, which by the decompoſition of the atmoſphere returns again, in a great meaſure, with the water, as ſenſible heat to the ſurface of the earth: in the phænomena of combuſtion, reſpiration, and other great operations in the œconomy of nature, I can ſee not only a decompoſition of vital air, but that alſo of aëriform water, which is ſo intimately connected with air. But as I dare not venture to proceed further in this path, for which I yet feel myſelf too weak, I ſhall return to the principal object of this paper.

I have repeated the experiments which I before communicated to you, as already obſerved, and with the ſame reſult: I entertained no doubt that the dilatation of freezing water, previouſly deprived of air, is to be aſcribed, beſides to cryſtallization, in particular to the aëriform water, which during the freezing being carried off with the liberated caloric, deranges in its paſſage and ſeparates the cryſtals from each other. I had entirely given up the idea of the exiſtence of air in chemical union with water, or with its baſes, which could not be ſeparated by boiling, and which by freezing might again be liberated in an elaſtic ſtate, till the treatiſe of my friend Dr. Uilkens, written in March 1800, came into my hands.

The well-known merit of this philoſopher, the modeſty with which he communicates his ideas to the public, made me conſider it as my duty to examine his objections with all due reſpect; but at the ſame time to prove to my pupils, if poſſible, the truth of my experiments.

According to my experiments, water can be perfectly freed from air. In ſuch water no air-bubbles ariſe during the proceſs of freezing, provided the air has no acceſs to it. Its communication with the air would inſtantly make them appear.

For the ſake of thoſe who may be deſirous to repeat my experiments, I ſhall deſcribe an infallible method by which the ſmalleſt traces of air in water may be detected. None of the means commonly uſed for the purpoſe are ſufficient when accuracy is required, as is the caſe in the preſent inſtance.

I pour the water deſtined for examination into an earthen veſſel, communicate a blue tint to it by a ſufficient quantity of ſolution of litmus, which I preſerve ready, of a certain ſtrength, for ſuch purpoſes: I then fill with it a flaſk in ſuch a manner, that the aperture of the flaſk is kept under water during the time of filling; taking care to ſhake the flaſk, that the ſmalleſt air-bubble may not remain in it. I then invert the flaſk in the water, and introduce into it, in the common manner, pure nitrous gas until 1-16th or 1-20th of it is filled. I then place the flaſk in a diſh, and intercept its communication with the atmoſphere by means of mercury. Now, if there be any air in the water, its oxygen gas muſt unite with the nitrous gas, producing more or leſs acid according to the quantity of the oxygen gas. The ſolution of litmus, therefore, muſt acquire a red colour, and that, *cæteris paribus*, more or leſs, and in a ſhorter or longer time, according to the quantity of oxygen preſent in the water. When you intend to examine different kinds of water at the ſame time, after it has ſtood two days introduce into it carefully, in ſmall quantities, by means of a ſyringe, an alkaline ſolution; and from the quantity of the ſolution neceſſary to reſtore the blue colour to the ſolution of litmus, which has been turned red, you can eſtimate very accurately the *relative* quantity of oxygen gas, and conſequently that of the common air mixed with the water.

A proof of this kind was neceſſary in order to determine with certainty whether water could be entirely freed from oxygen gas, and conſequently from common air; for azotic gas is never preſent in water alone.

Diſtilled water, however carefully prepared, and immediately received as it comes off in flaſks cloſely ſtopped, cannot bear the proof by nitrous gas. Water freed from air by means of the air-pump again imbibes it as ſoon as the air has free acceſs to it: even if the water be boiled in the flaſk, when expoſed for a few moments to the atmoſphere it becomes, in ſome meaſure, reddiſh by this proof.

After a great many fruitleſs attempts, I was never able to free water entirely from air but by the following proceſs:

I boil pure water for at leaſt two hours in a large baſon; colour it with litmus; warm a large ſtoppered flaſk in it, and, holding it by means of a pair of forceps, fill it entirely with the boiling water. I let the flaſk remain for at leaſt half an hour with its aperture ſome inches below the ſurface of the boiling water, frequently ſtriking its ſides with the forceps to expel any air which may ſtill remain in it. I then invert the flaſk, place it in an earthen veſſel, and immediately introduce nitrous gas: I then intercept all communication with the external air by mercury, of which there muſt be a ſufficient quantity, on account of the diminution of the volume of the water, and particularly of the nitrous gas, by cooling; in conſequence of which the mercury riſes into the flaſk to a certain height. In this manner, I had the ſatisfaction to ſee that the liquor retains its blue colour for weeks; a certain proof that water can be entirely freed from air.

I was now enabled to proceed a ſtep further, and to determine whether after this proceſs any of the *baſes* of air, and, in particular, free oxygen, remained in the water, which during freezing might be extricated in the ſtate of elaſtic air.

On one of the coldeſt evenings of the month of December 1801, I filled ſome flaſks, in the manner already mentioned, with blue-coloured water freed from its air and with nitrous gas, and others with water without any addition; all of which I placed in the window. Next morning no change was to be obſerved, though the froſt had been very ſtrong: the water in all the flaſks was in a ſtate of perfect fluidity. This phænomenon, I acknowledge, is very common: but I mention it becauſe ſome conſider the impeded diſengagement of air as the cauſe of it; an idea which I was not ignorant of, and which agrees with the reſult of this experiment.

This circumſtance rendered the experiment more agreeable; and being certain that every thing would freeze on the leaſt agitation, I was enabled to obſerve better the ſmalleſt change which would take place during the proceſs of freezing.

What I expected took place; none of thoſe preſent during the freezing could obſerve the leaſt change of colour. We every where ſaw bubbles appear, and diſperſe themſelves widely through the lump of ice; but they were merely nitrous gas. At the end of two days we did not perceive the leaſt trace of redneſs in the ice, till the flaſk at length burſting, and the air thus having free acceſs, the whole maſs inſtantly became red. The flaſks which contained no nitrous gas, or tincture of litmus, exhibited during the freezing of the water no air-bubbles; but when the flaſks burſt in conſequence of the froſt increaſing, the maſs of ice was immediately interſperſed here and there with air-bubbles.

But though theſe experiments fully proved my idea in regard to the origin of air-bubbles in ice, I was very deſirous to obſerve the phænomena of freezing *in vacuo.*

My former pupil and worthy friend Van In En Kniphuiſen, lord of Nienoord, who has added ſo much luſtre to his noble birth by an aſſiduous cultivation of the ſciences, was ſo kind as to aſſiſt me in theſe experiments, and to allow me the uſe of his valuable apparatus.

The experiment of Lichtenberg falls to the ground. We have ſeen curious phænomena. I ſhall give you an account of them, which I find in my memorandums, dated January 16, 1802. This will be more than ſufficient to induce all the amateurs of the ſtudy of nature to repeat my experiments.

We had already obſerved ſeveral times, with great ſatisfaction,

tion, the ſpongy appearance which water acquires by freezing *in vacuo*, in conſequence of its greater expanſion, which every time that the glaſs burſt was unaccompanied with the appearance of air-bubbles, but immediately on the introduction of air they ſpread themſelves through the whole maſs. Hence there was no doubt that this increaſed dilatation was entirely the conſequence of the aëriform water, the expanſion of which was not impeded by the preſſure of the atmoſphere. We repeated this experiment for the laſt time on the ſame day, when the froſt was perceptibly leſs intenſe. A beer-glaſs capable of containing eight ounces was two-thirds filled with diſtilled water as much freed from air as poſſible: it was immediately placed under the receiver of an air-pump, and the air was exhauſted as ſpeedily a ſpoſſible. At the end of ſome hours we ſaw a phænomenon which appeared ſo ſtrange and ſingular, that I was at firſt incapable of ſaying what it might be. The freezing water had raiſed itſelf up in a ſtraight column nearly three inches above the glaſs, being in thickneſs in the middle above a line, and at the bottom two lines. The water was equally frozen at the ſurface and throughout the whole maſs: the cellular or ſpongy appearance exiſted only under the column, and extended to the middle of the baſe. No air-bubbles were any where to be ſeen. It is probable that the ſlowneſs of the freezing may have contributed to produce this ſingular phænomenon.

We conjectured, and with great probability, that this column was hollow, and had ſerved for diſcharging the aëriform water. As ſoon as the air was admitted under the receiver we ſaw a movement in the pipe like that of air which penetrates into a wet glaſs tube, and in the mean time a great many air-bubbles appeared in the cavities at the bottom of the column. By introducing a ſtiff briſtle into the aperture, and employing a magnifying glaſs, we could eaſily trace the hollow part of the tube to the cavity where the air-bubbles were.

The air, therefore, on this occaſion had no ſhare in the expanſion of the ice; and as our water before freezing never contains ſo much air, it appears that during the freezing it could contribute little or nothing towards the expanſion.

Expanſion is a phænomenon common during all cryſtallization, and is a conſequence of the peculiar regular form which the moleculæ of bodies adhering mutually to each other aſſume, and by which means larger vacuities are formed. Expanſion is increaſed, and particularly during the cryſtallization of water, by the aëriform ſtate to which a part of it is brought by the diſengaged caloric.

While a part of this caloric, diſengaged during the proceſs of freezing, keeps the water beneath the cruſt of ice in a ſtate of warmth and fluidity, another part, in conſequence of the aëriform expanſion of the water, forms the cavities in the ice by which the ſo neceſſary communication between the external air and the fluid water is maintained, in order to ſupport the life of thoſe beings which reſide in it.

And what would be the caſe with vegetation if ice were a body impermeable to air? While the warmth of the earth is maintained as far as poſſible under its hard ſurface by the power of congelation, the air continually penetrates through it; by which means the principles of germination are preſerved in the ſeeds, and prepared for development.

My experiments have alſo ſhown that ice water produced by a ſlow thaw contains more air than water which has not been frozen during the ſame time.

But ſnow water in particular contains much more air than common rain water; and this air contains more oxygen than the air obtained from rain water.

The opinion of Haſſenfratz, which may be ſeen in the fourth part of the *Journal Polytechnique* *, is fully confirmed, whatever Dr. Carradori may aſſert to the contrary †. Snow water may, with great propriety, be called *oxygenated water*. This I can incontrovertibly prove, not only in the uſual manner by ſulphate of iron, but in particular by means of nitrous gas; and have ſhown it more than twenty times in my lectures, as a thing of importance not only to philoſophy but alſo to pharmacy. What I aſſert may be eaſily proved in the following manner:

Fill two flaſks, one with ſnow and the other with rain water, and place them, inverted, in boiling water: the air will then evidently be ſeen to collect itſelf at the bottom (now the top). Yeſterday my amanuenſis, H. Uilkens, exhibited to us this important phænomenon. A flaſk filled with rain water, which at that time conſiſted chiefly of melted ſnow, did not preſent one-half of the air-bubbles exhibited by ſnow water collected in the garden.

Beſides this larger quantity of air, and particularly oxygen gas, ſnow water contains alſo much fewer extractive particles than rain water; and from theſe two circumſtances we can explain the properties by which this water diſtinguiſhes itſelf from rain water, as a medicine, as drink, and in many œconomical uſes.

It is needleſs to inquire, why this water is ſometimes hurtful

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* See Phil. Mag. vol. iii. p. 233.

† Ibid. vol. iv. p. 217.

ful in caſes of inflammation of the eyes? why it occaſions colic, griping pains, and other affections, when drunk cold? But, without enlarging further on this ſubject, I ſhall here mention a remarkable effect of the wiſe diſpenſations of Nature. As ſnow water contains oxygen united with little caloric, it thereby poſſeſſes a ſtronger tendency to communicate its oxygen to bodies ſuſceptible of oxygenation. No ſubſtance in nature deprives water of its oxygen with more avidity than fertile earth. Snow water mixed with vegetable mould, and expoſed to the ſolar light, improves the mould in a ſhort time. Almoſt as ſoon as a lively fiſh placed in a glaſs of water containing oxygen makes the ſuperfluous oxygen diſappear, and fills the water with carbonic acid gas in its room, is the oxygen taken up by the earth aſſiſted by the influence of the light. Pure earths do not exhauſt ſnow water of its oxygen; nor do they attract the oxygen of the atmoſphere, as aſſerted by Von Humboldt.

I have long been convinced of the contrary, from various experiments. It appears, in particular, that carbon is the principle whoſe ſtrong affinity for oxygen produces ſo many important phænomena, and which nature continually employs in the compoſition and decompoſition of organic bodies: it is carbon alſo which in this caſe deprives water of its oxygen, and conſequently frees it from what renders it prejudicial to health, and unfit for the purpoſes of life.

That which is prejudicial to us is improved by the ground, and at the ſame time gives power and activity to the mould.

When the ſnow covers the earth, it tends to keep it warm: the ſnow, as a body which cannot conduct caloric in a very ſmall degree, prevents, by its interpoſition, the cold air from taking the caloric from the earth. But this does not appear to be the only cauſe of its fertilizing power. The ſnow melting and penetrating into the ſoftened earth communicates to it oxygen, promotes by theſe means the germination of ſeeds: the young plant grows with more vigour, becauſe the carbon of the fertile earth combining with the oxygen is converted into carbonic acid, and thereby acquires more ſolubility; while the water, by its ſtimulating property, contributes to excite that activity which had been rendered dormant in the roots by the cold.

This fertilizing power of ſnow, which was before aſcribed to nitrous particles, but the preſence of which was never proved, ſeems thus, according to the idea of Ingenhouſz, Haſſenfratz, and other naturaliſts, to be explained in a more ſatisfactory manner.

I ſhall now conclude this ſhort eſſay by expreſſing a hope that I have here given philoſophers a further inducement to admire with me the great and noble operations of the divine providence, which ſo evidently appear in the different changes effected in air and water, and which are ſo neceſſary for ſupporting and maintaining the creation.

Groningen,
February 26, 1803.

XLV. *Obſervations on the Lotus of Egypt. By* ALIRE RAFFENAU-DELILE, *Member of the Inſtitute of Egypt**.

SINCE my return from Egypt I preſented to the Muſeum of Natural Hiſtory different kinds of ſeeds and ſeveral bulbs of the *nymphæa cærulea*, of which an accurate deſcription was given in Egypt by C. Savigny, my colleague. Theſe bulbs began to germinate towards the end of Meſſidor, when kept immerſed in water. I collected them in the iſland of Roſetta in the month of Ventoſe, year 8; and though I had kept them two years without planting them, they had not become deſiccated: one of the plants of the nymphæa has already produced ſeveral flowers ſomewhat ſmaller, leſs odorous, and of a paler blue colour, than thoſe in Egypt.

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We obſerved the nymphæa cærulea painted and engraven on the Egyptian monuments much oftener than the two kinds of lotus mentioned by the oldeſt hiſtorians. This nymphæa has a great reſemblance to theſe kinds of lotus, and appears to be a third ſpecies of it.

Moſt authors who have undertaken to make known the lotus in regard to its antiquity have fallen into many errors; but Mahudel, of the Academy of Inſcriptions and Belles Lettres,

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* From *Annales du Muſeum National d'Hiſtoire Naturelle*, No. 5.

At preſent, however, I ſhall give you only a ſhort account of my experiments, becauſe I hope, in the courſe of a few years, to collect materials for a more elaborate work on the freezing of water, and the ſtate of the atmoſphere in winter; having been induced to turn my attention to the latter object by the obſervations I had made, and which I mentioned to you three years ago, on the force with which a conſiderable quantity of vapour, eſpecially during the time of ſtrong froſt, is diſengaged in an aëriform ſtate from freezing water, and riſes, as it were, with violence into the atmoſphere; and on the increaſe of volume which the air thereby acquires even in cloſe veſſels, and which it retains during the ſtrongeſt cold.

When I traverſe the ſnow-clad fields during the coldeſt days of winter; when I conſider the dry ſtate of the air, and of every thing around me, ariſing, no doubt, from a ſtrong and continued tendency of the aqueous particles, of which I neither ſee nor feel any thing, to riſe into the higher regions of the atmoſphere; and when in conſequence of the ſerenity of the heavens I can diſtinguiſh very ſmall objects at a great diſtance, I cannot help admiring the power of nature, as yet ſo little known, by which air ſo cold and ſo much condenſed can be ſo intimately connected with water. This water, in my opinion, can be conſidered only as in an aëriform ſtate, as ſuch combined with the atmoſphere by a very ſtrong affinity, and forming with it one and the ſame body: I muſt obſerve alſo, that this affinity ſeems to increaſe in the ratio of the cold.

The atmoſphere being an elaſtic fluid in which caloric is combined by the ſtrongeſt affinity with its baſes, it by theſe means appears to be more capable of preſerving by the power of this union, in the ſtate of atmoſpheric vapour, the aëriform water which has ſo weak an affinity for its ſpecific caloric. The air thus condenſed, and containing more aërial particles in the ſame ſpace than in ſummer, attracts, on this account, the water with much greater force. The water becoming aëriform during the proceſs of freezing, and in that ſtate entering into combination with the air, has no need of external heat to maintain it in that ſtate of union. The aëriform ſtate of water, which in ſummer is maintained by the heat of the ſurrounding bodies, is in this ſeaſon preſerved by a ſtate of greater affinity.

Is the water then, during this cold, ſerene, wintery atmoſphere, in a more perfect ſtate of union with the air? I anſwer in the affirmative; and therefore I can readily conceive why the mercury ſtands at ſuch heights in the barometer during that ſeaſon. This air alſo may be conſidered as a receptacle for a very large quantity of combined caloric, which by the decompoſition of the atmoſphere returns again, in a great meaſure, with the water, as ſenſible heat to the ſurface of the earth: in the phænomena of combuſtion, reſpiration, and other great operations in the œconomy of nature, I can ſee not only a decompoſition of vital air, but that alſo of aëriform water, which is ſo intimately connected with air. But as I dare not venture to proceed further in this path, for which I yet feel myſelf too weak, I ſhall return to the principal object of this paper.

I have repeated the experiments which I before communicated to you, as already obſerved, and with the ſame reſult: I entertained no doubt that the dilatation of freezing water, previouſly deprived of air, is to be aſcribed, beſides to cryſtallization, in particular to the aëriform water, which during the freezing being carried off with the liberated caloric, deranges in its paſſage and ſeparates the cryſtals from each other. I had entirely given up the idea of the exiſtence of air in chemical union with water, or with its baſes, which could not be ſeparated by boiling, and which by freezing might again be liberated in an elaſtic ſtate, till the treatiſe of my friend Dr. Uilkens, written in March 1800, came into my hands.

The well-known merit of this philoſopher, the modeſty with which he communicates his ideas to the public, made me conſider it as my duty to examine his objections with all due reſpect; but at the ſame time to prove to my pupils, if poſſible, the truth of my experiments.

According to my experiments, water can be perfectly freed from air. In ſuch water no air-bubbles ariſe during the proceſs of freezing, provided the air has no acceſs to it. Its communication with the air would inſtantly make them appear.

For the ſake of thoſe who may be deſirous to repeat my experiments, I ſhall deſcribe an infallible method by which the ſmalleſt traces of air in water may be detected. None of the means commonly uſed for the purpoſe are ſufficient when accuracy is required, as is the caſe in the preſent inſtance.

I pour the water deſtined for examination into an earthen veſſel, communicate a blue tint to it by a ſufficient quantity of ſolution of litmus, which I preſerve ready, of a certain ſtrength, for ſuch purpoſes: I then fill with it a flaſk in ſuch a manner, that the aperture of the flaſk is kept under water during the time of filling; taking care to ſhake the flaſk, that the ſmalleſt air-bubble may not remain in it. I then invert the flaſk in the water, and introduce into it, in the common manner, pure nitrous gas until 1-16th or 1-20th of it is filled. I then place the flaſk in a diſh, and intercept its communication with the atmoſphere by means of mercury. Now, if there be any air in the water, its oxygen gas muſt unite with the nitrous gas, producing more or leſs acid according to the quantity of the oxygen gas. The ſolution of litmus, therefore, muſt acquire a red colour, and that, *cæteris paribus*, more or leſs, and in a ſhorter or longer time, according to the quantity of oxygen preſent in the water. When you intend to examine different kinds of water at the ſame time, after it has ſtood two days introduce into it carefully, in ſmall quantities, by means of a ſyringe, an alkaline ſolution; and from the quantity of the ſolution neceſſary to reſtore the blue colour to the ſolution of litmus, which has been turned red, you can eſtimate very accurately the *relative* quantity of oxygen gas, and conſequently that of the common air mixed with the water.

A proof of this kind was neceſſary in order to determine with certainty whether water could be entirely freed from oxygen gas, and conſequently from common air; for azotic gas is never preſent in water alone.

Diſtilled water, however carefully prepared, and immediately received as it comes off in flaſks cloſely ſtopped, cannot bear the proof by nitrous gas. Water freed from air by means of the air-pump again imbibes it as ſoon as the air has free acceſs to it: even if the water be boiled in the flaſk, when expoſed for a few moments to the atmoſphere it becomes, in ſome meaſure, reddiſh by this proof.

After a great many fruitleſs attempts, I was never able to free water entirely from air but by the following proceſs:

I boil pure water for at leaſt two hours in a large baſon; colour it with litmus; warm a large ſtoppered flaſk in it, and, holding it by means of a pair of forceps, fill it entirely with the boiling water. I let the flaſk remain for at leaſt half an hour with its aperture ſome inches below the ſurface of the boiling water, frequently ſtriking its ſides with the forceps to expel any air which may ſtill remain in it. I then invert the flaſk, place it in an earthen veſſel, and immediately introduce nitrous gas: I then intercept all communication with the external air by mercury, of which there muſt be a ſufficient quantity, on account of the diminution of the volume of the water, and particularly of the nitrous gas, by cooling; in conſequence of which the mercury riſes into the flaſk to a certain height. In this manner, I had the ſatisfaction to ſee that the liquor retains its blue colour for weeks; a certain proof that water can be entirely freed from air.

I was now enabled to proceed a ſtep further, and to determine whether after this proceſs any of the *baſes* of air, and, in particular, free oxygen, remained in the water, which during freezing might be extricated in the ſtate of elaſtic air.

On one of the coldeſt evenings of the month of December 1801, I filled ſome flaſks, in the manner already mentioned, with blue-coloured water freed from its air and with nitrous gas, and others with water without any addition; all of which I placed in the window. Next morning no change was to be obſerved, though the froſt had been very ſtrong: the water in all the flaſks was in a ſtate of perfect fluidity. This phænomenon, I acknowledge, is very common: but I mention it becauſe ſome conſider the impeded diſengagement of air as the cauſe of it; an idea which I was not ignorant of, and which agrees with the reſult of this experiment.

This circumſtance rendered the experiment more agreeable; and being certain that every thing would freeze on the leaſt agitation, I was enabled to obſerve better the ſmalleſt change which would take place during the proceſs of freezing.

What I expected took place; none of thoſe preſent during the freezing could obſerve the leaſt change of colour. We every where ſaw bubbles appear, and diſperſe themſelves widely through the lump of ice; but they were merely nitrous gas. At the end of two days we did not perceive the leaſt trace of redneſs in the ice, till the flaſk at length burſting, and the air thus having free acceſs, the whole maſs inſtantly became red. The flaſks which contained no nitrous gas, or tincture of litmus, exhibited during the freezing of the water no air-bubbles; but when the flaſks burſt in conſequence of the froſt increaſing, the maſs of ice was immediately interſperſed here and there with air-bubbles.

But though theſe experiments fully proved my idea in regard to the origin of air-bubbles in ice, I was very deſirous to obſerve the phænomena of freezing *in vacuo*.

My former pupil and worthy friend Van In En Kniphuiſen, lord of Nienoord, who has added ſo much luſtre to his noble birth by an aſſiduous cultivation of the ſciences, was ſo kind as to aſſiſt me in theſe experiments, and to allow me the uſe of his valuable apparatus.

The experiment of Lichtenberg falls to the ground. We have ſeen curious phænomena. I ſhall give you an account of them, which I find in my memorandums, dated January 16, 1802. This will be more than ſufficient to induce all the amateurs of the ſtudy of nature to repeat my experiments.

We had already obſerved ſeveral times, with great ſatisfaction,

tion, the ſpongy appearance which water acquires by freezing *in vacuo*, in conſequence of its greater expanſion, which every time that the glaſs burſt was unaccompanied with the appearance of air-bubbles, but immediately on the introduction of air they ſpread themſelves through the whole maſs. Hence there was no doubt that this increaſed dilatation was entirely the conſequence of the aëriform water, the expanſion of which was not impeded by the preſſure of the atmoſphere. We repeated this experiment for the laſt time on the ſame day, when the froſt was perceptibly leſs intenſe. A beer-glaſs capable of containing eight ounces was two-thirds filled with diſtilled water as much freed from air as poſſible: it was immediately placed under the receiver of an air-pump, and the air was exhauſted as ſpeedily as poſſible. At the end of ſome hours we ſaw a phænomenon which appeared ſo ſtrange and ſingular, that I was at firſt incapable of ſaying what it might be. The freezing water had raiſed itſelf up in a ſtraight column nearly three inches above the glaſs, being in thickneſs in the middle above a line, and at the bottom two lines. The water was equally frozen at the ſurface and throughout the whole maſs: the cellular or ſpongy appearance exiſted only under the column, and extended to the middle of the baſe. No air-bubbles were any where to be ſeen. It is probable that the ſlowneſs of the freezing may have contributed to produce this ſingular phænomenon.

We conjectured, and with great probability, that this column was hollow, and had ſerved for diſcharging the aëriform water. As ſoon as the air was admitted under the receiver we ſaw a movement in the pipe like that of air which penetrates into a wet glaſs tube, and in the mean time a great many air-bubbles appeared in the cavities at the bottom of the column. By introducing a ſtiff briſtle into the aperture, and employing a magnifying glaſs, we could eaſily trace the hollow part of the tube to the cavity where the air-bubbles were.

The air, therefore, on this occaſion had no ſhare in the expanſion of the ice; and as our water before freezing never contains ſo much air, it appears that during the freezing it could contribute little or nothing towards the expanſion.

Expanſion is a phænomenon common during all cryſtallization, and is a conſequence of the peculiar regular form which the moleculæ of bodies adhering mutually to each other aſſume, and by which means larger vacuities are formed. Expanſion is increaſed, and particularly during the cryſtallization of water, by the aëriform ſtate to which a part of it is brought by the diſengaged caloric.

While a part of this caloric, diſengaged during the proceſs of freezing, keeps the water beneath the cruſt of ice in a ſtate of warmth and fluidity, another part, in conſequence of the aëriform expanſion of the water, forms the cavities in the ice by which the ſo neceſſary communication between the external air and the fluid water is maintained, in order to ſupport the life of thoſe beings which reſide in it.

And what would be the caſe with vegetation if ice were a body impermeable to air? While the warmth of the earth is maintained as far as poſſible under its hard ſurface by the power of congelation, the air continually penetrates through it; by which means the principles of germination are preſerved in the ſeeds, and prepared for development.

My experiments have alſo ſhown that ice water produced by a ſlow thaw contains more air than water which has not been frozen during the ſame time.

But ſnow water in particular contains much more air than common rain water; and this air contains more oxygen than the air obtained from rain water.

The opinion of Haſſenfratz, which may be ſeen in the fourth part of the *Journal Polytechnique* *, is fully confirmed, whatever Dr. Carradori may aſſert to the contrary †. Snow water may, with great propriety, be called *oxygenated water.* This I can incontrovertibly prove, not only in the uſual manner by ſulphate of iron, but in particular by means of nitrous gas; and have ſhown it more than twenty times in my lectures, as a thing of importance not only to philoſophy but alſo to pharmacy. What I aſſert may be eaſily proved in the following manner:

Fill two flaſks, one with ſnow and the other with rain water, and place them, inverted, in boiling water: the air will then evidently be ſeen to collect itſelf at the bottom (now the top). Yeſterday my amanuenſis, H. Uilkens, exhibited to us this important phænomenon. A flaſk filled with rain water, which at that time conſiſted chiefly of melted ſnow, did not preſent one-half of the air-bubbles exhibited by ſnow water collected in the garden.

Beſides this larger quantity of air, and particularly oxygen gas, ſnow water contains alſo much fewer extractive particles than rain water; and from theſe two circumſtances we can explain the properties by which this water diſtinguiſhes itſelf from rain water, as a medicine, as drink, and in many œconomical uſes.

It is needleſs to inquire, why this water is ſometimes hurtful in caſes of inflammation of the eyes? why it occaſions colic, griping pains, and other affections, when drunk cold? But, without enlarging further on this ſubject, I ſhall here mention a remarkable effect of the wiſe diſpenſations of Nature. As ſnow water contains oxygen united with little caloric, it thereby poſſeſſes a ſtronger tendency to communicate its oxygen to bodies ſuſceptible of oxygenation. No ſubſtance in nature deprives water of its oxygen with more avidity than fertile earth. Snow water mixed with vegetable mould, and expoſed to the ſolar light, improves the mould in a ſhort time. Almoſt as ſoon as a lively fiſh placed in a glaſs of water containing oxygen makes the ſuperfluous oxygen diſappear, and fills the water with carbonic acid gas in its room, is the oxygen taken up by the earth aſſiſted by the influence of the light. Pure earths do not exhauſt ſnow water of its oxygen; nor do they attract the oxygen of the atmoſphere, as aſſerted by Von Humboldt.

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* See Phil. Mag. vol. iii. p. 233. † Ibid. vol. iv. p. 217.

I have long been convinced of the contrary, from various experiments. It appears, in particular, that carbon is the principle whoſe ſtrong affinity for oxygen produces ſo many important phænomena, and which nature continually employs in the compoſition and decompoſition of organic bodies: it is carbon alſo which in this caſe deprives water of its oxygen, and conſequently frees it from what renders it prejudicial to health, and unfit for the purpoſes of life.

That which is prejudicial to us is improved by the ground, and at the ſame time gives power and activity to the mould.

When the ſnow covers the earth, it tends to keep it warm: the ſnow, as a body which cannot conduct caloric in a very ſmall degree, prevents, by its interpoſition, the cold air from taking the caloric from the earth. But this does not appear to be the only cauſe of its fertilizing power. The ſnow melting and penetrating into the ſoftened earth communicates to it oxygen, promotes by theſe means the germination of ſeeds: the young plant grows with more vigour, becauſe the carbon of the fertile earth combining with the oxygen is converted into carbonic acid, and thereby acquires more ſolubility; while the water, by its ſtimulating property, contributes to excite that activity which had been rendered dormant in the roots by the cold.

This fertilizing power of ſnow, which was before aſcribed to nitrous particles, but the preſence of which was never proved, ſeems thus, according to the idea of Ingenhouſz, Haſſenfratz, and other naturaliſts, to be explained in a more ſatisfactory manner.

I ſhall now conclude this ſhort eſſay by expreſſing a hope that I have here given philoſophers a further inducement to admire with me the great and noble operations of the divine providence, which ſo evidently appear in the different changes effected in air and water, and which are ſo neceſſary for ſupporting and maintaining the creation.

Groningen,
February 26, 1803.

XLV. *Obſervations on the Lotus of Egypt.* *By* ALIRE RAFFENAU-DELILE, *Member of the Inſtitute of Egypt*[*].

SINCE my return from Egypt I preſented to the Muſeum of Natural Hiſtory different kinds of ſeeds and ſeveral bulbs of the *nymphæa cærulea*, of which an accurate deſcription was given in Egypt by C. Savigny, my colleague. Theſe bulbs began to germinate towards the end of Meſſidor, when kept immerſed in water. I collected them in the iſland of Roſetta in the month of Ventoſe, year 8; and though I had kept them two years without planting them, they had not become deſiccated: one of the plants of the nymphæa has already produced ſeveral flowers ſomewhat ſmaller, leſs odorous, and of a paler blue colour, than thoſe in Egypt.

Though I made a tour with the Commiſſion of Arts into Upper Egypt during the time of the inundation, which is that alſo of the increaſe of the two kinds of nymphæa of the Nile, we met with none of theſe plants in that country.

The waters of the Nile riſe to a conſiderable height in the Said, and much leſs in Lower Egypt. The nymphæa, therefore, eaſily reaches the ſurface of the water, during the inundation, in the lakes and the canals of the Delta, where they are not hurt by the current of the river. It is, perhaps, for a contrary reaſon that the nymphæa does not grow in the Said.

We obſerved the nymphæa cærulea painted and engraven on the Egyptian monuments much oftener than the two kinds of lotus mentioned by the oldeſt hiſtorians. This nymphæa has a great reſemblance to theſe kinds of lotus, and appears to be a third ſpecies of it.

Moſt authors who have undertaken to make known the lotus in regard to its antiquity have fallen into many errors; but Mahudel, of the Academy of Inſcriptions and Belles Lettres,

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[*] From *Annales du Muſeum National d'Hiſtoire Naturelle*, No. 5.

Lettres[*], and lately profeſſor Sprengel, of Halle[†], have given very accurate information reſpecting theſe plants.

The expedition to Egypt afforded me an opportunity of making the following obſervations on the ſame ſubject: they are naturally connected with thoſe already made in Egypt by C. Savigny on the nymphæa cærulea, and complete the hiſtory of that plant.

Herodotus[‡] relates that the Egyptians fed on the lotus which grew in the waters of the Nile. He calls this lotus a lily, and ſays that the ſeeds, ſimilar to thoſe of the poppy, were employed for making bread. He adds, that they ate alſo the roots of the lotus, which were round, of the ſize of an apple, and of a ſweet and agreeable ſavour. Herodotus then ſpeaks of another kind of lily reſembling a roſe, which grew alſo in the Nile, the fruit of which had the form of a waſp's neſt, and which contained ſeveral ſeeds good to eat, and of the ſize of an olive.

Theophraſtus[§] gives a very accurate deſcription of the ſame plants:—"The lotus," ſays he, "grows in the fields when they are inundated. Its flowers are white, and have petals like thoſe of the lily. They ſpring up in great numbers cloſe to each other. They ſhut their leaves at ſun-ſet, and conceal their fruit. Theſe flowers open again when the ſun reappears, and riſe above the water; and this alternation is repeated till the fruit is completely formed, and the flowers have dropped off. The fruit is equal to that of a large poppy, and contains a great number of ſeeds ſimilar to grains of millet. The Egyptians depoſit the fruit in heaps, and ſuffer the rind to putrefy; they then ſeparate the ſeeds by waſhing them in the Nile, dry them, and knead them into bread. The root of the lotus called *corſion* is round, and of the ſize of a quince. Its rind is black, and like that of the cheſtnut. This root is white in the inſide, and is eaten either raw or boiled."

The lily of the Nile, like the roſes mentioned by Herodotus, is called by Athenæus[||] Egyptian bean, and the roſe lotus. Theophraſtus gives it the name of *bean*.

"This bean grows in the marſhes and ponds. Its ſtem is four cubits in length, and as thick as the finger. It reſembles a reed without knots. The fruit it bears is ſhaped like a waſp's neſt, and contains about thirty beans a little prominent, each depoſited in a different cell. The flower is twice as large as that of the poppy, and entirely a roſe. The leaves are ſupported by pedicles ſimilar to thoſe of fruits: they are large, and have a reſemblance to a Theſſalian cap. When one of the beans is bruiſed there is ſeen in the inſide of it a ſmall body folded together, from which the leaf ariſes. The root is thicker than that of a ſtrong reed, and has partitions like the ſtem. It ſerves as nouriſhment to thoſe who reſide near the marſhes. This plant grows ſpontaneouſly, and in abundance. It is ſown alſo in the mud, by forming for it a bed of ſtraw, in order that it may not rot."

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[*] Hiſtoire de l'Academie des Inſcriptions, vol. ii. p. 285.

[†] Antiquitatum botanicarum Specimen primum, p. 47.

[‡] In Euterpe, cap. xcii.

[§] Theophraſti Hiſt. Plant. lib. iv. cap. 10.

[||] Athenæi Deipnoſoph. lib. iii. p. 72; lib. xv. p. 677.

The lotus of Herodotus and Theophraſtus grows alſo in Egypt. It is the *nymphæa lotus* of Linnæus[*], the characters of which, compared by C. Savigny to thoſe of the *nymphæa cœrulea*, agree with the deſcriptions given by the antients.

In regard to the roſe lily or Egyptian bean, which is frequently carved out among the ornaments and ſymbolical repreſentations of the Egyptian temples, it no longer grows in that country; and would have remained unknown to naturaliſts had it not been diſcovered in India. This plant has been called by Linnæus[†] the *nymphæa nelumbo*. Its fruit, which the Greeks compare to a waſp's neſt, has a perfect reſemblance to it. They called it *ciborion*[‡] on account, no doubt, of its reſemblance to a cup. This fruit is ſhaped like the ſpout of a watering-pot, and is flatted at the upper part, which contains from fifteen to thirty ſmall foſſæ, containing a like number of ſeeds of the ſize of a nut, and ſomewhat prominent. The plumula[§], which is the rudiment of the leaves, is, indeed, rolled up in the middle of the ſeed, as Theophraſtus ſays. The flowers are roſes, and very large. The leaves are orbicular, thuriform, and about two feet in diameter[‖].

Belon has confounded the Egyptian bean with the colocaſia[¶] (*arum colocaſia* Linn.), which is cultivated in Egypt: as the Greeks often gave the name of *colocaſia* to the root of the Egyptian bean[**], it was a difficult matter to avoid confounding theſe two plants.

Sprengel remarks, that the appellation of *colocaſia* was given alſo to the *nymphæa lotus*[††].

But we receive as much information reſpecting theſe plants from antient ſculptures as from the account of hiſtorians. The roſe lotus, or Egyptian bean, is very correctly repreſented in the moſaic of Paleſtrine, an explanation of which has been given by Barthelemy in the Memoirs of the Academy of Inſcriptions and Belles-Lettres *. The fruit, the flowers, and the leaves of that plant are exceedingly like. They float on the ſurface of the water in a lake which carries ſeveral barks during the time of a feſtival. This painting brings to remembrance a paſſage of Strabo †, who ſays, that people made excurſions of pleaſure in boats on the lakes covered with beans, and that they ſheltered themſelves from the ſun with the leaves of that plant. On the Egyptian monuments Harpocrates is repreſented above the flower of the fruit of the roſe lotus. This plant, ſo well known in antient Egypt, is at preſent celebrated in the religion of the Bramins, and is often placed among the attributes of the Indian deities ‡.

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[*] Species Plantarum, p. 729.

[†] Ibid. p. 730.

[‡] Athenæi Deipnoſoph. lib. iii. p. 72; & lib. xi. p. 477. Diodorus Siculus, lib. i. Dioſcorides, lib. ii. cap. 97.

[§] Gærtner De Fructibus, &c. tom. i. p. 74. tab. 19.

[‖] Hortus Malabar. tom. xi. p. 61.

[¶] Belon, Obſervations, lib. ii. chap. 28.

[**] Athenæi Deipnoſoph. lib. iii. p. 72 & 73.

[††] Antiquitat. botanic. Specimen prim. c. 89.

It is not poſſible to ſay to what kind of lotus the flowers ſeen repreſented on the heads of the Egyptian kings or deities in ſeveral medals belonged, becauſe the ſpecies of lotus differ chiefly in regard to the colour of their flowers, and the form of their fruits or leaves; but on the walls of the temples of Egypt, and on the caſes containing the mummies, they may be eaſily diſtinguiſhed when the painting is in good preſervation. The Egyptians often repreſented the leaves of the white lotus (*nymphæa lotus*) of the ſame ſize as the flowers, though naturally the leaves are much larger; but they have omitted to mark the indentations of theſe leaves, which indeed are wanting when the plant is very young. I have, however, ſeen in the table of Latopolis this lotus repreſented with the leaves indented.

But it would be in vain to ſeek for ſcrupulous exactneſs in allegorical ſculptures. Thus on the baſe of the ſtatue of the Nile, placed in the gardens of the Tuileries, the fruit of the lotus is very exactly repreſented, but the leaves which accompany it are not thoſe of that plant.

The fruit of the white lotus, which has the ſame form as that of the poppy, may be diſtinguiſhed on the Egyptian monuments. In my opinion, this reſemblance has made the fruit of the lotus, delineated on ſeveral Egyptian medals, to be confounded with that of the poppy. The fruits repreſented on theſe medals are the ſame as thoſe ſculptured on the Egyptian monuments anterior to thoſe of the Greeks. It does not appear from any hiſtorical teſtimony that the Egyptians ever made much uſe of the poppy; and they rather placed the fruits of the lotus among the attributes of Iſis, with ears of corn, as a ſign of abundance and fertility, ſince they were long accuſtomed to make a ſort of bread with the ſeeds of that plant. The Egyptian lotus was very little known to the Greeks and the Romans, who compared it to the moſt common plants. Herodotus calls the lotus a *lily*; Theophraſtus compared it to the *poppy*, and Pliny calls its flowers *poppies* [*]. Another cauſe which may have occaſioned the lotus to be confounded with the poppy, is the reſemblance which exiſts between the attributes of Iſis and thoſe of Ceres, to whom the poppy was conſecrated [†].

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* Hiſtoire de l'Acad. des Inſcript. 1790. The *Pitture antiche di Pietro S. Bartholi*, which repreſent the moſaic with its colours, may be ſeen in the library of the Pantheon.

† *Strabo*, lib. xvii. The Greek text has been badly interpreted by tranſlators, who ſuppoſed that the boats rowed along under the ſhade of the leaves, which roſe to a great height above the water. Theſe leaves float on the ſurface of the water, and are very broad. It appears that the boats were covered with them in order to form a ſhade, as they are covered in Egypt at preſent with the leaves of the date-tree and with reeds.

‡ Syſtema Brahmanicum Fr. Paullinii, à Bartholom. tab. 9 & 10.

The *nymphæa lotus* and the *nymphæa nelumbo* are the two ſpecies of lotus deſcribed by Herodotus and Theophraſtus. Both grew naturally in Egypt, and were cultivated in that country. It was the fruit of the cultivated *nymphæa lotus* to which Pliny gives the name of *lotometra* [‡].

A paſſage in Athenæus proves that the *nymphæa cærulea* is a third ſpecies of lotus. This author relates that Antinoian [§] crowns were made at Alexandria with the roſe lotus, and that the blue were interwoven with theſe crowns. The flowers of theſe different kinds of lotus are very odoriferous, have great ſplendour and freſhneſs, and muſt have been ſelected for making crowns. Heliodorus relates, that couriers who announced a victory in Meroe were crowned with lotus [‖]. When Plutarch ſpeaks of a crown of melilot which fell from the head of Oſiris [¶], and when he claſſes that plant among thoſe which grow in the Nile, he alludes to a crown of lotus. Athenæus relates, that the lotus was alſo called melilot [**], and that it was formed into melilotine garlands. The ſame hiſtorian tells us alſo why the roſe lotus was called the Antinoian. A poet preſented the emperor Adrian, during his reſidence at Alexandria, with a roſe lotus as a rarity, and ſaid that this lotus, which had grown up in a country moiſtened by the blood of ſo terrible a lion, ought to be called the Antihoian. The lion of which the poet here ſpeaks had ravaged a part of Libya, and had at length been deſtroyed by the emperor Adrian during a hunting excurſion.

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[*] Plin. Hiſt. Nat. lib. xiii. cap. 17.

[†] Virgil. Georg. i. ver. 212. Ovid. Faſt. lib. iv. Theoc. Idyll. vii. ver. 153. Callimach. Hymn. Cer. ver. 45.

[‡] Plin. Hiſt. Nat. lib. xxii. cap. 21.

[§] Athenæi Deipnoſoph. lib. xv. p. 677.

[‖] Heliodor. Ethiop. lib. x. cap. 28.

[¶] Treatiſe on Iſis and Oſiris.

[**] Athenæi Deipnoſoph. lib. iii. p. 73.

Independently of the plauſible conjectures of ſeveral writers reſpecting the origin of the religious employment which the Egyptians made of the lotus, theſe plants, on account of their ſimple and natural properties, muſt have been very much celebrated in antient Egypt. This country being indebted to the Nile for its proſperity, its inhabitants conſidered as the ſigns of a great benefit the plants which grew in the waters of that river. The flowers of the lotus riſe to the ſurface of the water when the Nile begins to increaſe, and announce the inundation, which is about to bring abundance along with it. Beſides the names of *bachenin* and *naufar*, which the Egyptians give to the nymphæa, they call them alſo *araïs el Nil*, that is to ſay, which grow up in the Nile. Theſe names certainly refer to the fertility about to be renewed by the preſence of the waters.

The Egyptians collected the roots of the lotus when the waters of the Nile retired. At preſent they are ſeldom collected; but they multiply very much in the rice-fields, ſo that the peaſants are obliged to pull them up after the rice harveſt. They then ſometimes eat theſe roots, which they call *biaro*. I ſaw ſome of them ſold in the market of Damietta in the month of Frimaire, year 7: I taſted them, and found that their taſte was not diſagreeable. Theſe roots are round, or ſomewhat oblong, and ſmaller than a common egg. The rind is black, and coriaceous: they exhibit tubercles, traced out by the baſe of the petioles or ſhoots. Theſe roots in the inſide are white and farinaceous; in the centre they are yellowiſh. After the inundation they remain ſunk in the earth which has become dry; and the following year, when covered by water, they ſend forth leaves and ſmall roots merely from the ſummit, which has a cottony appearance. The radiculæ penetrate laterally into the mud, where they produce tubercles which become ſimilar to the former roots, and which multiply the plant. The Egyptians to collect the ſeeds waſhed them, after leaving the rind of the fruit to putrefy. This method is the only one that could be employed, otherwiſe theſe ſeeds would become mixed and deſiccated with the parenchyme of the fruit. Theſe ſeeds are very ſmall, roſe-coloured or gray on the outſide, and farinaceous within. The antients compared them to grains of millet. I have heard the peaſants call them *docbn el' bacbenin*, that is to ſay, millet of bachenin: but they told me that they were of very little uſe. The roots and ſeeds of the *nympbæa lotus* and thoſe of the *nympbæa cærulea* are ſimilar. It is natural, then, to believe that the Egyptians not only made garlands of the blue lotus, but that they uſed it for food like the *nympbæa lotus*. This is proved by the Egyptian ſculptures, ſince the blue nymphæa is often repreſented among the offerings of fruits in the grottos of Said, the paintings of which exhibit ſcenes of domeſtic life.

Of the two kinds of nymphæa, the Egyptians at preſent prefer that with blue flowers, which is often repreſented in the temples. Ebn il Bitar, an Arabian phyſician, who has written a treatiſe on plants, quoted by Proſper Alpinus *, diſtinguiſhes two kinds of *bacbenin* or *nympbæa*, the beſt of which is called that of the Arabs. I obſerved that the peaſants of the Delta gave the name of bachenin of the Arabs to the *nympbæa cærulea*, and that they ſet leſs value on the *nympbæa lotus*.

As the antients have made very little mention of the blue lotus, one might believe that it was brought to Egypt from the Eaſt Indies along with the rice, ſince it grows abundantly in the rice-fields of the Delta; but the paintings in the temples evidently prove that this plant is as antient in Egypt as the *nympbæa lotus*.

It is certain that the *nympbæa cærulea* exiſts in India. In the *Hortus Malabaricus* it is called *citambel* †. Rumphius conſiders it as a variety with blue flowers of the *nympbæa lotus* ‡. This nymphæa grows alſo at the Cape of Good Hope, and ſeems to be ſufficiently diſtinguiſhed by the following phraſe of Breyn: *nympbæa flore cæruleo odoratiſſimo Capitis Bonæ Spei* §. A figure of it has been given by Andrews ‖.

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* Proſperi Alpini Rerum Egypt. lib. iii. cap. 10.

† Tom. ii. p. 53. tab. 27.

‡ Herbar. Amboin. tom. vi. p. 72.

§ Prodrom. ii. 26.

‖ Botaniſts Repoſitory, 197.

XLVI. *Memoir on ſome Peculiarities in the Anatomy and Phyſiology of the Shark, particularly reſpecting the Production of its Young. By* Dr. MITCHILL, *of New York**.

ALTHOUGH the generation and multiplication of animals have ſo long exerciſed the attention of philoſophers, the whole ſubject remains involved in intricacy and indiſtinctneſs. The maxim laid down by Harvey, and adopted by Linnæus, of *omne animal ex ovo,* that every animal proceeds from an egg, has, perhaps, been too generally received, ſince numerous facts, related by Mr. Bonnet, in his work entitled *Conſidérations ſur les Corps Organiſés,* evince the propagation of animals, in a great variety of caſes, from germs. Notwithſtanding the multiplicative powers of animals have been thus traced to eggs and germs, yet a popular diſtinction ſtill prevails to a conſiderable extent, of claſſing animals that propagate their ſpecies by means of genital organs, into oviparous and viviparous.

The great diſcovery of Haller, that the membrane covering the yolk of an egg was really a continuation of the membrane covering the inteſtines of the chick, had not only given countenance to the idea of the pre-exiſtence of the embryo, but has ſhown that animals, whether of the oviparous or viviparous kinds, really propagated their ſpecies in pretty much the ſame way.

Amidſt the different modes in which the embryo and its membranes are organized in different animals, there ſeems to be one caſe which has not been hitherto deſcribed with the accuracy and minuteneſs which its ſingularity deſerves: the genus of the ſqualus, which includes all the animals of the ſhark tribe, has ſome peculiarities which make theſe animals approach both to the oviparous and viviparous claſſes, without, however, belonging ſtrictly to either.

It had been known a long time, that the young of the ſhark had ſomething in their ſtructure conſiderably different from any other creatures, and figures of them have been given by Edwards in his Natural Hiſtory, and probably copied from thence into the Encyclopædia; but there has not been, as yet, any diſſection of theſe animals in this period of their exiſtence, nor any explanation of their phyſiology that I know of. It is the object of this ſhort memoir to explain the ſtructure and functions of the fœtus of a ſpecies of ſhark found frequently along the coaſt of New-York, in the waters of the Atlantic, during the ſummer months. About two years ago, as I was engaged in a fiſhing party in one of the bays on the ſouth ſide of Long Iſland, a ſhark, between four and five feet in length, was taken in the ſeine, and ſecured in our boat, without receiving any material injury. Upon examination, this animal was found to be a female, whoſe uterus contained eleven young ones, of the ſize and figure repreſented in the plate.

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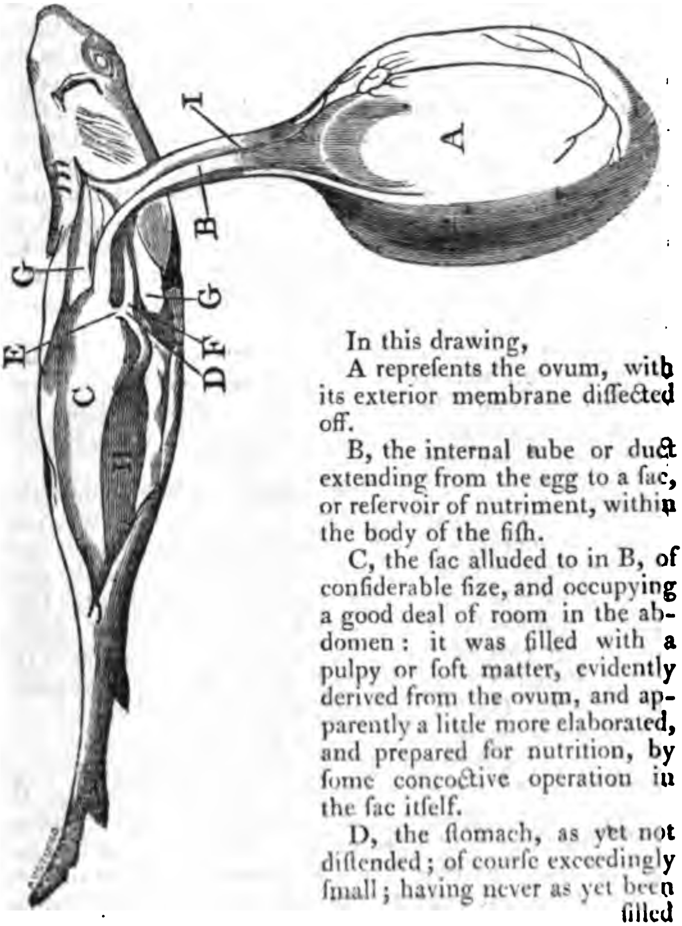
* Communicated by the Author.

Beſides theſe young ones that had advanced thus far in their growth, there were contained a large number of ova within the body of this fiſh, in different degrees of evolution and ſize, ſome of them reſembling the full grown eggs of the tortoiſe, and others ſimilar to the ſmaller rudiments of eggs found in the ovaria of laying hens. On opening the uterus with a knife, the young fiſhes, as repreſented in the figure, were found each connected with an egg, dependent from that part of the belly which may be conſidered as the umbilicus, and appearing in the form of a very large hernia. This hernia, on examination, proved to be a true ovum, filled with yolky ſubſtance, evidently intended for its nouriſhment: and what was very remarkable, the young animal, though grown to a conſiderable ſize, and connected in this manner with its egg, had no connection whatever by means of an umbilical cord, a placenta, or by veſſels of any kind, to the uterus of its dam; but it was ſo completely organized as to derive no ſuſtenance to its body, nor to receive any renovation of its blood from its parent.

The membrane covering the egg contained moſt beautiful ramifications of blood-veſſels. The arteries deſcending from the little fiſh could be ſeen ſending off their branches over its whole exterior ſurface; and terminating in veins, to unite their trunks, and carry back their contents.

The ſingularity of all theſe appearances was conſiderably heightened by the capability of the little ſharks, when cut out of the uterus, to live for a conſiderable time in the open air. The larger part of the brood had been left on the graſs of the ſhore where the dam had been diſſected; but the three which I reſerved for examination lived, and exhibited, during the greater part of the time, briſk motion for almoſt two hours, although expoſed to the temperature of a common atmoſphere. During this time, while they lay before me on a plate, nothing of the kind appeared more beautiful or diſtinct than the branches of blood-veſſels ſhooting through and running over the tranſparent membrane of the egg, the blood evidently appearing to acquire a brighter ſcarlet colour, whilſt the fiſh was thus expoſed to the air, than it had poſſeſſed during the immerſion of the young animal in the fluid of the uterus. It ſeemed to have acquired more rapidly, and to a greater quantity than before excluſion from its maternal membrane, the oxygen of the air to which it was expoſed; the union of which with the blood evidently brightened its colour, and imparted to it at the ſame time ſo much of a ſtimulant quality as to have ſhortened the duration of its life by exceſſive excitement.

The internal ſtructure of the fœtus of this ſhark may be ſeen in the plate. The diſſection was made by my colleague Mr. Poſt, profeſſor of anatomy in Columbia College; and the drawing was executed, immediately after, by Dr. Alexander Anderſon, of New York, in the preſence of the late Dr. Elihu H. Smith, Mr. William Dunlap, and Mr. Thorne.



In this drawing,

A repreſents the ovum, with its exterior membrane diſſected off.

B, the internal tube or duct extending from the egg to a ſac, or reſervoir of nutriment, within the body of the fiſh.

C, the ſac alluded to in B, of conſiderable ſize, and occupying a good deal of room in the abdomen: it was filled with a pulpy or ſoft matter, evidently derived from the ovum, and apparently a little more elaborated, and prepared for nutrition, by ſome concoctive operation in the ſac itſelf.

D, the ſtomach, as yet not diſtended; of courſe exceedingly ſmall; having never as yet been filled with food derived from the mouth. In the plate will be ſeen a ſmall paſſage or opening,

E, through which the alimentary matter in the ſac, C, paſſes into the ſtomach, to be abſorbed afterward by the lacteals of the inteſtines.

F, the œſophagus, of nearly the ſame ſize with the ſtomach.

GG, the two lobes of the liver.

H, the cloaca, or gut ſtretching towards the anus, and filled with the refuſe matter of the alimentary maſs.

I, the external membrane connecting the egg with the fiſh, cut through and turned back.

A ſpecies of ſhark, called *catulus major vulgaris*, is delineated by Edwards, but appears to be different from this. He has taken two views of the parent animal, and two more of the young. From the figures there given, it would ſeem that the ſame law of generation obtained as in this ſpecies. All the ſpecies probably breed in the ſame way.

There is a great variety in the multiplicative proceſs of living beings. The female *rana pipa*, or Surinam frog, nouriſhes its young in certain cells or cavities in her back, and not in the womb. The *opoſſum* of this country ſupports her young appendant to the teats within the paunch or ſac, called a falſe belly. The *kangaroo* of New Holland has ſomewhat of a ſimilar ſtructure and œconomy. More extenſive and penetrating inquiries ſhow the exceptions to the common mode of generation to be almoſt indeſcribably curious and diverſified. We ſee no end to the variety of ways in which the perpetuation of the ſpecies is carried on. Even Spallanzani (5 *Viaggi alle due Sicilie*, &c. p. 46), though he went to the lake of Orbitello on purpoſe to examine the anatomy of the large eels which live there, could diſcover in them no appearance of ſex.

When I publiſhed my Inaugural Diſſertation in 1786, *Circa novi Genituram Animalis*, I was decidedly in favour of the hypotheſis of pre-exiſtent germs, and of the production of animals by their gradual evolution. All my numerous experiments on generation appeared at that time to lead to ſuch a concluſion, though I have ſince had a good deal of reaſon to doubt the ſoundneſs of the inference; and the experiment now to be related, though it throws light on ſome part of the proceſs, leaves the main queſtion almoſt as unſettled as ever. In 1789 I ordered a large ſow to be killed, immediately after having had intercourſe with a male. On examining the genital organs, the blood-veſſels of the vagina, uterus, fallopian tubes, and fimbriæ, were more than uſually diſtended, and the fimbriæ in particular were in a condition of high redneſs and inflammation. Their fibres were lengthened, and reached ſo far as, on one ſide, to embrace the whole ovarium, and contain it within their incloſure. But the moſt remarkable appearances were in the ovarium itſelf. The ſow had borne pigs before. Some of the ova were of courſe exhauſted of their powers, and exhibited the appearance common in ſuch caſes. The whole ovarium was tinged with blood, and appeared to have been under the operation of a powerful ſtimulus. The entire ſubſtance ſeemed to have been enlarged. The ova partook of this enlargement, and all of them had evidently ſhared in the excitement and ſuffuſion. Some of them were but little ſwelled: ſome were ſo tumid as to be on the point of burſting: the membrane of others was ruptured, and the contents partly protruded; and, in ſeveral, the ſubſtance diſcharged from the broken ovum was fairly within the graſp of the fimbriæ. I imagined now I had before me proof ſufficient to determine the derivation of the fœtus from the mother. I accordingly examined the portions of ſubſtance diſcharged from one ovum, and extracted from others, with all poſſible care. But inſtead of finding an embryo, or any thing like the rudiment of a young animal, the little maſſes I had obtained reſembled coagulated blood more than any thing to which I can compare them, and appeared to have no more of organization or figure than is frequently ſeen in grumes or clots of that fluid.

XLVII. *On the Management and Improvement, by Tillage, of old Graſs Lands on a direct Clay, ſuch as is found in the Wilds of Surrey and Suſſex*[*].

THE ſubſtratum to this ſoil being impervious to water, the ſurface muſt conſequently be continually ſaturated with moiſture. Many landlords on ſuch ſoils reſtrict their tenants, by covenants, not to till or plough up the old graſs lands, under an apprehenſion of the ſoil being exhauſted by tillage and cropping. In this unproductive ſtate the land lets for from five ſhillings to ſeven ſhillings and ſixpence an acre; and, being always full of ſtagnant water, the produce conſiſts of poiſonous weeds, water quitch, and a little ſour graſs. Two acres will keep one head of lean cattle alive for twenty

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[*] From Mr. Cloſe's Paper in the Communications to the Board of Agriculture, vol. iii. part 1. We gave a former part of this paper in our laſt Number, p. 167.

twenty ſpring and ſummer weeks. Some labour, and a conſiderable expenſe, will be requiſite to bring this ſtubborn, ſterile ſoil to a ſtate of fertility.

I would firſt recommend the ſurface to be pared and burned, and the aſhes to be ſecured dry upon the headlands of the fields, by a covering of earth, ſods, or thatch. This operation muſt be finiſhed in the ſummer. As ſoon as the autumnal rains have moiſtened the ſoil ſo as to enable a plough to turn a furrow ſeven or eight inches deep, this operation ſhould be performed, forming each ridge with only two furrows, three feet from the centre of one ridge to the centre of the other. Then let the ſcarifier, with three long tines, or teeth, paſs at the bottom of every furrow to looſen the ſoil five or ſix inches deeper. Water furrows ſhould be cut ſo as to lay the land dry all the winter. Should the winter prove favourable, the tops of theſe ridges may be harrowed down with the fixed harrow, and the ſcarifier paſſed again through the bottom of the furrows, and the ridge formed over this deeply pulverized ſoil: if not, they muſt remain until perfectly dry in the ſpring. In tilling ſoils of all denominations in which the clay predominates, this general rule ſhould be obſerved; no injury will be ſuſtained from ploughing them when wet, previous to the winter froſts: but all the horſes on a farm had better remain idle than touch a furrow in the ſpring, until the ſoil be perfectly dry. When in this latter ſtate, harrow down the tops of the ridges; ſcarify, with Mr. Cooke's ſcarifier, the bottoms, and once more reverſe the lands, forming the ridges where the furrows were. Thus a depth of fifteen inches of finely pulverized ſoil will be obtained by two half ploughings, two ſcarifyings, and two harrowings; at leaſt the ſoil will be ſo much opened to that depth as to admit the rain water to paſs freely. Every ſhower will not make a paſte on the ſurface, neither will forty-eight hours ſun form it into bricks. In ſhort, by ſtirring this land deep you avoid the injuries to which it was before expoſed by exceſſive wet or drought, and the ſoil will become healthy and productive. Should it not be ſufficiently pulverized for turnips, ſcarify, harrow, and roll, with a heavy roller only five feet long, until a garden tilth be obtained, leaving the land, when completely fine, on three-feet ridges. Previous to putting aſhes on for drilling turnips, beat down three or four inches of the tops of the ridges with a ſled, or beam of wood five feet long, annexed to the handles of Mr. Cooke's ſcarifier. On this bed of finely pulverized mould in the furrows ſpread the aſhes; about eighty buſhels on an acre. This work will be eaſily performed, as the horſe will walk in the furrow, and each wheel of the cart will occupy another. As the horſes and the cart-wheels will make the ſoil in the bottom of the furrows immediately under the aſhes compact, paſs the ſcarificator through, as before directed, with only three long tines, or teeth, in the centre of the beam, then with one bout of the common plough reverſe the ridges and cover the aſhes. Let the land continue in this ſtate until the ſeaſon ſuit for ſowing. Then with the ſled, as before, level the tops of the ridges ſo as to obtain a ſurface on each ridge eighteen inches wide. Mr. Cooke's drill will ſow two of theſe ridges at a time, two rows of turnips on each ridge, nine inches from row to row, with an interval of twenty-ſeven inches. The turnips, as ſoon as the rough leaf appears, ſhould be harrowed acroſs the ridges with the fixed harrow, then ſcarified between the rows; and a few days after theſe operations they ſhould be hand-hoed, leaving the plants in rows, ſingle, and one foot apart. The intervals ſhould be kept highly pulverized by alternate ploughings, horſe-hoeings, and ſcarifying; being careful, before the autumnal rains commence, to throw all the mould from the bottoms of the furrows up to the ridges with a double mould board plough. This will not only keep the land dry and healthy all the winter; but, as the horſes will walk in one furrow, and each wheel of the cart will occupy another, the turnips may be carted to the bullock-ſheds without poaching or injuring the land.

Knowing the difficulty in theſe clayey ſoils of obtaining a fine tilth for ſpring ſowing, I ſhould recommend the ſame ridges, after the turnips are drawn, and carted home, to be worked with the fixed harrow and roller, only ſo as to obtain a fine tilth five inches deep; and then to drill two rows of oats, nine inches from row to row, leaving a furrow or interval of twenty-ſeven inches. When the oats are about two inches above the ſurface, harrow acroſs with the fixed harrow, ſcarify twice in a place between the rows of oats, and roll the whole. After theſe operations, ſhould weeds appear, horſe-hoe with the flat hoes. When the oats are harveſted, throw you ridges up high, like trenching in a garden, without reverſing them, keeping the bottoms of the furrows clean from mould, and laying the land perfectly dry. In the firſt froſt cart about twenty loads per acre of good horſe or bullock dung, not too rotten, into the furrows; ſpread it; and as ſoon as the froſt is out of the land, harrow or ſled down the tops of the ridges, reverſe them, and cover the dung: the firſt dry weather in February or March, ſled down the tops of theſe ridges, to obtain a ſurface of eighteen inches, and drill two rows of beans on each ridge, nine inches from row to row, and twenty-ſeven inches interval. Scarify, horſe-hoe, and plough between the intervals, ſo as to keep a fine tilth, and to deſtroy the weeds. When the beans are fit for harveſting pull them, and when carried off the land pulverize the tops of the ſame ridges by ſcarifying, rolling, and working with the fixed harrow; and, if any ſurface-weeds appear, ſkim the land with the ſcufflers; and by repeating theſe operations prepare the ſame ridges for wheat, and drill two rows on each ridge nine inches from row to row. In the ſpring harrow acroſs, &c. as directed for the oat crop. Should the farmer wiſh to ſubſtitute cabbages for turnips, one row ſhould be planted on each ridge, ſetting the plants two feet and a half from each other in rows, with three-feet intervals. In this ſucceſſion of crops, viz. turnips or cabbages, oats or barley, beans, peaſe and clover, and wheat, even this naturally ſterile ſoil will be in a conſtant ſtate of improvement: and if the vegetable crops and ſtraw be expended in the moſt advantageous method upon the farm, the land may be dreſſed with fourteen loads per acre twice in four years, viz. for the turnip or cabbage crop, and for the bean, pea, and clover crop. I ſhall not, therefore, limit the number of years for keeping it in tillage to any preciſe term, as it appears to me immaterial; but proceed to point out the beſt method of ſecuring a good meadow whenever the occupier may wiſh it *.

We will ſuppoſe the land cropped eight years in the rotation of crops before recommended, and at the expiration of that term, a wheat ſtubble. The firſt operation I ſhould recommend would be to drain the land ſo as to take off the ſurface water, for nothing more can be effected by draining theſe lands, as they will hold water like a bucket; and were the drains cut ever ſo deep, and only ſix inches from each other, the water would not draw from one to the other. The method I ſhall propoſe is eaſy and efficacious, and the expenſe very trifling. I ſpeak with confidence, as almoſt the whole pariſh of Hitcham, in Suffolk, where twenty years back it was thought impoſſible to grow barley, now produces fine and heavy crops of that grain, by the ſurface water being taken off expeditiouſly. Open furrows in the wheat ſtubble with the common plough, about five yards from each other, and as deep as the land was ever ploughed and ſcarified. Then with a narrow gripping-ſpade, twenty inches long,

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* Old graſs lands converted into tillage, though they might be ploughed by a judicious ſyſtem of cropping for any number of years, if naturally crude and unproductive, ſhould not, I think, be ſown with graſs ſeeds in leſs than eight years.—*Note by Mr. Cloſe.*

and only two inches and a half wide at the top, and one at the bottom, open a drain ſixteen inches deep; fill this with the wheat haulm, ramming it down as hard as poſſible; the price two ſhillings and ſixpence per ſcore rods, to dig and fill up. It is, I preſume, unneceſſary to obſerve that theſe and all other drains ſhould not be cut in a line with the natural fall of the land, but in an oblique direction. No buſhes ſhould be uſed, as, when theſe begin to decay, pieces of them will fall acroſs theſe very narrow drains, and obſtruct the courſe of the water. The ſtraw or haulm, when it decays, ſloughs off, and the water paſſing through the clay forms an arch, which will laſt many years. There are drains of this ſort ſtill open in Hitcham pariſh, which have been cut more than twenty years. Having thus freed the land from the noxious moiſture ariſing from ſtagnant water, with one bout of the plough throw the land on to three-feet ridges, and before January reverſe theſe. In the ſpring, when perfectly dry, harrow down the ridges with the fixed harrow, ſo as to make a complete fallow*; then dreſs with lime or compoſt, and baulk the land, viz. give it half a ploughing, leaving it in narrow ridges, ſo as it will harrow down quite level and fine without any furrows appearing. In the beginning of Auguſt ſow thirty-two buſhels per acre of the ſweepings of hay-chambers, where only good old meadow hay has been uſed without any broad clover and rye-graſs, five pounds of Dutch clover, and five pounds of trefoil; harrow the ſeeds three or four times over, and you may expect two tons or two tons and a half of hay per acre the firſt year; which will pay all the expenſes, and leave the land improved from five ſhillings or ſeven ſhillings and ſix-pence per acre, its original value, to fourteen ſhillings or ſixteen ſhillings per acre.

And now, gentlemen, as you ſeem particularly deſirous of obtaining information from actual experiment, permit me to aſſure you, that though I have not given you, neither is it in my power, an exact detailed account of an experiment I made on fifty acres of land in the wilds of Surrey, yet the great outlines of the plan, and the general reſult, correſpond with my actual practice. I purchaſed nearly 600 acres of land in that county for three thouſand ſix hundred pounds; the ſtock and crops were valued to me at about five hundred pounds, and the rent of the major part of the land eſtimated at five ſhillings per acre. After farming it ſome years, let the whole at ten ſhillings per acre; the tenant paid me for the ſtock and crops two thouſand and eighty pounds; and I afterwards ſold the eſtate to him at nearly double the price I gave for it, having previouſly paid myſelf the greater part of the expenſes for the improvements I had made. Fifty acres of the land will more particularly elucidate this ſubject. Part of it was broken up from old ſour graſs, not worth five ſhillings per acre, and part of it was old tillage land, which had been limed and cropped until it literally would produce no weeds except the ſow-thiſtle. After treating theſe lands, as nearly as I can recollect, according to the ſyſtem I have recommended, I fallowed and dreſſed them with a compoſt of lime and earth, and ſowed the quantity of ſeeds I have mentioned, at or near the ſeaſon recommended, and the produce was eſtimated at two tons and a half of hay per acre; and the fifty acres of land which were valued at five ſhillings per acre when I purchaſed them, were eſtimated at fifteen ſhillings per acre when I ſold the eſtate. I did not ſurface-drain the land, as I was not then acquainted with the ſyſtem, neither did I pare and burn, but dreſſed with a compoſt of lime and mould. The ſyſtem I have recommended differs from my actual experiment in theſe two circumſtances. But the ſurface-draining has proved effectual in the pariſh of Hitcham, and ſeems to carry conviction with it; and by paring and burning, as good a dreſſing as the lime may be procured, at a much leſs expenſe. Many may object to paring and burning and ploughing theſe lands into what is termed, by the farmers, the dead earth. I am of opinion theſe operations are pernicious, on really thin-ſkinned ſoils, when the immediate ſubſtratum is much inferior to the ſurface. But in the ſoil I have been treating of, the only difference between the ſurface and the ſubſtratum conſiſts in the former having been expoſed and pulverized, and the latter having remained impregnated with ſtagnant water, and never turned up ſo as to derive any benefit from the viciſſitudes of the atmoſphere. By paring and burning you deſtroy the ſurface-weeds, and obtain a cheap dreſſing; by ploughing the land deeper than ever it was ploughed before, expoſing it to a winter's froſt and ſummer's ſun, and dreſſing it with aſhes or lime, you obtain a depth of ſweet mellow ſoil, not eaſily injured by wet or drought, and adapted to all the purpoſes of vegetation. To prove the utility of moving theſe clay ſoils fourteen or fifteen inches deep, let any one cover a pewter diſh, or any ſubſtance like the ſubſtratum of theſe lands, impervious to water, with earth one inch deep, and expoſe it to a trifling ſhower, and the whole maſs will become a quagmire; let another diſh be covered with the ſame ſoil fifteen inches thick, and it is evident it will require fifteen times the number of cubic inches of water to put this into the ſame boggy ſtate. The former alſo by twenty-four hours dry weather will become as hard as a brick, when the other will only have a ſlight incruſtation formed on the ſurface. Suppoſing this effect to be produced on your field, by paſſing the fixed harrow once acroſs the land, it will be put into a ſtate of fine pulverization. By drilling on three feet ridges, you keep the land in a dry healthy ſtate; and by ſowing your ſpring crop on the ſame ridges, you obviate the only objections to growing green winter crops on wet lands, viz. cutting up the lands by carting off the crop, and the impracticability of afterwards preparing them for ſpring ſowing. In carting, the horſes walk in one furrow, and the wheels track in two other furrows; the tops of the ridges are always dry; and the fixed harrow paſſing twice in a place, will prepare ſuch lands for ſowing, before the farmers in the common mode of management, without a crop of green food, can put a hoof on their lands. Should ſurface weeds appear before ſowing, Mr. Cooke's ſcufflers, or broad ſhares fixed in the ſcarifying-beam, ſhould be paſſed through the land.

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* The ſcarifiers would be dangerous inſtruments to uſe after the drains are cut, as they, by penetrating too deep, might pull down the ſides of the drains, and bring the haulm to the ſurface.——*Note by Mr. Cloſe.*

[To be continued.]

Erratum.—In the laſt Number, p. 168, laſt line, for Cole read Cloſe.

XLVIII. *Some Account of* GEORGE PEARSON, *M. D. F. R. S. &c. &c. with a Portrait from an original Painting.*

WE are ſenſible that an editor of the biography of living perſons ſhould conſider himſelf as in a very delicate ſituation, and the more reſpectable the character who is his ſubject, the greater ſhould be his care; for utterly unmerited eulogy, from motives of intereſt or vanity, is ſo uſual, that any praiſe at all, or at leaſt exceeding what is already generally conferred and confeſſedly due, muſt be offenſive to a man of intrinſic merit. But theſe conſiderations need not reſtrain an editor from gratifying the curioſity of the public, who may not perſonally know a man of eminence, by a ſtatement at leaſt of the departments in which he has been occupied, and a recital of his publiſhed literary labours.

Dr. Pearſon graduated at Edinburgh at a very early period of life, and of courſe he muſt have previouſly ſtudied a certain number of years in that univerſity. An extract from his inaugural diſſertation, *De Putredine*, may be found in the Medical Commentaries for 1775. Several communications on medical and philoſophical ſubjects, ſubſequently to this period, are inſerted in that annual work.

In 1784 Dr. Pearſon publiſhed a treatiſe on the Buxton waters, principally to make known his diſcovery that the gas which iſſues from them ſpontaneouſly is not, as was believed, fixed air or carbonic acid, but phlogiſticated air or azotic gas. Dr. Saunders, in his late treatiſe on Mineral Waters, has given due credit to the author for this diſcovery, as well as for his able analyſis in general.

In the year 1788 Dr. Pearſon publiſhed in ſeveral journals his account of the ſalt compoſed of phoſphoric acid and ſoda, recommending it as a purgative which poſſeſſes no diſagreeable taſte, and is equally mild with any other of the cathartic ſalts. It is one of the articles in the Edinburgh Pharmacopæia of 1791, under the title of Soda Phoſphorata: ſince that time it has been in general uſe.

In the Philoſophical Tranſactions for 1791 we find Dr. Pearſon's maſterly and at that time unrivalled analyſis of the James's Powder. Immenſe ſavings have been made by individuals ſince this diſcovery, eſpecially in the navy, to the amount, as the late Dr. Johnſon, one of the Commiſſioners of the Sick and Hurt Office, aſſerted, of ſeveral thouſand pounds a year.

In the Philoſophical Tranſactions for 1792 is a paper entitled, Experiments made with a View of decompounding Fixed Air or Carbonic Acid, by Dr. Pearſon. In 1789 M. Lavoiſier ſhowed that carbonic acid might be compoſed by uniting carbon to oxygen: but the author in the preſent paper ſays, "The honour of the firſt analytical experiments on carbonic acid is due to Mr. Tennant, F. R. S." who obtained phoſphoric acid and carbon by paſſing phoſphorus through red-hot carbonate of lime, or marble powder. Whatever doubts were entertained of this fact, as ſtated by Mr. Tennant, they were entirely removed by the deciſive experiments of Dr. Pearſon.

In the Philoſophical Tranſactions for 1794 is inſerted Dr. Pearſon's paper on the wax-like matter called white lack, in which he diſcovered a new acid called by him *laxic acid*, according to the new nomenclature of chemiſts.

In the ſame work for 1795 is Dr. Pearſon's paper on the ſteel ſuppoſed to be made directly from the ore by the Hindoos, called *wootz*.

In the following year is our author's paper on the compoſition of the hard and ſtrong metals of the antients, which he determines to have been copper united to tin, as demonpound-weight, than that it is as heavy as 7680 grains of wheat taken from the middle of the ear*.

"Theſe ideas were naturally ſuggeſted to the minds of the authors by the neceſſity which they have been under, in the courſe of their trade, of deciding on which of the various diſcordant authorities they ſhould found their eſtimation of the weight of given meaſures of different fluids, and of the relation which theſe meaſures of capacity bear to the linear meaſures of the country; and there cannot, perhaps, be a ſtronger proof of the propriety of a declaratory act to ſettle theſe matters, than that there ſhould ſtill remain ſomething to be aſſumed with reſpect to every one of them. This important meaſure is, however, as the writers of this eſſay believe, now in the contemplation of the government.

"It is ſtated in § 16, that they have eſtimated the wine-gallon at 231 cubic inches, the pound avoirdupoiſe at 7000 grains troy, and the relation between troy weight and linear meaſure to be ſuch, that the weight of a cubic inch of diſtilled water at 60°, when weighed in air at 60°, ſhall be equal to 252½ of the ſame grains.

"The aſſumption with regard to the wine-gallon is founded on the eſtabliſhed practice of the Boards of Cuſtoms and Exciſe to conſider it as of the content which is here given; it being ſufficiently known, from a very accurate experiment which was made on this ſubject May 25, 1688, in the preſence of the Lord-Mayor, the Commiſſioners of Exciſe, Dr. Halley, Mr. Flamſtead, and other men of ſcience, that the old wine gallon in Guildhall really contains only 224. The former, however, is, perhaps, at the preſent day, the legal wine-gallon.

"The ſuppoſition with reſpect to the relation between avoirdupoiſe and troy weight, is chiefly founded on a kind of tacit agreement amongſt the majority of ſcientific men at preſent to conſider the avoirdupoiſe pound as equal to 7000 grains troy; and that which is, perhaps, of ſtill more practical effect with regard to it,—the opinions of the beſt informed amongſt our ſcale-makers: for, whether it ariſes from the ſmaller value of the articles which are weighed by the former, and its conſequent leſs minute ſubdiviſion; from its introduction by cuſtom rather than by ſtatute; or from whatever other cauſe it may be derived, it appears to have been always ſubject to more uncertainty than the latter. Mr. Ward tell us, in his Mathematics, that he found, by a very nice experiment, that the pound avoirdupoiſe weighed 6999½ grains troy, and yet we find, from the Report on the Weights of Europe in general, in the Memoirs of the French Academy of Sciences for 1767, that the ratio between the avoirdupoiſe and troy pounds, which M. Tillet had obtained for the purpoſe, was that of 7004·5 to 5760.

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* "Per ordinacionem tocius regni Anglie fuit menſura domini regis compoſita, videlicet, quod denarius qui vocatur ſterlingus rotundus et ſine tonſura ponderabit triginti duo grana frumenti in medio ſpice. Et uncia ponderabit viginti denarios. Et duodecim uncie faciunt libram London." Stat. "De Ponderibus et Menſuris." 31 Ed. I.

“ The appreciation of the weight of a cubic inch of diſtilled water is principally founded on the experiments of Sir George Shuckburgh Evelyn, Bart., deſcribed in the Philoſophical Tranſactions for 1798; from a mean of which it follows *, that the cubic inch of diſtilled water at 60°, under the circumſtances deſcribed in § 16, weighs 252·506 grains of the ſtandard troy pound made for the Committee of the Houſe of Commons in 1758, or 252·816 if weighed in vacuo.

“ A variety of motives concur to induce the authors to take theſe as ſtandard experiments. All the older attempts of this kind are of no value whatever, having been made with water which was not diſtilled, and under other circumſtances which render them liable to innumerable errors: even the celebrated experiment of Mr. Everard in 1696 was made with undiſtilled New River water. The only ones, perhaps, which are at all to be compared with thoſe of Sir G. S. Evelyn are thoſe of Dr. Robiſon, and the French Commiſſioners of Weights and Meaſures.

“ The former gentleman weighed a cylinder whoſe height and diameter were each 6 inches, taken, as he ſays, from a moſt accurate copy of the Exchequer ſtandard, ſeveral times in diſtilled water at 55°, and found that it loſt 42895 grains of its weight without a variation of 2 grains in the whole. Now, the ſolid content of the above body being $6^3 \times .785398$, or 169·646 cubic inches, a cubic inch of water weighs in air, according to this experiment, 252·85 grains at 55°; or, by Mr. Gilpin's Tables, exactly 252¾ at 60°.

“ The authors of this eſſay are, however, induced to prefer the experiments of Sir G. S. Evelyn to that of Dr. Robiſon, principally for the following reaſon:—He does not tell us whether his weights had been immediately and accurately compared with any of the parliamentary ſtandards. The adjuſtment of the beſt weights is generally thus performed:—They are, in the firſt place, filed to ſomething near the required weight; then ground on a ſtone, and afterwards po-

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* See Philoſophical Journal, Octavo Series, vol. iv, p. 35 (No. for Jan. 1803.)

liſhed. Now, it very frequently happens, that ſome of theſe operations are carried a little too far, and one or two of the weights rendered ſomewhat too light; and when this is the caſe, unleſs the workman can be abſolutely depended on, the whole ſet will infallibly be ſpoiled; for, inſtead of making new ones to ſupply their place, he will, nine times in ten, prefer the adjuſting the others to them; conceiving it to be of more conſequence that they ſhould all agree, than that they ſhould be accurately of any particular weight. When weights are ſucceſſively copied from each other a number of times, the laſt of the ſeries will therefore frequently be lighter than the firſt, and ſometimes very conſiderably ſo.

“For the appreciation of the weight in troy grains of a cubic inch of diſtilled water at 60°, from the experiments of the French commiſſioners of weights and meaſures, we have the following data.

“We are told by the *Inſtruction ſur les Poids & Meſures*, prefixed to M. Briſſon's *Traité de Phyſique*, and which is certified by the commiſſioners to be “exactly founded on the reſults obtained and recorded by the ſaid commiſſion,” and in ſeveral other works, that they weighed diſtilled water in nine different caſes at various temperatures, in air and in vacuo. We will, however, ſelect the caſe which they have thought proper to aſſume as the baſis of their ſtandard of weight, viz. that in which the weight of water is taken at its maximum of denſity (or about the temperature of 40° of Fahr.) and in vacuo; under which circumſtances they inform us that the cubic decimetre weighs 2 pounds 5 gros and 35·15 grains, or 18827·15 grains of the *poids de marc*.

“This, according to M. Tillet's appreciation of the Engliſh troy pound, which he makes equal to 7021 of theſe grains, is equal to 15445·7 grains troy.

“Now, we find from the report made to the Mathematical and Phyſical Claſs of the National Inſtitute on the 27th of December 1801 *, relative to a compariſon of the metre with a ſcale of M. Pictet's, which was exactly ſimilar to that of Sir George Shuckburgh Evelyn, that this meaſure is equal to 39·38272 inches of that ſcale when at 32°; or, correcting for temperature according to General Roy's table of the expanſion of his braſs ſcale, in the 75th volume of the Philoſophical Tranſactions, equal to 39·3712 inches of the ſame ſcale when at 60°; the French platina and iron ſtandards, when at the freezing point, indicating the length of the metre.

“The cubic decimetre, therefore, is equal to 61·02896 cubic inches of Sir G. S. Evelyn's ſtandard at 60°, and 15445·7 ÷ 6 ·02896 is equal to 253·088; the weight in troy grains of ſuch a cubic inch of water at its maximum of denſity and in vacuo, according to the experiments of the French commiſſioners.

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* Philoſophical Magazine, vol. xii. p. 129.

"Now, according to Mr. Gilpin's Tables (Phil. Tranſ. for 1794, p. 382), the ſpecific gravity of water in this ſtate is to that which it poſſeſſes at 60° as 1000·94 to 1000. But 1000·94 : 1000 : : 253·088 : 252·851. The weight of a cubic inch of water, therefore, according to this method of calculation, would, if weighed *in vacuo* at 60°, be 252·85 grains; and if weighed in air at the ſame temperature, under the preſſure of $29\frac{1}{2}$ inches, = 252·55; being within $\frac{1}{1000}$th part of the weight deduced from Sir G. S. Evelyn's experiments.

"It was thought indiſpenſable thus to ſtate the hypotheſes on which the authors have proceeded with reſpect to their calculations, and conſequently with regard to the inſtruments which they have hitherto manufactured for aſcertaining the relative ſtrengths and values of liquors of various deſcriptions, and particularly thoſe which conſtitute the ſubject of theſe ſheets. The variation of denſity in alcoholic compounds is a circumſtance which is not only of importance to the merchant, the ſpirit dealer, and the revenue officer; to the philoſopher, who is engaged in the inveſtigation of the nature and progreſſion of thoſe forces by which the conſtituent particles of matter are connected with each other, it muſt alſo afford matter of intereſting contemplation. It is, therefore, of no inconſiderable conſequence that every thing relating to it ſhould be eſtabliſhed on the beſt authorities and the ſureſt foundations, and that the public ſhould be enabled to judge of the degree of credit which is to be given to thoſe who endeavour to elucidate ſo important a ſubject."

L. *Proceedings of Learned Societies.*

GALVANIC SOCIETY, PARIS.

THIS ſociety has organized a commiſſion of experiments. Thouret is elected preſident; Aboville, vice-preſident; and Izarn, ſecretary.

M. Winckler gave an extract of ſeveral obſervations on caſes of deafneſs cured by M. Schaub, of Caſſel.

The ſenator Abrial communicated a memoir of M. Pfingſten, director of the inſtitution for deaf and dumb at Kiel, tending liſhed. Now, it very frequently happens, that ſome of theſe operations are carried a little too far, and one or two of the weights rendered ſomewhat too light; and when this is the caſe, unleſs the workman can be abſolutely depended on, the whole ſet will infallibly be ſpoiled; for, inſtead of making new ones to ſupply their place, he will, nine times in ten, prefer the adjuſting the others to them; conceiving it to be of more conſequence that they ſhould all agree, than that they ſhould be accurately of any particular weight. When weights are ſucceſſively copied from each other a number of times, the laſt of the ſeries will therefore frequently be lighter than the firſt, and ſometimes very conſiderably ſo.

"For the appreciation of the weight in troy grains of a cubic inch of diſtilled water at 60°, from the experiments of the French commiſſioners of weights and meaſures, we have the following data.

"We are told by the *Inſtruction ſur les Poids & Meſures*, prefixed to M. Briſſon's *Traité de Phyſique*, and which is certified by the commiſſioners to be "exactly founded on the reſults obtained and recorded by the ſaid commiſſion," and in ſeveral other works, that they weighed diſtilled water in nine different caſes at various temperatures, in air and in vacuo. We will, however, ſelect the caſe which they have thought proper to aſſume as the baſis of their ſtandard of weight, viz. that in which the weight of water is taken at its maximum of denſity (or about the temperature of 40° of Fahr.) and in vacuo; under which circumſtances they inform us that the cubic decimetre weighs 2 pounds 5 gros and 35·15 grains, or 18827·15 grains of the *poids de marc*.

"This, according to M. Tillet's appreciation of the Engliſh troy pound, which he makes equal to 7021 of theſe grains, is equal to 15445·7 grains troy.

"Now, we find from the report made to the Mathematical and Phyſical Claſs of the National Inſtitute on the 27th of December 1801 *, relative to a compariſon of the metre with a ſcale of M. Pictet's, which was exactly ſimilar to that of Sir George Shuckburgh Evelyn, that this meaſure is equal to 39·38272 inches of that ſcale when at 32°; or, correcting for temperature according to General Roy's table of the expanſion of his braſs ſcale, in the 75th volume of the Philoſophical Tranſactions, equal to 39·3712 inches of the ſame ſcale when at 60°; the French platina and iron ſtandards, when at the freezing point, indicating the length of the metre.

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M. Winckler gave an extract of ſeveral obſervations on caſes of deafneſs cured by M. Schaub, of Caſſel.

The ſenator Abrial communicated a memoir of M. Pfingſten, director of the inſtitution for deaf and dumb at Kiel, tending to prove that Galvaniſm is of very little uſe in deafneſs.

M. Mojon gave an account of ſome experiments which ſeem to prove that Galvaniſm is proper for retarding the putridity of animal matters.

M. Nauche the preſident, and Pajot-Laforêt, communicated a new Galvanic phænomenon. Having ſubjected to the action of the pile frogs expoſed to a temperature of ten degrees below zero, they obſerved, that repeated contact of the conductor communicating with the copper pole produces the development of a whitiſh mucous fluid, exceedingly abundant in the liver, lungs, and particularly the nerves and the heart; not very abundant in the muſcles, the inteſtines, and none in the integuments; while the conductor in communication with the zinc pole produces no development of this fluid, and ſeems rather to cauſe that which has been produced to diſappear.

C. Gautherot has made a ſeries of experiments tending to prove that electricity is developed in the ratio of the ſurfaces.

C. Nauche the preſident, with his fellow-labourers Bonnet and Pajot-Laforêt, has been able, by means of two *homogeneous* metallic conductors, to draw off the electric fluid from the brain and ſpinal marrow of an ox recently killed, and to convey it to the thighs of a frog, where it produced muſcular contractions. This operation ſucceeded alſo in the palpitating muſcles, and could be continued only a quarter of an hour after death. The ſenator Lamartillière gave an explanation of the diſengagement of mucous matter by the poles, and ſhowed that it ariſes from a chemical decompoſition.

C. Izarn gave an account of the conſtruction of a pile invented by C. Alizeau, in which, inſtead of diſks moiſtened with a ſaline ſolution, a ſtratum of moiſtened ſalt is employed, and which can produce its effects for a month without being cleaned.

The Commiſſion of Medical Application, conſiſting of Guillotin, Dudaujon, Petit-Radel, &c. have made a great number of experiments on aſphyxia by ſtrangulation.

The application of Galvaniſm to diſeaſes, ſuſpended in conſequence of the winter ſeaſon, is going to be reſumed at a place deſtined for the purpoſe in the School of Medicine, and in the private laboratory of the ſociety.

LI. *Intelligence and Miſcellaneous Articles.*

ANTIQUITIES.

GENERAL REYNIER having preſented to the French National Inſtitute a tunic and the remains of ſome antient veſtments found near Sakara, in Egypt, the three claſſes appointed commiſſioners to give in a report on theſe curious articles. A very exact idea may be given of this tunic by comparing it to the tunics worn by the deacons and ſubdeacons of the Roman catholic church, if we ſuppoſe the latter to have long cloſe ſleeves, as was formerly the caſe. It is ornamented with embroidered pieces, ſome of which deſcend from the ſhoulders; others are applied to the ſhoulders, and at the bottom before and behind: two pieces of the ſame kind ſurround the extremity of the ſleeves. The colour of the ſtuff is a ſouci yellow, and the embroidery is puce colour, or dark brown. The deſign is of little importance, and has no relation either to natural objects, to hieroglyphics, or to written characters. The ſtuff has been wove in the loom, but the embroidery ſeems to have been executed according to the proceſs of tapeſtry *au petit point*. In regard to the nature of them, chemiſts have found that the yellow ſtuff of the tunic is animal matter. In regard to the embroidery, the yellow tiſſue or ground is vegetable matter, and the brown thread, animal matter.

General Reynier was not able to procure any further information from the inhabitants of Sakara, who ſold him this tunic, except that they had found it with other articles in a pit filled with ſand which they had dug up.

No particular account can be given of the time when this tiſſue was wove, nor of the perſonage to whom it belonged. In the report made on it to the Inſtitute, the author ſhows that it belonged neither to a Macedonian nor a Greek eſtabliſhed in Egypt, as it has ſleeves which deſcend to the wriſt, whereas the Grecian tunic had no ſleeves, or ſleeves ſo ſhort that they reached only to the elbow; but it appears certain that the Macedonian tunic was not different from the Grecian. The Macedonians were diſtinguiſhed from the other Greeks only by their head-dreſs (*cauſia*) and their chlamys. The tunic of Sakara belonged then to an Egyptian. But the point is to determine at what period. The author of the report only ſays, that it cannot be older than the time when Thebes was abandoned. The grottoes of Sakara, which are at the diſtance of a few miles from Thebes, near the ruins of Thebes, were no doubt dug about that time. It was in the ſixth century before the Chriſtian æra that Cambyſes ravaged Thebes, and plundered it of its riches and monuments. The moſt remote period, therefore, which can be aſſigned to the time when the tunics were wove is the fifth or fourth century before Auguſtus.

It is impoſſible to ſay any thing more correct in regard to the perſonage who wore it. We are told by Herodotus that the Egyptian prieſts wore a ſingle veſtment of linen, and ſhoes of papyrus. The prieſts of Iſis at Rome were called the troop clothed in linen, *linigera turba.* Pythagoras, who imitated them, made no uſe of woven ſtuffs made from animal matters. The tunic of Sakara was therefore not worn by an Egyptian of the ſacerdotal order.

It did not form part of a female dreſs, for we are told alſo by the father of hiſtory that the Egyptian women were clothed in linen. The men uſed alſo ſimilar veſtments; but they wore above theſe tunics white woollen veſtments. "But," Herodotus adds, "they do not wear woollen veſtments in the temples, and they do not bury their dead in dreſſes of that kind: they would be conſidered as indecent."

The yellow colour of the tunic, if not the effect of antiquity, would be no proof of its not having belonged to an Egyptian; but if it be the effect of art, we may ſuppoſe that it was the diſtinguiſhing mark of ſome dignity. In a word, the only thing certain which can be ſaid of this Egyptian veſtment is, that it was not conveyed with the body of a deceaſed perſon to the grottoes of Sakara, which ſerved as tombs, becauſe it was repugnant to the ideas of the Egyptians to be interred in wove woollen cloth. It muſt therefore have been depoſited with other riches which the proprietors wiſhed to ſecure from the rapacity of the enemy.

C. Mongez has communicated to the Inſtitute a memoir on ſome antient medals and other articles diſcovered near Aurillac, in the department of Cantal. Some workmen digging in a meadow found the portion of a circular incloſure conſiſting of a double wall. It was about ſix feet in diameter, and four feet eight inches in height; but roſe very little above the ſurface of the meadow. The interior wall was of baked brick, exceedingly beautiful, circular, and with the joinings tending to the centre. This interior wall was ſurrounded by another of dry ſtones, deſtined, no doubt, to ſupport the earth. No traces of a covering appeared, and it is probable there never was any.

In digging in this incloſure, and demoliſhing it, there were firſt found medals of gilt bronze of all the Roman emperors down to Commodus, Otho excepted; which proves that in the ſecond century bronze medals of that prince were exceedingly rare, and that they had not then, perhaps, been carried into Gaul. Along with the ſmall veſſels of baked earth which contained theſe medals, there were dug up ſome ſmaller ones which contained aromatic ſubſtances. The workmen took them for tobacco; but finding by the taſte that they were deceived, they threw them away, and carefully waſhed the veſſels which contained them; ſo that neither the nature of theſe aromatics, nor even the odour which the veſſels would have retained, could be known. Two ſmall bits of very white moulded clay were alſo dug up: one repreſented a dog, the legs of which were broken; the other was a female buſt, but the remainder of the body was deſtroyed by the digging. A claſp of bronze, and a glaſs ring of ſo large a ſize that it could be worn only on the thumb if it ſerved for a ring, were alſo dug up.

The circular form of the incloſure diſcovered near Aurillac induces C. Mongez to conjecture that it might have been uſed for burning dead bodies, and that it was an *uſtrinum*, ſuch as the circular incloſure of earth in which the body of Auguſtus was burnt, and which was religiouſly preſerved near his mauſoleum, ſtill in part exiſting; and ſuch as the incloſure of the ſame form diſcovered in 1763 near Placentia, in the ruins of the antient Veleia, which appears to have been buried by the fall of a mountain, and which Winckelmann found to be an *uſtrinum*. Some of the inſcriptions on antient ſepulchral ſtones of the Romans announce an expreſs prohibition to conſtruct an *uſtrinum* near a monument. The reaſon of this prohibition has not yet been examined. After mentioning the law of the twelve tables, which forbade the burning of bodies nearer any edifice than the diſtance of ſixty feet, without the owner's conſent, C. Mongez is of opinion that this prohibition ſuppoſed ſome edifice to be in the neighbourhood, the proprietor of which inſiſted on the rigorous execution of the law.

In regard to the ſmall female buſt found near Aurillac, C. Mongez obſerves, that Montfaucon has given engravings of four female figures of the like kind. They were all of that kind of argillaceous earth called tobacco-pipe clay; the workmanſhip was coarſe, and they had all been moulded: one of them was found in 1710, in digging in the abbey of Saint-Lomer, at Blois. It was depoſited in a ſmall grotto containing the half-burnt bones of animals, among which were found the thigh-bone of a horſe and the tooth of a dog. The Gauls were accuſtomed to throw into the funeral piles of their dead, thoſe animals they had been fondeſt of, ſuch as dogs and horſes. There is reaſon, therefore, to conclude that the grotto of Blois was a burying-place of the Gauls, and, by analogy, that the incloſure of Aurillac belonged to the ſame nation. The female buſt found there exhibits the ſame characters as the figures publiſhed by Montfaucon; and beſides, there was found along with it a figure of a dog of the ſame ſubſtance and workmanſhip.

Theſe female figures have been ſo often dug up in the ſepulchres of the Gauls, and the ſtyle, the workmanſhip, and materials have ſo great a reſemblance, that one cannot help ſuppoſing that they muſt have been depoſited there from the ſame motives. C. Mongez is of opinion that they may have repreſented the principal mother goddeſſes in general, and thoſe in particular whom the deceaſed whoſe aſhes repoſed in theſe tombs had adopted as their protectors. A great deal has been written on the mother goddeſſes mentioned in the ſepulchral inſcriptions of the Romans: *Diis matribus ..... matronis*, &c. As bas-reliefs repreſenting three females, ſometimes ſtanding and ſometimes ſitting, holding fruits, cones of the fir-tree, and cornucopias, were ſometimes added to ſuch inſcriptions; the mother goddeſſes were at firſt taken for rural deities: but one of theſe monuments was found in the city of Lyons, and on others they are called the mothers of Galicia, the mothers of the Gabii, &c. Their protection therefore was extended to cities and provinces alſo. Keyſler thinks that they were thoſe druid females for whom the Gauls entertained ſo great a veneration; but this opinion is contradicted by monuments of the ſame kind, conſecrated in countries at a very great diſtance from Gaul. Others conſidered the mother goddeſſes as the three Parcæ or Fates; but it is not certain that the Parcæ ever formed a part of all the nations among whom the deities in queſtion were worſhipped: beſides, the latter had their proper denomination, *Fata*. In the laſt place, Barrier propoſed a ſtill more probable opinion reſpecting the mother goddeſſes: he ſuppoſed them to be deities common to ſeveral nations, and that their ſurnames denoted the places where they were worſhipped.

To this may be added, that the women acknowledged them as their ſpecial protectors; for we read on two inſcriptions: *Matronis Gabiabus, ... Junonibus Gabiabus.* Every woman believed that ſhe had a female genius who protected her, and whom ſhe called her Juno. The Greeks, the Cretans in particular, and the Scilians, rendered worſhip to the celeſtial bears under the name of Mothers. But it appears doubtful whether the mother goddeſſes of the Gaul's of Britain, Germany, Spain, &c. were the ſame deities. It is at leaſt certain that in conſequence of migrations, the notions reſpecting theſe goddeſſes, their functions and attributes, had at that time been ſtrangely altered. The figures given by Montfaucon hold children in their arms, like the female figures ſeen on medals of ſome of the empreſſes, with the legend *Juno Lucina*; which confirms the opinion of C. Mongez, that theſe figures repreſent the mother goddeſſes in general, and in particular the Junos and Genii of women.

PALLADIUM, OR NEW SILVER.

We have juſt been favoured with a ſpecimen of a metal, ſaid to be a new one, to which the above name has been given, and which, among others, is ſaid to poſſeſs the following properties:—1. It diſſolves in pure ſpirit of nitre, and makes a dark red ſolution. 2. Green vitriol throws it down in a ſtate of a regulus from this ſolution, as it always does gold from *aqua regia*. 3. If you evaporate the ſolution you get a red calx, that diſſolves in ſpirit of ſalt or other acids. 4. It is thrown down by quickſilver, and by all the metals but gold, platina, and ſilver. 5. Its ſpecific gravity by hammering was only 11·3; but by flatting, as much as 11·8. 6. In a common fire it tarniſhes a little, and turns blue; but comes bright again, like other noble metals, when ſtronger heated. 7. The greateſt heat of a blackſmith's fire would hardly melt it. 8. But if you touch it while hot with a ſmall bit of ſulphur, it runs as eaſily as zinc.—It is ſold only by Mr. Forſter, at No. 26, Gerrard-ſtreet, Soho, London, in ſamples of five ſhillings, half a guinea, and one guinea each.—We have not had time to ſubject the ſpecimen ſent to us to any chemical teſts. When we do, we ſhall report the reſult to our readers. Where or how it has been procured we have not yet learnt.

IMPROVED LAMPS.

Mr. Paul, of Geneva, now in London, has made ſome important improvements in the conſtruction of lamps and reflectors, by which, among other advantages, that light which is uſually thrown up into the atmoſphere and loſt, is reflected to the ſpace intended to be illuminated; by which a much greater effect is produced, and at the ſame time a ſaving in the conſumption of the oil. We underſtand they are ſoon to be employed by Mr. Smethurſt in lighting the ſtreets.

ASTRONOMY.

In our laſt we preſented our readers with tables containing the geocentric motion of the two new planets, Ceres Ferdinandea and Pallas, for the month of April; we now ſubjoin tables of their motion calculated for the month of May 1803.

	Geocentric Motion of Pallas.					Geocentric Motion of Ceres Ferdinandea.				
	Right Aſcenſion.			Declin. North.		Righ Aſcenſion.			Declin. South.	
May 3	18^{h}	55^{m}	15^{s}	18°	38′	19^{h}	11^{m}	54^{s}	24°	6′
6	18	55	4	19	7	19	12	28	24	15
9	18	54	42	19	35	19	12	50	24	25
12	18	54	8	20	1	19	12	59	24	35
15	18	53	22	20	26	19	12	52	24	46
18	18	52	24	20	51	19	12	30	24	57
21	18	51	15	21	14	19	11	56	25	9
24	18	49	56	21	36	19	11	5	25	22
27	18	48	27	21	55	19	10	3	25	35
30	18	46	49	22	14	19	8	44	25	48

GALVANISM.

Profeſſor Aldini, when on his way to France, made the following experiment at Calais:—He extended a wire from the top of the Weſt Jetty to Fort Rouge, diſtant about 160 feet, and which are ſeparated by an arm of the ſea. The extremity of this wire towards the jetty was immerſed in the water, the other extremity was fixed to a maſt, and a portion of the wire was free. A ſecond wire was diſpoſed in ſuch a manner, that its lower extremity was immerſed in the water at the bottom of the maſt, while its upper extremity could be made to communicate with the wire fixed to the maſt. Between the extremity of the wire attached to the maſt and that of the ſecond wire immerſed in the ſea the profeſſor placed an animal, as a prepared frog for example, which being then brought into contact with the two wires, immediately experienced the uſual contractions. The ſea water, therefore, ſerved as a conductor between the other extremities of the wires.

A NEW SPECIES OF INOCULATION.

Accounts have been received from one of the travellers in Africa (Hornemann, we believe), ſtating, that in a diſtrict which he has viſited he found a kind of inoculation practiſed, which anſwers the ſame end reſpecting ſyphilis that the vaccine pock does reſpecting the ſmall-pox—exempting the patient from the poſſibility of infection.

LII. *Letter from Dr.* Olbers *of Bremen to Baron* Von Zach, *on the Stones which have fallen from the Heavens*[*].

YOU know that in a lecture which I delivered in the Muſeum of Bremen in the year 1795, on the ſhower of ſtones, as it is called, which fell at Siena in Italy, I expreſſed the ſame idea which I lately read with much ſatisfaction in a letter of Laplace, in which he ſays: "It is not impoſſible that large maſſes, detached from ſome of the celeſtial bodies, and particularly from the moon, may have ſometimes been projected to the earth." As you require a more circumſtantial illuſtration of this ſubject, I embrace the preſent opportunity of gratifying your wiſhes.

I muſt readily acknowledge, that when I wrote the beforementioned eſſay on the ſhower of ſtones which fell at Siena, I conſidered theſe ſtones to be of a volcanic origin. The ſtones found at Siena fell at the diſtance of ſixty miles from Veſuvius, only eighteen hours after the commencement of the great eruption which deſtroyed the unfortunate Torre del Greco, and of which ſo elegant a deſcription has been given by ſir William Hamilton. I was acquainted with the principles of Zolner and Lichtenberg, according to which theſe ſtones could not be aſcribed to Veſuvius; but to me theſe principles did not appear ſatisfactory. The great velocity which would be neceſſary to make a maſs projected from Veſuvius to proceed to ſo great a diſtance, was not ſufficient to induce me to abandon my opinion. By calculation I found that it was not ſo great as what might be ſuppoſed to be produced by the dreadful convulſion which takes place on ſuch occaſions. What tended chiefly to confirm me in my idea was, that ſir William Hamilton then believed that he had often found at Veſuvius ſtones which had a great reſemblance to thoſe which fell at Siena[†]. I alſo ſuppoſed that Veſuvius might have projected from one of its mouths a half-fuſed maſs at an angle of from forty to fifty degrees, which, like every thing elſe proceeding from the crater, was in a high degree electric; that this maſs had taken its direction towards Siena; that when within a certain diſtance of the earth it had diſcharged itſelf; and that in conſequence of this diſcharge it had burſt, ſo that the fragments fell down in the ſtate of ignited ſtones[*].

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[*] From *Algemeene Konſt en Letter-Bode*, No. 17, 1803.

[†] Stones of the ſame nature, at leaſt as far as the eye can judge of them, are frequently found on Mount Veſuvius; and when I was on the mountain lately I ſearched for ſuch ſtones near the new mouths; but as the ſoil around them has been covered with a thick bed of aſhes, whatever was thrown up during the force of the eruption lies buried under thoſe aſhes, &c.—Philoſophical Tranſactions 1795, p. 104.

On the other hand, however, it is known that no ſuch ſtones are found on Mount Veſuvius. It has been lately ſhown by the mineralogical deſcription of Count de Bournon, and the chemical analyſis of Mr. Howard, that the ſtones found at Siena have a perfect reſemblance to all the other ſtones which have been ſeen to fall from the heavens, and which certainly cannot be productions of our earth[†]. My former explanation and conjectures fall therefore to the ground, and we muſt claſs the ſtones of Siena among thoſe which are formed by nature in ſome mode with which we are unacquainted, and which burſting always with the appearance of light fall down to the earth.

Since the time when naturaliſts firſt ventured, or were obliged, to believe in the falling of ſuch ſtones, it has been found that this phænomenon is not ſo uncommon as was ſuppoſed. During the laſt century, fourteen or fifteen inſtances at leaſt can be quoted.

When I erroneouſly aſcribed in my lectures the ſtones found at Siena to an eruption of Veſuvius, the nature of other ſtones fallen from the heavens, which certainly were not of volcanic origin, had given occaſion to the queſtion, whether it was impoſſible that heavy maſſes could be projected to the earth from other heavenly bodies, and particularly from the moon? The celebrated ſtone which fell near Argos Potamos, in the year 462 before Chriſt[‡], induced me in particular to undertake an examination of this queſtion; and on this occaſion my learned friend, Mr. Bredenkamp, clergyman of Bremen, was ſo kind as to collect for me all the paſſages in antient authors which make mention of this ſtone. In regard to the reſt, the reader may conſult what Struyk, Pingré[§] and Chladni[||] have ſaid on the ſubject.

It appears from calculation, that when a greater vertical velocity than 34435·7 Paris feet in a ſecond can be given to a heavy body projected from the earth's ſurface, ſuppoſing the atmoſphere to oppoſe no reſiſtance, this body would not fall back to the earth, but would continue to move from it *in infinitum*. A cannon bullet ſometimes acquires a velocity of 1800 or 2000 feet in a ſecond*; and heavy maſſes are projected from the volcanoes of our earth with a velocity four or five times as great. This velocity, therefore, is far greater than that of 35,000 feet per ſecond. But even if it were poſſible, by the help of chemiſtry and mechanics, to give a ball a velocity of 35,000 feet, or even more, per ſecond, the ſtrong reſiſtance oppoſed by the air, which always increaſes at the leaſt as the ſquare of the velocity, would tend very much to deſtroy it. We may therefore conſider it as almoſt impoſſible that heavy bodies can ever be entirely projected from the earth. I conſider it altogether unneceſſary to take into account in this calculation the influence which the attractive force of the moon would have on the motion of a body projected in this manner.

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[*] The abbé Tata actually ſaw a large fire-ball thrown up from Veſuvius, which when it had reached a certain diſtance burſt, and the abbé then heard a noiſe like that of ſtones falling down.

[†] Howard's Experiments and Obſervations on certain Stony and Metallic Subſtances, &c —Philoſophical Tranſactions 1802.

[‡] Plin. Hiſt. Nat. lib. ii. cap. 58. [§] Cometographie, p. 256.

[||] Philoſophical Magazine, vol. ii.

But the caſe is entirely different if we ſubject to calculation the motion of a heavy body projected from the ſurface of the moon. The moon is a great deal ſmaller than the earth, poſſeſſes a much weaker power of attraction, and at the ſame time its atmoſphere is of ſo ſmall extent, and ſo rare, that it could oppoſe very little reſiſtance to a heavy body in a ſtate of motion. It is therefore poſſible that a heavy body might be projected from the moon with ſuch a velocity as to carryit to a diſtance where it would be attracted with greater force by the earth.

It is found by calculation that 7780 feet per ſecond is the minimum of velocity with which a body projected from the moon would require to fall upon the earth. It appears, therefore, that if heavy bodies were projected from the moon with a vertical velocity of 7800 or 8000 feet under certain circumſtances, ſome of theſe bodies would reach the earth and fall down upon it. Such a velocity appears to me to be very poſſible. The ſurface of the moon, by the new craters which ariſe, ſhows evident traces of violent eruptions, by which even as great or greater velocity might be communicated to the bodies thrown up by them.

It appears then, that it is not altogether impoſſible that the ſtones or maſſes which have been ſeen to fall from the heavens, and which, though entirely different from all the mineral bodies of our earth, have a great reſemblance to each other, may have fallen from the moon. It is in their great ſimilarity and correſpondence that the grounds of this opinion are to be found; for the ſimilarity of their external appearance, and of their component parts, evidently ſhow that they have all had the ſame origin. If we admit, with Halley and Chladni, that, beſides the large celeſtial bodies, there are in boundleſs ſpace an infinite number of ſmall maſſes which move about till they approach ſo near a planet as to enter its atmoſphere, when they take fire, burſt, and fall down upon it; we can explain why all theſe maſſes diſperſed throughout infinite ſpace conſiſt of iron, nickel, ſiliceous earth, and talc earth, which, according to the ingenious experiments of Mr. Howard, are the component parts of all the maſſes which have fallen from the heavens.

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* Mém. de l'Acad. Royale des Sciences 1769, p. 247.

But, on the other hand, great difficulties occur, if theſe ſtones which have fallen from the heavens be ſeriouſly conſidered as bodies projected from the moon. In conſequence of the motion of the moon, bodies projected from it acquire, beſides their projectile velocity, that velocity which the moon has in the direction of the tangents to her orbit. If we therefore take this velocity into the account, it will appear, that heavy bodies projected from the moon with a velocity of 8000 feet or more per ſecond, as ſoon as they have got to ſuch a diſtance from the moon as to experience leſs attraction from it than from the earth, would deſcribe around the earth a conic ſection paſſing more or leſs through the moon. Theſe conic ſections, according to the difference of the direction and velocity, may be either hyperbolas or ellipſes *. In order that the body ſhould fall to the earth, the ellipſe it deſcribes ought to be of ſuch a nature that its perigeum falls within the body of the earth, or at leaſt within its atmoſphere. The moon, therefore, muſt perceptibly decreaſe in ſize, as it would be neceſſary that it ſhould throw out a great number of maſſes to make ſome of them reach the earth. For this purpoſe alſo a very limited direction and velocity of the heavy bodies would be required. And would not an infinite number of ſuch ſmall fragments move around our earth as ſatellites? Would they not be viſible through our beſt teleſcopes, as we know that fire-balls ſometimes are of very great ſize, and as the obſervation of Ceres and Pallas has ſhown that bodies of a very ſmall diameter become viſible to us when illuminated by the light of the ſun; or are the ſhooting ſtars, which ſeem to have a coſmical origin, ſuch ſmall ſatellites of our earth? Perhaps we ought to claſs among theſe alſo the ſmall pale luminous ſpot which Schröeter ſaw paſs over the field of his teleſcope. Theſe objections, and others which

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* A prodigious velocity would be required to make a body projected from the moon to fall to the earth in an hyperbola.

ariſe from an examination of the circumſtances of their falling, ſeem to me to be of great importance, and difficult to be removed.

I am therefore far from aſſerting that theſe ſtones which have fallen to the earth are to be conſidered as maſſes projected from the moon; nor does Laplace make any ſuch aſſertion. His object, as well as mine, is to ſhow philoſophers engaged in reſearches on this ſubject the poſſibility of theſe maſſes having a *ſelenitic* origin. It is much to be wiſhed that the ingenious Chladni would favour us with a new edition of his celebrated eſſay on the maſs of iron found in Siberia [*], as he no doubt would be able, from Benzenberg and Brandes' Obſervations on falling Stars, Howard's Chemical Examination, and from various other documents, to make conſiderable additions to it.

LIII. *On the Nervous Power, and its Mode of acting* [†].

It has been generally ſuppoſed by phyſiologiſts that the power by which ſenſations were tranſmitted to the ſenſorium, was likewiſe the medium by which mental impreſſions were communicated to the body. But this opinion is totally inconſiſtent with that moſt frequent obſervation, that very often every poſſible ſuſceptibility of ſenſation is loſt in a whole limb, which nevertheleſs retains voluntary motion. This appears moſt ſtrikingly in the St. Vitus's dance, which deprives the afflicted parts of every ſuſceptibility of ſenſation without deſtroying voluntary motion. And, on the contrary, there are caſes where the functions of the ſenſes remain unimpaired, and are ſometimes increaſed, while the muſcular motion is completely loſt. In order to explain this phænomenon two kinds of nerves have been hypothetically adopted, nerves of ſenſation and nerves of motion; but without rendering the explanation more ſatisfactory. Certain it is, that the action of mental irritation upon the body, and the communication of the ſenſations with the ſenſorium, are effects of two different powers, which cannot poſſibly have their ſeats in one and the ſame organ.

That which is underſtood by the word *nerves* conſiſts properly of two entirely different parts, the *medullary ſubſtance* of the nerves, and the *cellular texture*; the latter being compoſed of cylindric tubes, containing the former in their cavity: and in theſe tubes, theſe ſheaths of the nerves, is the ſeat of the power which continues the irritation of the mind into the body. The medullary ſubſtance of the nerves, on the other hand, poſſeſſes no other power than that of exciting in the ſenſorium ſenſations correſponding with the impreſſions received by the ſenſes. The cellular texture of the nerves poſſeſſes a power of contraction, elaſticity; but the medullary ſubſtance of the nerves poſſeſſes only the ſimple ſenſitive power of the nerves. From this, the above-mentioned phænomena, eſpecially thoſe obſervable in St. Vitus's dance, are eaſily explained. The ſenſitive power of the nerves diſappears, becauſe its organ, the medullary ſubſtance, is deſtroyed or wounded. The two following arguments confirm the correctneſs of this opinion:—1. According to Aaneman's numerous experiments on the regeneration of the nerves, each end of a nerve which has been cut in two forms itſelf into a ſcirrhus, and both afterwards join again in the cellular texture; but the lower end of the nerve thus interſected always loſes its ſenſitive power, whereas it recovers in a few months the power of motion when both the ends are joined again by a ſufficient quantity of the cellular ſubſtance. 2. Arſenic and all ſorts of mercurial preparations, when immediately applied to the brain, exhibit not the leaſt immediate effect upon it. But thoſe very minerals, when applied in any other part, excite the moſt violent convulſions of the whole body; and it is well known, from other circumſtances, that theſe produce the greateſt irritation upon the cellular texture. Without entering into any further elucidation, every one will eaſily conceive the great importance of theſe ideas in the explanation of many phyſiological and pathological phænomena.

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[*] Uber den urſprung der von Pallas gefundenen und anderer ihr ahnlicher Eiſen maſſen, und über einige damit in verbindung Stehende Natur-erſcheinungen; Von E. T. T. Chladni. Riga 1794. 4to.

[†] From *Reil's Archiv für die Phyſiologie*, vol. i.

LIV. *Letter from Dr.* Barton *to Profeſſor* Zimmermann, *on the faſcinating Faculty which has been aſcribed to the Rattleſnake, and other American Serpents.*

[Concluded from p. 202.]

AFTER quoting Mr. Michaëlis's obſervations, which I have already noticed, Mr. Blumenbach has the following words:—"I would, however, add another mode of explanation from Dr. Barton's work itſelf, where he aſſures us that the reſult of his inquiries, whether the rattleſnake creeps up trees or not, induces him to believe the latter to be the caſe. The rattleſnake," Mr. Blumenbach continues, " is alſo one of the lazieſt of all the ſerpent tribe; under theſe circumſtances it ſeems very natural that ſuch a lazy animal ſhould be endowed with the faſcinating power of bringing down from trees ſmall animals, which otherwiſe would have nothing to fear from a ſnake that cannot creep upwards."

It is true, as I have aſſerted, that the rattleſnake is one of the moſt ſluggiſh ſpecies of ſerpents; and further inquiries have confirmed me in my former opinion, that this ſerpent does not climb up trees. But if Mr. Blumenbach had recollected what I have ſo particularly ſaid on the ſubject of the nidification of our birds *, he would not, I preſume, have imagined that it is at all neceſſary that the ſerpent ſhould be endowed with " a faſcinating power of bringing down from trees ſmall animals." I have ſhown that the rattleſnake feeds upon bull-frogs, which are never found upon trees; upon the ground-ſquirrel, which is moſt commonly found upon the ground; and upon the fringilla erythrophthalma, or ground-robin, a ſpecies of finch, which receives its common Engliſh name from its being ſo generally ſeen upon the ground †. I may now add, that this reptile feeds upon young rabbits (lepus americanus), wild mice of different kinds, moles (ſorex aquaticus), and many other ſmall animals, which it cannot have much trouble in obtaining without its poſſeſſing the power of charming.

But although the rattleſnake is, in reality, a ſluggiſh reptile, it is nevertheleſs an animal of more activity than Mr. Blumenbach ſeems to imagine. I believe it is true, as the old Indian ‡ informed Mr. Heckewelder, that in the ſpring ſeaſon theſe reptiles make conſiderable journeys from their dens in ſearch of food. I know it to be a fact, that they ſwim acroſs ſtreams of water, and even over large rivers. It is obſerved by the hunters and others, that the rattleſnake ſwims faſter than it moves on the land. Indeed, it ſeems to run upon the ſurface of the water. It is very probable that in theſe watery peregrinations it may occaſionally have opportunities of catching certain kinds of fiſh. But this is a mere conjecture, which at preſent I am incapable of ſupporting by any decided fact.

I have obſerved in my memoir, that " among the Indians of South America I do not find any traces of the notion that ſerpents can faſcinate other animals." Mr. Blumenbach,

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* A Memoir, &c. pages 50, 51, 52, 53.
† Ibid. pages 63 and 64.
‡ See page 31.

however, informs us, that Dobriſhoffer aſſerts, in his Hiſtory of the Abipons, "that all the Spaniards and Indians in that part of Paraguay unanimouſly aſcribe a like property to the ſnake called *ampalabas*." I alſo find that Dr. Bancroft has mentioned the faſcinating power of a large but innocuous ſpecies of ſerpent which inhabits Guiana [*].

I was ignorant of theſe facts when I printed my memoir; and now that they are known to me, they do not appear to be of much conſequence. They certainly do not prove that ſerpents are endowed with the power of faſcinating. It is not by any means aſcertained that the Abipons have not derived the notion from the Europeans, with whom they have been long acquainted.

Since the publication of my memoir I have been able to make a more complete collection of the ſentiments of the North American Indians on the ſubject. I am led to believe, that it is far from being the general opinion among theſe people that the rattleſnake is endowed with the faculty of charming. I cannot in any other way ſo ſtrikingly ſhow the notions of the Indians on the ſubject, as by extracting part of a very curious manuſcript which I received from my friend Mr. John Heckewelder:—"Having queſtioned Indians a number of times with reſpect to ſnakes having the power of charming, and always being anſwered in the negative, I was at length deſired," ſays Mr. Heckewelder, "to give the reaſon the white people had for believing ſuch a thing; which not being ſatisfactory, Pemaholend [†] declared: 'The rattleſnake obtains its food merely by ſlyneſs, and a perſevering patience. It knoweth as well where to watch for its prey as a cat does, and ſucceeds as well. It has, and retains, its hunting grounds. In ſpring, when the warm weather ſets in, and the woods ſeem alive with the ſmaller animals, it leaves its den. It will croſs a river and go a mile and further from its den to the place it intends to ſpend the ſummer; and in fall, when all the young animals bred this ſeaſon are become ſtrong and active, ſo that they are no more ſo eaſily overtaken or caught, it directs its courſe back again, to its den, the ſame as a hunter does to his camp.

"'The white people,' continued Pemaholend, 'probably have taken the idea of this ſnake having the power of charming from a tradition of ours (the Indians), which our forefathers have handed down to us, from many hundred years back, and long before ever the white people came into this country. Then (they tell us) there *was* ſuch a ſnake, and a rattleſnake too; but then there was only *this one* ſnake which had this power, and he was afterwards deſtroyed; and ſince that time it hath never been ſaid that any other of the kind had made its appearance.'"

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[*] An Eſſay on the Natural Hiſtory of Guiana, &c. p. 205: London 1769. Mr. Stedman, a late writer, poſitively denies the exiſtence of this faſcinating power in the aboma, the ſerpent mentioned by Dr. Bancroft.

[†] An aged and much reſpected Delaware Indian.

The whole of this tradition, as related by Pemaholend, is in my poſſeſſion. It is a very curious piece of American mythology, and will be publiſhed at large in another place, perhaps in my Fragments of the Natural Hiſtory of Pennſylvania. It is a new proof of my aſſertion, "that the mythology, or ſuperſtitious religion, of the Americans is a fragment of that mythology whoſe range in Aſia and in Africa has been ſo extenſive *." But this tradition is intereſting in the diſcuſſion of the queſtion in which I am now engaged. It plainly ſhows that the Indians do not in general ſuppoſe that the rattleſnake is gifted with the faculty of charming; and it renders it ſtill more doubtful that the whites derived this notion from the Indians.

The Indians are ſo far from believing in the exiſtence of this faculty in ſnakes, that the worthy gentleman from whom I received the tradition which I have juſt mentioned, aſſures me that he would be unwilling in future to trouble them with any further inquiries on the ſubject, as the ſure reward of the pains of inquiring is a laugh at the eaſy credulity of the Whites. I may here add, what I have mentioned in my memoir, that Mr. William Bartram never underſtood that the nations of Indians among whom he travelled had any idea of the faſcinating power of ſnakes †. Among other Indians Mr. Bartram viſited, with the zeal and knowledge of a naturaliſt, the nations of Eaſt and Weſt Florida. As this ingenious and amiable gentleman believes that ſerpents can charm other animals, there will be no ſuſpicion, among candid people, that he has concealed the opinions of the Indians on the ſubject.

I think I have now conſidered every eſſential part of Mr. Blumenbach's remarks on my memoir. I have little doubt that this illuſtrious profeſſor, who is not leſs candid than he is learned and ingenious, will give to the new facts which I have adduced all the conſideration which they merit. What is the proportion of that conſideration I muſt leave it with you and other able judges to decide. In the meanwhile I am not a little flattered that one of my earlieſt eſſays in natural hiſtory has ſolicited ſo much of the attention of the philoſophers of Europe.

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* A Memoir, &c. p. 16—Note.   † Ibid. p. 14.

You inform me, that my explanation of the ſuppoſed faſcinating faculty of ſerpents is adopted by many of your naturaliſts. This is pleaſing to me. Indeed, ſuch is our ſelfiſhneſs, that I fear I ſhould have felt ſomewhat gratified to learn that the theory had been adopted, though I myſelf had relinquiſhed it. But I aſſure you that, as yet, I have ſeen no cauſe to relinquiſh it. On the contrary, I poſſeſs a great body of additional facts in ſupport of it. Theſe facts will be carefully adduced in that part of my Fragments which is intended to compriſe the hiſtory of the *amphibia* of Pennſylvania.

In my native country the explanation which I have offered has been adopted by many perſons. But there are others who ſtill believe in the exiſtence of a true faſcinating faculty in the rattleſnake and other ſerpents. What change time and further attention to the ſubject may accompliſh in them, I know not. But why ſhould we expect to make all philoſophers converts to our opinions? Almoſt every phænomenon, almoſt every fact in nature, ſeems to admit of an explanation upon more than one principle. The ſtream of inquiry is often diverted by trifling circumſtances into very oppoſite directions. Prejudices, or, to name them by a milder phraſe, the earlier biaſſes of our minds, frequently detain us, in the inveſtigations of ſcience, in a long and pleaſing reign of tyranny. Our firſt love is ſaid to be the ſtrongeſt. Our firſt principles in ſcience, in religion, and in politics, are often adhered to with the extreme of pertinacity. He who, turned of fifty years of age, relinquiſhes a favourite error, has infinitely more merit than the world may be willing to allow him.

I beg you, ſir, to make what uſe of this letter you may think proper. If it ſhall add any thing to the ſtock of your knowledge on the ſubject, or if it ſhall ſerve to amuſe you in an hour of leiſure, I ſhall feel highly gratified.

Be aſſured that I am, with very great reſpect,
Dear ſir, your faithful and obliged
ſervant and friend,
BENJAMIN SMITH BARTON.

POSTSCRIPT.

I BEGAN this long letter on the very day that I firſt ſaw Mr. Blumenbach's remarks in Tilloch's Magazine. I had not finiſhed it before I received a copy of the original publication of the profeſſor. In the concluſion I find he urges me to extend my inquiries into the real uſe of the *crepitaculum caudæ*, or rattle of the crotalus. This is certainly a queſtion worthy of the attention of American naturaliſts, who enjoy the beſt opportunities of inveſtigating the ſubject. I am, indeed, inclined to think that we are not yet acquainted with the real or excluſive uſe of the rattle of the crotalus. That it was given to this reptile to warn man and other animals of a dangerous enemy, does not appear a ſufficient explanation of the uſe of the organ. Many ſerpents whoſe poiſon is not leſs deleterious than that of the rattleſnake, are entirely deſtitute of any apparatus like the crepitaculum of this reptile. Beſides, we have ſeen that, when moſt intent upon obtaining his prey, the rattleſnake keeps his rattle ſtill. This would ſeem to ſhow that it was not deſigned to terrify its enemies. Indeed, it is highly probable that one reaſon why the rattleſnake ſo frequently ſucceeds in capturing animals is, becauſe he makes no noiſe, and therefore ſurpriſes his prey.

I do not doubt that the crepitaculum of the rattleſnake is an organ of very eſſential importance: but I muſt ſay that it has always been deemed of more importance than it can yet be ſhown to be. Thus it has been ſaid to give an unerring indication of the age of the reptile. In this reſpect it is a leſs ſure criterion than has been generally imagined. It is a fact which, I believe, has entirely eſcaped the notice of all the writers on the natural hiſtory of this reptile, that the rattles are formed before the excluſion of the young ones from the uterus. Towards the latter end of Auguſt a number of female rattleſnakes were opened. The young animals were about five inches long, and about the thickneſs of a ſmall-ſized gooſe-quill: the ſcales were formed ſo as to be viſible to the naked eye; the head very large, and the fangs, though of a ſomewhat gelatinous conſiſtence, were ſhaped, and diſtinctly viſible. The rattles were ſo far formed that three bells could be plainly diſcerned; and this was the caſe in more than ſixty inſtances. Hence it is evident, that when they are excluded from the womb the young animals have at leaſt three rattles. After this, I believe, they generally acquire two bells every year. Yet, in one inſtance, a rattleſnake* has been known to acquire four bells in the term of a year. I may add, that the bells are liable to be loſt: they are ſometimes broken, and it is not improbable that they wear out.

I am ſometimes almoſt inclined to think, with your learned and eloquent countryman Mr. Herder, that "natural hiſtory has reaped no advantage from the philoſophy of final cauſes." And yet without an inquiry into the ultimate intentions of nature, what is natural hiſtory? A barren waſte of unconnected facts. Speculations, which too often, indeed, proceed from indolent philoſophers, are neceſſary to render natural hiſtory an agreeable and even uſeful ſcience. We are, moreover, ſometimes able to diſcover the final cauſe or intention of nature in her operations. And as to the *uſus partium*, every philoſopher muſt confeſs that this is a queſtion which ought never to be neglected. It is a kind of ſoul to the ſcience of anatomy. It is exceedingly to be regretted, however, that we are ſo often at a loſs to determine the uſes of parts even where the forms and ſtructure of theſe parts are well known to us.—But let us not deſpair. Nature intends to reveal to us many things (precious in their kind), which are now entirely unknown to us. Phyſiology will, in time, aſſume one of the higheſt ſtations in the range of ſure as well as ſplendid ſciences.

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* In the Muſeum of Mr. Peale.

LV. *A Survey and Report of the Coaſts and Central Highlands of Scotland; made by the Command of the Right Honourable the Lords Commiſſioners of his Majeſty's Treaſury in the Autumn of* 1802. *By* THOMAS TELFORD, *Civil Engineer, Edinburgh, F. R. S.*

MY LORDS,

IN reporting upon the ſurvey I made in Scotland, in obedience to the inſtructions I had the honour of receiving from your lordſhips, dated the 1ſt of July 1802, I find the buſineſs may be moſt conveniently arranged under the following heads:

I. What regards rendering the intercourſe of the country more perfect, by means of bridges and roads.

II. Aſcertaining various circumſtances relative to the Caledonian canal, eſpecially with regard to the ſupplies of water on the ſummit level, and the beſt communications from this canal to the fiſhing locks at the back of the Iſle of Skye.

III. The means of promoting the fiſheries on the eaſt and weſt coaſts.

IV. The cauſes of emigration, and the means of preventing it.

V. Improving the means of intercourſe between Great Britain and the northern parts of Ireland; particularly as to the bridges and roads between Carliſle and Port Patrick, and alſo the harbour of Port Patrick.

Under each of thoſe heads are comprehended ſubjects highly deſerving the attention of government; the more they are inveſtigated, the more important they will appear, and the public will become more fully convinced, that the general intereſts of the Britiſh empire are extenſively connected with the ſeveral improvements which are mentioned in your lordſhips' inſtructions.

Of Bridges and Roads.

The obſtacles which at preſent obſtruct the communications in the north of Scotland are numerous and well known, not only to the inhabitants, but to every perſon who has travelled through, or even inquired into the ſtate of, the country.

Previous to the year 1742, the roads were merely the tracks of black cattle and horſes, interſected by numerous rapid ſtreams, which being frequently ſwoln into torrents by heavy rains, rendered them dangerous or impaſſable. The military roads, which were formed about this time, having been laid out with other views than promoting commerce and induſtry, are generally in ſuch directions, and ſo inconveniently ſteep as to be nearly unfit for the purpoſes of civil life; and in thoſe parts where they are tolerably acceſſible, or where roads have ſince been formed by the inhabitants, the uſe of them is very much circumſcribed from the want of bridges over ſome of the principal rivers.

The general connections of the country may be ſtated as leading from Edinburgh to the north and north-weſt counties, by means of one road through the Highlands, and by another along the eaſt coaſt and ſouth ſhore of the Murray Frith to the town of Inverneſs, and from thence through Beauley and Dingwall, to Tain in Roſs-ſhire. From Glaſgow and Greenock, the communications by land are through Argyleſhire, to the weſtern parts of Inverneſs and Roſs-ſhire, and to the ſhores oppoſite the whole of the Hebrides. There is an important communication from Inverneſs weſtward, acroſs the country to Fort Auguſtus and Fort William, and from Fort Auguſtus there are juſt the veſtiges remaining of what was once a military road to Bernera, oppoſite the back of the Iſle of Skye.

In conſidering theſe lines of roads, it appears moſt regular to begin on the borders of the improved country, and near to the ſeats of commerce and induſtry. In proceeding from Edinburgh northwards, by the eaſt coaſt and Moray Frith to Inverneſs, or through the central Highlands to the ſame place, and from thence to Tain on the Dornoch Frith in Roſs-ſhire, we find the communications intercepted, and we learn that accidents frequently happen from the want of a bridge over the river Tay, at Dunkeld, in Perthſhire; another over the river Spey, at Fochabers, in Bamffshire; a third over the river Beauley, in Invernefs-ſhire; and a fourth over the river Conon, near Dingwall, in Roſs-ſhire. Theſe rivers are large, and at preſent are all croſſed by means of ferry-boats.

At Dunkeld the river Tay is deep and broad, and there is reaſon to expect the foundations will be expenſive, the bed of the river at and for a great diſtance above and below the town being compoſed of alluvial earth and gravel. The beſt ſituation for a bridge is a little below the lower ferry; at this place there is a ſtraight reach of the river, and in winter the ice is broken by paſſing over a ford nearly oppoſite the mouth of the river Bran. This ſituation will alſo connect with the improved lines of road which are propoſed to be made on each ſide of the river. It is probable that a flat rubble ſtone will be got near the ſlate quarries, which are within a ſhort diſtance of the place. Freeſtone of a durable quality is to be had near Dundee: it may be brought by water carriage to Perth, and from thence by land to Dunkeld.

Under all theſe circumſtances the expenſe would be conſiderable; and, taking into account the uncertainty of the foundations, the amount cannot be ſtated at leſs than 15,000 l.

The two ferries which are now at Dunkeld belong to his grace the duke of Athol; he has authorized me to ſtate, that if government will defray one-half the expenſe of a bridge, he will advance the other half; that he will give up his intereſt in the ferries, if in lieu thereof a reaſonable toll be put upon the bridge, in order to liquidate the capital advanced by the duke: after this has been accompliſhed, with a ſmall ſurplus to anſwer the repairs, the bridge ever after to remain free of toll.

This ſeems a very reaſonable and juſt mode of defraying the expenſe: the ſafety and accommodation would be ſo great, that no perſon travelling that road could object to paying the ſame toll for a ſafe and convenient bridge which at preſent is paid for a dangerous and inconvenient ferry-boat, eſpecially when there will be a certain proſpect of having a bridge free of toll in a few years.

This bridge is of the firſt importance to the Central Highlands; it would accommodate a great diſtrict of that country, and at the ſame time facilitate the communication with the North Highlands.

The river Spey is alſo rapid and deep, being the drain of a great extent of mountainous country, where there is much rain. It is of courſe a very dangerous ferry. This ferry is on the great coaſt road eaſtward from Inverneſs and Fort George, through the towns and cultivated country in Murray and Banff-ſhires, from whence it paſſes through Fraſerburgh and Peterhead to Aberdeen.

Another branch of road alſo ſtrikes off at Fochabers, on the Spey, and paſſes by Huntly and Inverury to Aberdeen.

The neceſſity of having a bridge over the Spey at this place became ſo urgent, that his grace the duke of Gordon began a ſubſcription in the adjacent country in order to raiſe a part of the money neceſſary to defray the expenſe of a bridge; his grace ſet a liberal example, and it has been followed by moſt of the gentlemen in that part of the country. In conſequence of this exertion, a contract has been entered into, and ſome ſteps have been taken towards carrying this uſeful work into execution; but unleſs government will grant an aid equal to one-half the expenſe, the works muſt ſtill be left unfiniſhed, and unfit for the purpoſes intended.

The ſituation fixed upon for this bridge is adjacent to the ferry and town of Fochabers. At this place a rock paſſes quite through the river, at about from eight to ten feet below the ſurface of ordinary low water; and, as far as I could learn, it is the only place, unleſs at a great diſtance up the country, where the rock paſſes quite through the river. On theſe accounts it was judicious to prefer it for the ſituation of the bridge; and it was fortunate this happened preciſely in the line of the preſent road.

As one-half of the expenſe will be raiſed by ſubſcription in the neighbourhood, if government will defray the other half, I underſtand it is propoſed that the bridge ſhall be free of toll. The expenſe cannot be ſtated at leſs than 12,000l.

I have made plans and ſections for both thoſe bridges. On account of their being ſo nearly connected with the ſeats of the two before-mentioned noblemen, I have introduced more decorations than what are abſolutely neceſſary for common road bridges. This extra expenſe will not amount to much; but whatever it is, I propoſe it ſhall be defrayed by thoſe noblemen.

I laſt year produced plans and eſtimates for the bridges over the rivers Beauley and Conon: I have again examined the propoſed ſituations, and perceive that, on account of the uncertainty of the foundations, and the alteration which muſt be made in ſome parts of the beds of the rivers, it will be neceſſary to take the expenſes of each bridge at 5000l. inſtead of 4000l.

Theſe two bridges are greatly wanted, in order to facilitate the communications with Roſs-ſhire, Sutherland, and Caithneſs; they are equally ſo for the north-weſt coaſt of the main land, and the northern parts of the Hebrides; they are the roots from which a great number of branches of roads are to proceed, which are neceſſary for the improvement of the country and the extenſion of the fiſheries.

Before entering upon the conſideration of the roads to the north of the line of the Caledonian canal, it is neceſſary to ſpeak of the communications from Glaſgow and Greenock by Argyleſhire to Fort William. From Glaſgow there is already a road, which paſſes through Dumbarton up the weſt ſide of Loch Lomond, by the upper ends of Loch Long and Loch Fyne to Inverary. From Greenock, by croſſing the frith of the Clyde, there is a road which paſſes up the eaſt ſide of Gare Loch and Loch Long, and joins the road to Inverary at the top of Loch Long. From Inverary there is one road which paſſes the upper end of Loch Awe, and another which croſſes that loch by a ferry at Port Sonachan; and theſe roads unite at Bunawe, on the banks of Loch Etive. From Bunawe the road is already made to Oban, and a branch croſſes Loch Etive and paſſes along the ſouth ſide of the Linnhe Loch and Loch Eil to Fort William. Between Bunawe and Fort William there are three ferries, over arms of the ſea, which running many miles into the land, cannot well be avoided: in other reſpects, the communication from Glaſgow and Grenock, thus far, is tolerably good. The Highland Society, in the excellent report to which I ſhall frequently have occaſion to refer, and which may be found in the appendix*, have pointed out a new line of road from the north ſide of the Frith of Clyde, nearly oppoſite to Greenock, to be carried to the Bay of Strachan upon Loch Fyne: this would be a very direct line from Greenock to Inverary, but it would be ſubject to two ferries, and it ſeems doubtful whether this inconvenience would not overbalance the additional diſtance round the upper end of the lochs; at leaſt, as there is already a very good communication by this road, it ſeems moſt prudent to attend to the other more neceſſary portions of road before this is undertaken.

From Fort William it will be very adviſeable to improve and extend the road which paſſes along the north ſide of the portion of Loch Eil which turns from Fort William to the weſt, and to carry it from thence acroſs the upper end of Loch Shiel, through Arraſaig to Morer, as deſcribed in the report of the Highland Society. This would open a very direct communication from the Clyde to the fiſhing lochs at the back of Skye, to Skye itſelf, and to the iſlands of Egg, Rum, Muck, Barra, and South Uiſt. This would prove of great importance to the fiſheries on account of facilitating intelligence, which is one of the moſt neceſſary ſteps to promote the ſucceſs of this buſineſs.

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* This ſhall appear in our next Number.

In the year 1796, Mr. Brown, of Elgin, made a ſurvey and eſtimate of this road from Fort William to Morer, and ſtated the expenſe at 6456 l.; ſince that time I underſtand that about 1500 l. has been laid out upon it, ſo that 4000 l., and perhaps ſomewhat more, is ſtill required to render it perfect.

With regard to the improvement of the roads which lead from Fort William eaſt to Inverneſs, there is one principle which in future ought never to be loſt ſight of, which is, to make the new roads as near to the banks of the rivers and lochs as is practicable at a reaſonable expenſe. If the Caledonian canal is executed, it is more than probable that many improvements will be pointed out in the courſe of carrying on that great work; and it would be imprudent to decide raſhly in a matter which is ſo much connected with this national object.

We now come to conſider the communications of the countries which lie to the north and weſt of the track of the Caledonian canal. From this valley, which runs from Fort William to Inverneſs, it is of great importance that there ſhould be lines of communication with the Iſle of Skye and the fiſhing lochs which lie at the back of it; theſe lines of road are not only neceſſary for promoting the fiſheries, but are urgently called for by the ſituation of the interior parts of the country, where there are many fertile valleys which hitherto have remained nearly inacceſſible: it is incalculable the loſs which the public has ſuſtained, and are about to ſuffer, from the want of roads in this country.

From the bridges of Beauley and Conon, lines of road, from the ſame important cauſes, are wanted in ſeveral directions to the weſt coaſt, and through the whole of the counties of Roſs-ſhire, Sutherland, and Caithneſs. The outlines of thoſe roads are well deſcribed in the report of the Highland Society; and although there is a diverſity of opinion as to the comparative importance of ſome of them, as well as of thoſe to Skye, yet it is evident that they are all neceſſary for the welfare of the country.

The empire at large being deeply intereſted in thoſe improvements, as it regards promoting the fiſheries and increaſing

creaſing the revenue and population of the kingdom, juſtifies government in granting aid towards making roads and bridges in a country which muſt otherwiſe remain, perhaps for ages to come, thus imperfectly connected. Yet as the land-owners in thoſe extenſive diſtricts through which the roads would paſs, and indeed the whole of the adjoining diſtricts of country, would enjoy improved cultivation and paſturage, increaſed incomes, and all the bleſſings which are to be derived from a facility of intercourſe, it is certainly juſt that they ſhould contribute a ſhare with government in the expenſe of acquiring thoſe advantages. They might be enabled to do this without inconvenience to the preſent poſſeſſors, by being empowered by an act of parliament to ſell land, or borrow money upon the land, to the amount of their proportion of the expenſe to be incurred by the roads and bridges. This is reaſonable, becauſe the money ſo raiſed would be applied to improve the remainder of the entailed eſtate, which would be enhanced in value though ſomewhat diminiſhed in extent.

The expenſe of the lines of road which were ſurveyed and eſtimated by Mr. Brown, comprehending the whole of the beforementioned counties, and nearly 1000 miles in length, did not amount to 150,000l. If, therefore, we admit an equal quantity of lines of road, to be undertaken and completed in the courſe of three years, would require an annual ſupply of 50,000l.; and ſuppoſing this to be raiſed in equal moieties by government and the land-owners, it would amount to 25,000l. each: but as it is not likely the whole could be brought ſo immediately into operation, it may, with more probability, be expected that ſix years would be taken up in executing all the lines, which would reduce the annual ſupply to 25,000l. or 12,500l. from each party. But this is excluſive of the four great bridges, the total expenſe of which is reckoned at 37,000l.; if they were undertaken by proper perſons they might be executed in three years, which would require an annual ſupply of 12,333l. 6s. 8d.; and if government was to grant one-half,

It would be annually -	£.6,166	13	4
To which add the former	12,500	0	0
	£.18,666	13	4

Speaking therefore generally, if government was diſpoſed to encourage theſe plans of improvement, and would agree to advance 20,000l. in each of the firſt three years, and 12,000l. in each of the laſt three years, it would then remain with the land-owners of the diſtricts of country through which the lines were to be carried, to come forward with their ſurveys and eſtimates, and ſubſcriptions to one-half the amount of the expenſe, proving at the ſame time, to the ſatisfaction of government, that the propoſed lines would be of public as well as private utility. It would be neceſſary alſo to provide that the works ſhould be ſubſtantially executed, and that the land-owners or others intereſted ſhould always lay out a certain ſum before government advanced an equal moiety.

Means ſhould be provided for the maintenance of the roads and bridges after they have been completed; and for this purpoſe there ſhould either be a fund reſerved, or a ſmall toll laid on, to go in aid of the ſtatute labour of the country. A very important conſideration alſo, is the erecting and maintaining proper inns upon the roads.

Several of the houſes which were built by government upon the military roads, are ſtriking inſtances of the neceſſity there is of giving the people who are to keep the inns ſomething elſe to depend upon beſides what ariſes from ſupplying travellers; there ſhould be ſome land attached to the houſe, at a rent to be ſettled by reference. I am not prepared to ſay what the quantity ſhould be, or of what particular deſcription; it is at preſent ſufficient to point out the principle.

Upon the whole, as far as regards the bridges and roads, I can have no heſitation in ſtating, that they are of the greateſt moment for promoting the improvement of the country, and for perfecting the connections with the fiſhing lochs and the Hebrides; and I ſhall hereafter endeavour to explain my reaſon for thinking that ſome ſhare of the emigrations is to be attributed to the want of proper communications.

Naval Stations.

Before entering upon the ſubject of the Caledonian canal it will be proper to obſerve, that I again examined the Bay of Cromarty, and have procured ſome more information reſpecting it and the Murray Frith.

Mr. Henderſon, the reſident cuſtom-houſe officer, a man of reſpectability, has, at my deſire, aſcertained the depth of water in the wells, and alſo what quantity flows from a ſpring about half a mile from the town. From this letter it appears, that at the depth of twenty feet from the ſurface of the plain upon which the town ſtands, there are four feet of water in their wells; when at Cromarty I learned, that at high ſprings this water is brackiſh, but it is generally uſed for waſhing, and other domeſtic purpoſes. At the diſtance of about half a mile from the town there is a fine ſpring, which was diſcovered by the late Mr. Roſs during his ineffectual attempt to find coal: from this circumſtance it is called the Coal Well. This ſpring produces, on an average, upwards of twenty hogſheads of water in an hour; this water might be collected into a reſervoir at a ſmall expenſe; from this reſervoir it might be brought to the ſhore, which is a ſhort diſtance, in pipes, and by means of hoſe run into caſks in a ſhip's boat: to protect the boats, a ſmall pier might be formed at this place.

Captain Duff, of the royal navy, who is well acquainted with the country, and has navigated the Moray Frith in a frigate, has been ſo obliging as to reſolve ſome queries which I took the liberty of tranſmitting to him through his brother-in-law colonel Dirom. Captain Duff confirms the general report of the excellence of the Bay of Cromarty, and the entrance to it; his opinion reſpecting the navigation of the Moray Frith is quite as favourable as I have been led to advance in my laſt report. From what he ſtates it appears, that even with a contrary wind a ſhip of war could clear the headlands in forty-eight hours from the time of leaving the Bay of Cromarty. From this account, perſons converſant in naval affairs will be able to judge how far this bay would be ſuitable for a ſquadron deſtined to watch the mouth of the Baltic and protect the coaſt. It would be ſingularly well ſituated for convoys to veſſels coming from the weſtward through the Caledonian canal, and when returning with the ſame trade; after ſeeing the merchant ſhips paſs Fort George, the ſhips of war would be cloſe in with their own harbour.

As an aid to this ſtation, the harbour of Aberdeen might be made to receive frigates. This harbour is not embayed, and frigates might ſail from it at all times. In my laſt report I ſtated the expenſe which would attend improving this harbour, and I diſtinguiſhed what extra expenſe would be required to render it capable of receiving frigates, which appeared to be 33,700 l.

The magiſtrates of the town, by whoſe ſpirited exertions ſeveral valuable improvements have already been effected, have authorized me to ſay, that they are ready to co-operate with government as far as their circumſtances will admit. If frigates were ſtationed at Aberdeen, and large ſhips at Cromarty, a naval protection would be immediately obtained in that quarter at a very moderate expenſe; and experience would point out what future improvements were neceſſary upon this coaſt.

From Aberdeen, Peterhead, and Fraſerburgh, a communication might be kept up with Cromarty by means of ſignals or a telegraph, or by land expreſſes to Nairn, where the frith is only three leagues over; or a faſt ſailing veſſel would ſoon run with an eaſterly wind from Fraſerburgh to Cromarty.

If Cromarty Bay was made a roadſtead, there would be wanted a ſtorehouſe and ſome other conveniencies, which may be on a ſmall ſcale, until the merits of the place have been fully proved: allow an expenſe here of 5000l.

The Caledonian Canal.

I paſſed along the whole line of this canal, that is, from Invernefs on the eaſt, to Fort William on the weſt coaſt. I took much pains to examine into the nature of the navigation of the lochs, their ſoundings, and anchoring places. For this purpoſe I applied chiefly to Mr. Guynn, who commanded the Lochneſs galley, in government ſervice, thirty-ſix years; from him I obtained a very full and ſatisfactory account of Lochneſs: he alſo took the ſoundings of Loch Oich and a part of Loch Lochy for me.

From him I learnt that Lochneſs is twenty-two miles in length, and from one to two and a half miles in breadth; that its extreme depth is 135 fathoms, and generally 15 to 20 very near the ſhores; that there are ſix anchoring places on the north, and four on the ſouth ſide; and that at each end of the loch there is a good anchorage in from three to five, eight, and ten fathom water.

That a veſſel paſſing from the weſt to the eaſt end before a weſterly wind, or in a contrary direction before an eaſterly wind, has nothing to do but run before the wind the whole way, which ſhe would do in from three to five hours, and with contrary winds in moderate weather ſhe would work it in from twenty-four to thirty-ſix hours.

That the eaſterly winds generally prevail from March till the end of September, when the weſterly winds ſet in, and continue for the reſt of the year.

The ſoundings he took in Loch Lochy are from ſeven to ſixty-eight fathoms, with bold ſhores and good anchorage.

In Loch Quoich, which is a ſhort loch on the ſummit level, there are ſome ſhallow parts, but they may be made ſufficiently deep.

I next proceeded to examine the ſupplies of water which are on the ſummit level, and for this purpoſe I paſſed up the valley of Glengarey to Loch Hourn at the back of Skye. In this valley I found Loch Garry, which appears to be upwards of five miles in length, and from half a mile to one mile in breadth: towards the head of the valley is Loch Quoich, which appears to be upwards of eight miles in length, and from one to two miles in breadth: theſe natural reſervoirs, placed in an extenſive valley and a rainy country, form an abundant proviſion for every purpoſe to which water can be applied in the courſe of the canal.

Having aſcertained theſe points, I paſſed by a very rocky and precipitous tract down to the head of Loch Hourn; from Loch Hourn I travelled by a tract ſcarcely leſs rugged to the top of Glen Elg, and over the ſteep mountain of Raatachan to the top of Loch Duich; from thence I travelled along the veſtiges of a military road, up Glen Shiel, down a part of Glen Morriſon, and over a rugged mountain to Fort Auguſtus. In Glen Morriſon and Glen Garry it is poſſible to make roads, if judiciouſly laid out, upon an eaſy aſcent; but the idea of water conveyance through them between the Caledonian canal and the fiſhing lochs is altogether unadviſable.

My next object was to examine the country which lies between the top of Loch Eil and Loch Shiel, in order to find whether a water conveyance could be made at a moderate expenſe from Loch Eil through Loch Shiel, and ſo into the fiſhing grounds to the ſouth of Skye, without paſſing down the Linnhe Loch, up the Sound of Mull, and round the Point of Ardnamurchan. I carried a level acroſs the neck of land which ſeparates Loch Eil from Loch Shiel: the diſtance is about three miles. I found the ſummit of the land forty-three feet above high water in Loch Eil, and thirty-five feet five inches above the level of the freſh water in Loch Shiel. In order to form a canal, as there is no water to be got on this ſummit, the ground muſt be cut down to twelve feet below the level of the water in Loch Shiel, which would make forty-ſeven feet five inches of cutting, and this depth of cutting would be continued for nearly a mile: I alſo ſuſpect that in this diſtance rock would be met with. I am therefore ſorry to ſay, I cannot adviſe the work being undertaken before the nature of the ground has been fully proved, and the whole of Loch Shiel has been examined with great care: and as this paſſage cannot be uſeful unleſs the Caledonian canal be made and navigated from the eaſt, if that event ſhould take place, there will be plenty of time to re-examine this point with care; at preſent it ought, in my opinion, to be poſtponed.

Having inveſtigated all the points which fell under my obſervation as a civil engineer, I became extremely deſirous of having the opinion of experienced and well-informed mercantile and ſea-faring people with regard to the preſent navigation by the Pentland Frith and the Orkneys, and the propoſed inland navigation by the Caledonian canal. With the view of procuring the beſt poſſible information on this ſubject, I applied to Leith, Aberdeen, and Peterhead, on the eaſt coaſt; and to Greenock, Dublin, Liverpool, and Briſtol, on the weſt.

In the appendix to this report I have inſerted the queries and anſwers; by which it may be ſeen that there is only one opinion as to the dangers and inconveniencies of the preſent navigation, and the advantages which may be expected from the propoſed inland navigation, if united with a naval ſtation in the Moray Frith, or on the adjacent coaſt of Scotland.

This ſanction of experienced people, who are all deeply intereſted in commercial concerns, will, I truſt, ſatisfy your lordſhips, that it has not been upon unſubſtantial grounds that I have ventured to recommend this great national object.

My eſtimate of the expenſe of forming this navigation is nearly 350,000l., and the time required to complete it would probably be about ſeven years: this diviſion would require an annual ſupply of 50,000 l.

Upwards of thirty veſſels have been wrecked on the coaſt of Caithneſs in the memory of Alexander Miller, of Staxigo.

[To be continued.]

LVI. *On Winds.* *By* Richard Kirwan, *Eſq. LL.D. F.R.S. and P.R.I.A.*[*]

Of the Origin of the general Trade Winds.

THOUGH the origin of the general trade winds appears to me to have been fully eſtabliſhed by Dr. Halley, yet it ſeems he has explained himſelf too briefly, ſince his explanation has been miſunderſtood by many, and was thought obſcure even by d'Alembert [†].

To underſtand it more perfectly, let us ſuppoſe the ſun for the firſt time in the meridian, and to communicate its heat every inſtant fifteen degrees all around. If it were to remain in this ſituation the ſurrounding air could have no other motion but upwards, for the lateral dilatations being equal, would neceſſarily check each other, but in the ſecond, and all the ſucceeding inſtants, the ſun moves weſtwards; therefore, of the originally equidiſtant eaſtern and weſtern points, the weſtern, to which the ſun approaches nearer, is more heated than the eaſtern, from which the ſun recedes; therefore in this, and all the ſucceeding inſtants, the eaſtern, being more cooled, will preſs on the weſtern, and thus an eaſtern wind will be eſtabliſhed.

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[*] From his paper entitled "Of the Variations of the Atmoſphere 1801."

[†] Sur la Cauſe des Vents. v.

It is true, that, in the northern hemiſphere, the northern air alſo preſſes upon the more heated ſpaces; but as this alſo follows the ſun's path to the weſtward, it becomes alſo eaſterly, preſerving only a few points of its primitive direction. D'Alembert adds alſo the ſolar attraction, which, according to him, elevates the air in the points over which the ſun is vertical, and conſequently produces a dilatation advancing from eaſt to weſt. But M. De la Place, not denying this cauſe, conſiders it too weak to produce ſingly any conſiderable effect *.

About the year 1735 Mr. Hadley publiſhed a very different account of the origin of the trade winds (Phil. Tranſ. Abrid. viii. p. 500); which, however, has been rejected by the moſt diſtinguiſhed aſtronomers that have ſince attended to this object, as d'Alembert, ſur la Cauſe des Vents, art. 376 and 385; Gentil Voy.; Bergman Erde Beſchreib. ii. p. 91.

According to Mr. Hadley, the air, being rarefied towards the equator, is conſequently invaded in the northern hemiſphere by the northern, and in the ſouthern hemiſphere by the ſouthern colder air.

But as the parallels of latitude enlarge as they approach the equator, and as the equatorial ſpace is nearly in the proportion of 1000 to 917, the difference of their circumference is nearly 2083 miles; conſequently, the ſurface of the globe at the equator moves ſo much faſter than under the tropics; and hence the northern or ſouthern air, moving from the tropics towards the equator, muſt poſſeſs leſs velocity than the parts it arrives at, and conſequently appear to move in a direction contrary to that of the earth's motion; which being from weſt to eaſt, the air arriving ſooner at the weſtern parts, will appear to move from eaſt to weſt; and this relative motion being combined with that towards the equator, a north-eaſt wind will be produced on the north ſide, and a ſouth-eaſt wind on the ſouth ſide of the equator. Theſe as they approach the equator ſhould become ſtronger and more eaſterly, and appear due eaſt in the equator itſelf, by reaſon of the concourſe of both currents from the north and from the ſouth. There the velocity of each ſhould be at the rate of 2083 miles in the ſpace of one natural day, or above 1·33

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* Mém. Paris 1776.

miles per minute, if it had not been that before the air at the tropics could arrive at the equator, it muſt have gained ſome motion eaſtwards from the ſurface of the earth or ſea, whereby the relative motion is diminiſhed to the degree that actually exiſts in it.

This theory appears to me rather ingenious than ſolid, for the following reaſons:

1. The trade winds are commonly gentle, moving only at the rate of eight miles an hour; therefore they have ſufficient time to gain or participate of the motion of the earth; therefore their contrary courſe muſt ariſe from an abſolute cauſe, and cannot be deemed merely relative.

2. Becauſe the north-eaſt wind ſcarce ever approaches nearer than eight or ten degrees to the equator, and there dies away; whereas it ought there, according to this theory, to be ſtrongeſt. And, on the contrary, the ſouth-eaſt paſſes the equator ſeveral degrees, even when the ſun is in the ſouth tropic. A fact which, as Gentil remarks, is abſolutely irreconcileable with this theory. (Gentil Voy. i. p. 638; Ibid. v. p. 116.)

3. Becauſe, if the conſtant eaſterly wind was in the northern hemiſphere ſupplied ſolely from the north, and in the ſouthern hemiſphere ſolely from the ſouth, we ſhould in the former have a conſtant north wind at leaſt at 35 or 40 degrees from the equator, or at leaſt from ſome northern point, and in the latter a conſtant ſouth wind, or at leaſt from ſome ſouthern point; whereas, on the contrary, a ſouth wind often prevails in thoſe latitudes on the north ſide of the equator, and a north wind on the ſouth ſide. Thus La Perouſe met an E. S. E. in north latitude 32°, and a due eaſt in latitude 31°; and a S. S. E. in latitude 14°, and a due eaſt in latitude 16°; and a due north in latitude 20°, (where then was the relative motion?) and a due ſouth in latitude 33°. (See his Journal in La Peyr. Voy. iii.) He alſo met with a due north in latitude 27° and 42° ſouth, and a N. N. E. in latitude 25° ſouth. So captain Cook met a S. S. E. wind in latitude 30° north, and alſo in latitudes 40° and 41°, and a due ſouth wind in latitude 38° and 20°; and in the ſouthern hemiſphere a due north in latitude 3°, 4°, and 44°. I might produce other inſtances from ſea journals, and particularly from that, moſt ample and inſtructive, kept by major Dalrymple during a voyage to the Eaſt Indies (Phil. Tranſ. 1778); but I think the alleged ſufficiently prove that the general eaſt wind is not ſupplied ſolely from the north or ſouth in the different hemiſpheres reſpectively.

4. Becauſe, during our ſix ſummer months, when the ſun is in or approaches to the northern tropic, the eaſterly trade

wind partakes leſs of the northerly, than when the ſun is in or approaches to the ſouthern tropic, (Phil. Tranſ. Abrid. ii. p. 134; and Schued. Abhandl. 1762, p. 175:) which is directly contrary to Hadley's ſyſtem; for when the ſun is in the ſouthern tropic, the north wind muſt traverſe more of that ſpace in which the earth's motion eaſtward is ſtrongeſt, and therefore ſhould participate more of that motion, as Hadley himſelf ſtates: though ſtill partaking of it in a ſmaller degree than that which the globe itſelf poſſeſſes, it ſhould appear to move weſtwards; yet it ſhould proportionably retain leſs of its original direction from north to ſouth than when it had traverſed a ſpace more diſtant from the equator, whereas the fact is that it retains more, and often paſſes into the ſouthern hemiſphere into the 13° ſouth latitude without having any eaſtern direction, (Marchand iii. p. 551;) and an analogous fact is obſerved with reſpect to the ſouth-eaſt wind when the ſun is in the northern tropic. Hence it is evident, that it is from the approach of the ſun, and not from the latitude traverſed, that the eaſtern direction is derived; nay, the wind is often more eaſterly than northerly between latitude 23° and 28°: (Foſter's Obſervations, p. 126.) He even obſerved that the trade winds extended far beyond the tropics when the ſun is in the ſame hemiſphere, which ſhews it is the ſun that cauſes them.

Eddy is a term introduced on this ſubject, which explains nothing when its cauſe is not aſſigned and proved; the trade winds are often interrupted by the approach of land; but the interruption, as Foſter mentions, extends only to a few miles. Ibid. 127.

The *monſoons* or periodical trade winds depending on local circumſtances, ſufficiently explained by Dr. Halley, I ſhall here paſs over; though certainly much may be added from obſervations made by ſubſequent navigators and travellers. I ſhall therefore confine myſelf to the *variable* winds, a ſubject much more obſcure.

Of Variable Winds.

With reſpect to winds we muſt lay down one general and fundamental principle, which is, that they always originate at the extremity of that point towards which they proceed. Thus the eaſterly trade wind begins at the point neareſt the ſun, which it follows, and is perpetually renovated and ſupplied from parts ſtill more eaſterly. Thus in the year 1709 a north wind was ſooner perceived in England than at Dantzic (Phil. Tranſ. Abrid. iv. part ii. p. 115. And Wargentin notes, that when the wind changes to the weſt, this change takes place at Moſcow before it happens at Abo, which is ſeveral degrees weſt of it; and ſooner in Finland than in Sweden. (Schwd. Abhandl. 1762, p. 195.) And Dr. Franklin, in his xxxvith Letter, p. 389, thinks that the north-eaſt ſtorms in North America begin firſt, in point of time, in the ſouth-weſt parts; that is to ſay, ſooner in Georgia than in Carolina, and ſooner in Carolina than in Virginia, &c. He found that a north-eaſt ſtorm began at Philadelphia at ſeven o'clock, but did not extend to Boſton (about forty miles to the north-eaſt) until eleven o'clock. The reaſon of which he well explains, as the current muſt begin in the places neareſt to that in which the rarefaction ariſes, towards which the current is directed.

Of Weſterly Winds.

That eminent and laborious meteorologiſt C. La Cotte, infers from numerous obſervations of many years, that between latitude 47° and 60° on the weſtern ſide of our hemiſphere, the weſt wind, with ſome participation of the north or ſouth, is that which obtains ofteneſt. (Roz. Jour. xxxix. p. 267.) Leche obtained the ſame reſult at Abo, latitude 60°, from twelve years obſervations; Muſchenbroeck, in Utrecht; Mr. Dalton, in Weſtmorland, latitude 54°, (ſee his Meteorological Eſſays, p. 48 and 88,) from five years obſervations.

This wind in our continent originates in the Pacific Ocean between the above-mentioned parallels, at leaſt in winter; the air incumbent on that ocean is then much warmer than that of Siberia and Chineſe Tartary that lie weſt of it; this therefore preſſes upon and flows into the ſupra-marine, and is immediately ſucceeded by air ſtill further weſtwards, and thus a current is gradually eſtabliſhed extending to the Atlantic, which, though in winter, being much warmer than the air of the iſlands and continent on which it flows, is forced into the current, both by the rupture of the equilibrium to the eaſtwards, and by the preſſure of the much colder air of the continent of North America.

Of Eaſterly Winds.

During the winter months there ſeems to be a frequent ſtruggle and conteſt betwixt the air incumbent over the Aſiatic continent and that incumbent on the North American lying betwixt the above-mentioned parallels and bordering on the Pacific Ocean, which of them ſhall rule over it.

The maſs of the American air being leſs conſiderable, and its efforts divided between the Pacific and the Atlantic, is generally obliged to yield to its antagoniſt; though ſometimes the Aſiatic being warmed, either by a diffuſion of the ſuperior current or by ſoutherly winds, the colder American becomes more forcible. In ſummer this muſt happen frequently, the E.N.E. ofteneſt prevailing: upon the whole, however, Leche remarked that the eaſt and E.S.E. were nearly the moſt uncommon; as did La Cotte in the climate of Paris. (Meteorolog. p. 305.)

With us this wind is moſt frequent in the months of April and May; and I have obſerved in Cook's Journal, tables 9th, 10th, and 11th, that it prevails alſo in the ſame months in the Pacific, therefore the colder continental air then pours in upon us.

La Cotte alſo obſerves, that in the weſtern tracts of Europe, in latitudes below 48°, this wind occurs ofteneſt during the winter months*; for the ſuperior heat of the Atlantic in the low latitudes determines the colder air incumbent on Hungary and European Turkey to flow in upon it.

Of Southerly Winds.

A few years ago, no problem in meteorology appeared to me more difficult than to aſſign a cauſe for the frequent prevalence of a ſouth wind even in winter, it being contrary to the laws of nature that warm air ſhould ruſh upon colder; yet I ſince diſcovered that the conjectural ſolution I then offered is grounded on a real fact.

In the eaſtern parts of our hemiſphere, from longitude 72° to 160°, that is, from the coaſt of Malabar to the Moluccas, it blows from the north-eaſt conſtantly from October until April. Now this northern blaſt muſt be ſupplied and recruited from countries ſtill further north until we arrive at the pole, and the polar air muſt conſequently be ſupplied by that which lies ſouth of it, and thus a ſouthern current is eſtabliſhed on the weſtern ſide of our hemiſphere.

Inſtances to ſupport or contradict this theory do not often occur; yet I have found ſome that appear to me deciſive, independently of the general reaſon alleged. Thus I find in the ninth table of the third volume of Cook's Voyages, that in north latitude 59°, and eaſt longitude 207°, on the 25th of May 1778, a ſtrong north-weſt wind prevailed; and on the 29th day of the ſame month and year, an equally ſtrong ſouth-weſt wind prevailed at Peterſburgh, latitude 60° and longitude 30° eaſt. Now the places of obſervation were 177 degrees diſtant, one on the eaſtern and the other on the weſtern ſide of our hemiſphere, (which, at this proximity to the pole, argues not a ſuperior diſtance to that I have mentioned;)

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* Mem. Meteorolog. ii. p. 189, &c.

tioned;) and four days is as ſhort a time as can be allowed to the ſouth-weſt to ſupply the more eaſtern north-weſt. (Mem. Peterſburgh 1778, p. 92.) So alſo in the ſame journal I find, that from the 4th to the 30th of May a north wind prevailed in the eaſtern part of our hemiſphere from latitude 58° to 61°, except ſeventeen days of variable winds; but in London it blew from the ſouth-weſt during the firſt fifteen days of June, thus replacing the northern air. And to replace the conſtant north-eaſt wind on the Indian peninſula to the Moluccas, there is a conſtant draught from the ſouth in the weſtern parts of our hemiſpherè; accordingly Leſke obſerved, that on an average of twelve years it blew 126 days each year, from October until May, from ſome ſouth point, namely, 86 days from the ſouth or ſouth-weſt, and 40 from the ſouth-eaſt, at Abo, latitude 60°.

It is true, that he found it to take place very frequently alſo in ſummer; but this is occaſioned by the great heat that then prevails in the northern tracts of Lapland.

And, upon the whole, more of the ſouth air is drawn off in winter than in ſummer; for its flow is gentle in ſummer, but often ſtormy in winter. See Leſke's 9th, 11th, and 12th tables. If all other meteorological tables of a ſeries of years had been arranged with equal ſagacity and preciſion as thoſe of Leche and Dr. Horſeley, a vaſt fund of information might be extracted from them.

At Peterſburgh, during the year 1793, Euler junior found a ſouth or ſouth-weſt wind prevailed 79 days, 52 from October to the end of March, and only 27 in the ſummer months: it was ſtormy in November, December, and January. I have not noted the ſouth-eaſt.

Mr. Stritter alſo found the ſouth wind to predominate at Moſcow during the ſix winter months of that year, (N. Acta Petrop. xi. p. 569;) ſo that the frequency of this wind in high latitudes is certain.

Of Northerly Winds.

In the weſtern parts of our continent and hemiſphere theſe are of all others the leaſt frequent in latitudes above 48°. See La Cotte's and Leche's tables. The cauſe of this unfrequency appears from what has been ſaid of ſouth winds.

But in latitudes below 48° they occur oftener, and ofteneſt in thoſe that are ſtill lower, as La Cotte remarks. An admirable inſtance of Divine Providence, that the warmeſt winds ſhould prevail ofteneſt in winter in the coldeſt regions, and cold winds in the warmeſt!

But it may be aſked, why a ſouth wind ſhould not prevail in the eaſtern parts of our hemiſphere to ſupply the conſtant north-eaſt wind that prevails in the low latitudes of the weſtern ſide? The reaſon is, that on the weſtern ſide the north-eaſt winds of low latitudes are eaſily ſupplied by the contiguous Atlantic, which is open up to the North Pole; and, as here, the upper current ſets and ceaſes, there can be no deficiency of air.

Of oppoſite concomitant Winds.

It has often been obſerved[*], but of late, ſince the invention of balloons, evidently proved, that currents of air from different and even oppoſite points of the horizon, prevail at different heights in the atmoſphere over the ſame tracts of land or water. This was originally inferred from the different courſes of the higher and lower clouds; but as ſuch obſervations were often liable to optical deceptions, better proofs were wanting.

Mont Louis is within thirty miles of Perpignan, but about 5000 feet higher. Now in March 1780, north and north-eaſt winds prevailed at Perpignan and a weſterly wind at St. Louis. In Auguſt a north wind prevailed at Perpignan and an eaſt at Mont Louis. Mém. de la Société de Medecine de Paris 1780. Derham ſuſpected[†], and Gentil has ſince ſhown, that changes of ſeaſons conſtantly begin in the upper atmoſphere; while a ſtrong wind blows from one point below, a wind from an oppoſite point reigns above, but more gentle, until at laſt (in about three weeks) it is propagated downwards. (Voy. ii. p. 23, 24, in 8vo.) The lower atmoſphere, he ſays, extends to the height of 2880 feet. (Vol. iv. p. 48.) At the commencement of winter, when the ſun approaches the ſouth tropic, and the north air begins to flow in and follow it, it muſt meet with more reſiſtance from the lower denſer air, as its impetuous courſe in an oppoſite direction is more ſlowly altered (this reſpects the monſoons) than in the rarer ſuperior ſtrata; and the ſame effect, but in a different direction, takes place when the ſun approaches to the northern tropic.

It has been ſaid by many, that winds in the ſuperior regions of the atmoſphere are much more violent and impetuous than in the lower. (Sauſſure Hygrom. p. 300: Ulloa's Voy. ii. p. 81: Muſchenbr. § 2612: Bergm. Erde kugel. ii. p. 99: De Luc, &c.) But the contrary has alſo been obſerved by Gentil, above quoted, and Morveau. (Aëroſt. de Dijon.)

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[*] Ulloa's Voy. ii. p. 62. Engliſh.

[†] Phil. Tranſ. Abridg. iv. part ii. 125.

Of

Of the Succeſſion of Winds.

Well eſtabliſhed general laws on this head would be extremely uſeful, as we might then foreſee what wind might next be expected. Beſides the *general* ſucceſſion in an open country, it is probable there is a *local*, confined to certain ſituations.

Gentil remarks, that in the ſouthern latitudes of our hemiſphere, a north-eaſt is ſucceeded by an eaſt, ſouth-eaſt, and ſouth. According to La Cotte, the order of ſucceſſion in the middle latitudes is ſouth-weſt, north, weſt, north-eaſt, ſouth, north-weſt, eaſt, ſouth-eaſt. (Roz. Journ. xxxix. p. 267.)

Of the Scirocco.

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LVII. *Report preſented to the Claſs of the Exact Sciences of the Academy of Turin, January 12, 1803, on the Action of Galvaniſm, and the Application of this Fluid and of Electricity to Medicine.* By A. M. VASSALI-EANDI[*].

THE Galvanic experiments made on the 10th and 14th of Auguſt laſt, in the preſence of a great many ſpectators, by Giulio, Roſſi, and myſelf, on the head and trunk of three decapitated criminals, an account of which has been publiſhed, gave riſe to ſeveral queſtions in regard to this agent, and by analogy reſpecting electricity. Theſe two fluids, and the uſes to which they may be applied, have become a common ſubject of converſation among well-informed per-the weſtern, to which the ſun approaches nearer, is more heated than the eaſtern, from which the ſun recedes; therefore in this, and all the ſucceeding inſtants, the eaſtern, being more cooled, will preſs on the weſtern, and thus an eaſtern wind will be eſtabliſhed.

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[*] From the *Journal de Phyſique*, Germinal, an. 11.

It is true, that, in the northern hemiſphere, the northern air alſo preſſes upon the more heated ſpaces; but as this alſo follows the ſun's path to the weſtward, it becomes alſo eaſterly, preſerving only a few points of its primitive direction. D'Alembert adds alſo the ſolar attraction, which, according to him, elevates the air in the points over which the ſun is vertical, and conſequently produces a dilatation advancing from eaſt to weſt. But M. De la Place, not denying this cauſe, conſiders it too weak to produce ſingly any conſiderable effect *.

About the year 1735 Mr. Hadley publiſhed a very different account of the origin of the trade winds (Phil. Tranſ. Abrid. viii. p. 500); which, however, has been rejected by the moſt diſtinguiſhed aſtronomers that have ſince attended to this object, as d'Alembert, ſur la Cauſe des Vents, art. 376 and 385; Gentil Voy.; Bergman Erde Beſchreib. ii. p. 91.

According to Mr. Hadley, the air, being rarefied towards the equator, is conſequently invaded in the northern hemiſphere by the northern, and in the ſouthern hemiſphere by the ſouthern colder air.

But as the parallels of latitude enlarge as they approach the equator, and as the equatorial ſpace is nearly in the proportion of 1000 to 917, the difference of their circumference is nearly 2083 miles; conſequently, the ſurface of the globe at the equator moves ſo much faſter than under the tropics; and hence the northern or ſouthern air, moving from the tropics towards the equator, muſt poſſeſs leſs velocity than the parts it arrives at, and conſequently appear to move in a direction contrary to that of the earth's motion; which being from weſt to eaſt, the air arriving ſooner at the weſtern parts, will appear to move from eaſt to weſt; and this relative motion being combined with that towards the equator, a north-eaſt wind will be produced on the north ſide, and a ſouth-eaſt wind on the ſouth ſide of the equator. Theſe as they approach the equator ſhould become ſtronger and more eaſterly, and appear due eaſt in the equator itſelf, by reaſon of the concourſe of both currents from the north and from the ſouth. There the velocity of each ſhould be at the rate of 2083 miles in the ſpace of one natural day, or above 1·33

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* Mém. Paris 1776.

miles per minute, if it had not been that before the air at the tropics could arrive at the equator, it muſt have gained ſome motion eaſtwards from the ſurface of the earth or ſea, whereby the relative motion is diminiſhed to the degree that actually exiſts in it.

This theory appears to me rather ingenious than ſolid, for the following reaſons:

1. The trade winds are commonly gentle, moving only at the rate of eight miles an hour; therefore they have ſufficient time to gain or participate of the motion of the earth; therefore their contrary courſe muſt ariſe from an abſolute cauſe, and cannot be deemed merely relative.

2. Becauſe the north-eaſt wind ſcarce ever approaches nearer than eight or ten degrees to the equator, and there dies away; whereas it ought there, according to this theory, to be ſtrongeſt. And, on the contrary, the ſouth-eaſt paſſes the equator ſeveral degrees, even when the ſun is in the ſouth tropic. A fact which, as Gentil remarks, is abſolutely irreconcileable with this theory. (Gentil Voy. i. p. 638; Ibid. v. p. 116.)

3. Becauſe, if the conſtant eaſterly wind was in the northern hemiſphere ſupplied ſolely from the north, and in the ſouthern hemiſphere ſolely from the ſouth, we ſhould in the former have a conſtant north wind at leaſt at 35 or 40 degrees from the equator, or at leaſt from ſome northern point, and in the latter a conſtant ſouth wind, or at leaſt from ſome ſouthern point; whereas, on the contrary, a ſouth wind often prevails in thoſe latitudes on the north ſide of the equator, and a north wind on the ſouth ſide. Thus La Perouſe met an E. S. E. in north latitude 32°, and a due eaſt in latitude 31°; and a S. S. E. in latitude 14°, and a due eaſt in latitude 16°; and a due north in latitude 20°, (where then was the relative motion?) and a due ſouth in latitude 33°. (See his Journal in La Peyr. Voy. iii.) He alſo met with a due north in latitude 27° and 42° ſouth, and a N. N. E. in latitude 25° ſouth. So captain Cook met a S. S. E. wind in latitude 30° north, and alſo in latitudes 40° and 41°, and a due ſouth wind in latitude 38° and 20°; and in the ſouthern hemiſphere a due north in latitude 3°, 4°, and 44°. I might produce other inſtances from ſea journals, and particularly from that, moſt ample and inſtructive, kept by major Dalrymple during a voyage to the Eaſt Indies (Phil. Tranſ. 1778); but I think the alleged ſufficiently prove that the general eaſt wind is not ſupplied ſolely from the north or ſouth in the different hemiſpheres reſpectively.

4. Becauſe, during our ſix ſummer months, when the ſun is in or approaches to the northern tropic, the eaſterly trade

wind partakes leſs of the northerly, than when the ſun is in or approaches to the ſouthern tropic, (Phil. Tranſ. Abrid. ii. p. 134; and Schued. Abhandl. 1762, p. 175:) which is directly contrary to Hadley's ſyſtem; for when the ſun is in the ſouthern tropic, the north wind muſt traverſe more of that ſpace in which the earth's motion eaſtward is ſtrongeſt, and therefore ſhould participate more of that motion, as Hadley himſelf ſtates: though ſtill partaking of it in a ſmaller degree than that which the globe itſelf poſſeſſes, it ſhould appear to move weſtwards; yet it ſhould proportionably retain leſs of its original direction from north to ſouth than when it had traverſed a ſpace more diſtant from the equator, whereas the fact is that it retains more, and often paſſes into the ſouthern hemiſphere into the 13° ſouth latitude without having any eaſtern direction, (Marchand iii. p. 551;) and an analogous fact is obſerved with reſpect to the ſouth-eaſt wind when the ſun is in the northern tropic. Hence it is evident, that it is from the approach of the ſun, and not from the latitude traverſed, that the eaſtern direction is derived; nay, the wind is often more eaſterly than northerly between latitude 23° and 28°: (Foſter's Obſervations, p. 126.) He even obſerved that the trade winds extended far beyond the tropics when the ſun is in the ſame hemiſphere, which ſhews it is the ſun that cauſes them.

Eddy is a term introduced on this ſubject, which explains nothing when its cauſe is not aſſigned and proved; the trade winds are often interrupted by the approach of land; but the interruption, as Foſter mentions, extends only to a few miles. Ibid. 127.

The *monſoons* or periodical trade winds depending on local circumſtances, ſufficiently explained by Dr. Halley, I ſhall here paſs over; though certainly much may be added from obſervations made by ſubſequent navigators and travellers. I ſhall therefore confine myſelf to the *variable* winds, a ſubject much more obſcure.

Of Variable Winds.

With reſpect to winds we muſt lay down one general and fundamental principle, which is, that they always originate at the extremity of that point towards which they proceed. Thus the eaſterly trade wind begins at the point neareſt the ſun, which it follows, and is perpetually renovated and ſupplied from parts ſtill more eaſterly. Thus in the year 1709 a north wind was ſooner perceived in England than at Dantzic (Phil. Tranſ. Abrid. iv. part ii. p. 115. And Wargentin notes, that when the wind changes to the weſt, this change takes place at Moſcow before it happens at Abo, which is ſeveral degrees weſt of it; and ſooner in Finland than in Sweden. (Schwd. Abhandl. 1762, p. 195.) And Dr. Franklin, in his xxxvith Letter, p. 389, thinks that the north-eaſt ſtorms in North America begin firſt, in point of time, in the ſouth-weſt parts; that is to ſay, ſooner in Georgia than in Carolina, and ſooner in Carolina than in Virginia, &c. He found that a north-eaſt ſtorm began at Philadelphia at ſeven o'clock, but did not extend to Boſton (about forty miles to the north-eaſt) until eleven o'clock. The reaſon of which He well explains, as the current muſt begin in the places neareſt to that in which the rarefaction ariſes, towards which the current is directed.

Of Weſterly Winds.

That eminent and laborious meteorologiſt C. La Cotte, infers from numerous obſervations of many years, that between latitude 47° and 60° on the weſtern ſide of our hemiſphere, the weſt wind, with ſome participation of the north or ſouth, is that which obtains ofteneſt. (Roz. Jour. xxxix. p. 267.) Leche obtained the ſame reſult at Abo, latitude 60°, from twelve years obſervations; Muſchenbroeck, in Utrecht; Mr. Dalton, in Weſtmorland, latitude 54°, (ſee his Meteorological Eſſays, p. 48 and 88,) from five years obſervations.

This wind in our continent originates in the Pacific Ocean between the above-mentioned parallels, at leaſt in winter; the air incumbent on that ocean is then much warmer than that of Siberia and Chineſe Tartary that lie weſt of it; this therefore preſſes upon and flows into the ſupra-marine, and is immediately ſucceeded by air ſtill further weſtwards, and thus a current is gradually eſtabliſhed extending to the Atlantic, which, though in winter, being much warmer than the air of the iſlands and continent on which it flows, is forced into the current, both by the rupture of the equilibrium to the eaſtwards, and by the preſſure of the much colder air of the continent of North America.

Of Eaſterly Winds.

During the winter months there ſeems to be a frequent ſtruggle and conteſt betwixt the air incumbent over the Aſiatic continent and that incumbent on the North American lying betwixt the above-mentioned parallels and bordering on the Pacific Ocean, which of them ſhall rule over it.

The maſs of the American air being leſs conſiderable, and its efforts divided between the Pacific and the Atlantic, is generally obliged to yield to its antagoniſt; though ſometimes the Aſiatic being warmed, either by a diffuſion of the ſuperior current or by ſoutherly winds, the colder American becomes more forcible. In ſummer this muſt happen frequently, the E. N. E. ofteneſt prevailing: upon the whole, however, Leche remarked that the eaſt and E. S. E. were nearly the moſt uncommon; as did La Cotte in the climate of Paris. (Meteorolog. p. 305.)

With us this wind is moſt frequent in the months of April and May; and I have obſerved in Cook's Journal, tables 9th, 10th, and 11th, that it prevails alſo in the ſame months in the Pacific, therefore the colder continental air then pours in upon us.

La Cotte alſo obſerves, that in the weſtern tracts of Europe, in latitudes below 48°, this wind occurs ofteneſt during the winter months*; for the ſuperior heat of the Atlantic in the low latitudes determines the colder air incumbent on Hungary and European Turkey to flow in upon it.

Of Southerly Winds.

A few years ago, no problem in meteorology appeared to me more difficult than to aſſign a cauſe for the frequent prevalence of a ſouth wind even in winter, it being contrary to the laws of nature that warm air ſhould ruſh upon colder; yet I ſince diſcovered that the conjectural ſolution I then offered is grounded on a real fact.

In the eaſtern parts of our hemiſphere, from longitude 72° to 160°, that is, from the coaſt of Malabar to the Moluccas, it blows from the north-eaſt conſtantly from October until April. Now this northern blaſt muſt be ſupplied and recruited from countries ſtill further north until we arrive at the pole, and the polar air muſt conſequently be ſupplied by that which lies ſouth of it, and thus a ſouthern current is eſtabliſhed on the weſtern ſide of our hemiſphere.

Inſtances to ſupport or contradict this theory do not often occur; yet I have found ſome that appear to me deciſive, independently of the general reaſon alleged. Thus I find in the ninth table of the third volume of Cook's Voyages, that in north latitude 59°, and eaſt longitude 207°, on the 25th of May 1778, a ſtrong north-weſt wind prevailed; and on the 29th day of the ſame month and year, an equally ſtrong ſouth-weſt wind prevailed at Peterſburgh, latitude 60° and longitude 30° eaſt. Now the places of obſervation were 177 degrees diſtant, one on the eaſtern and the other on the weſtern ſide of our hemiſphere, (which, at this proximity to the pole, argues not a ſuperior diſtance to that I have mentioned;)

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* Mem. Meteorolog. ii. p. 189, &c.

tioned;) and four days is as ſhort a time as can be allowed to the ſouth-weſt to ſupply the more eaſtern north-weſt. (Mem. Peterſburgh 1778, p. 92.) So alſo in the ſame journal I find, that from the 4th to the 30th of May a north wind prevailed in the eaſtern part of our hemiſphere from latitude 58° to 61°, except ſeventeen days of variable winds; but in London it blew from the ſouth-weſt during the firſt fifteen days of June, thus replacing the northern air. And to replace the conſtant north-eaſt wind on the Indian peninſula to the Moluccas, there is a conſtant draught from the ſouth in the weſtern parts of our hemiſphere; accordingly Leſke obſerved, that on an average of twelve years it blew 126 days each year, from October until May, from ſome ſouth point, namely, 86 days from the ſouth or ſouth-weſt, and 40 from the ſouth-eaſt, at Abo, latitude 60°.

It is true, that he found it to take place very frequently alſo in ſummer; but this is occaſioned by the great heat that then prevails in the northern tracts of Lapland.

And, upon the whole, more of the ſouth air is drawn off in winter than in ſummer; for its flow is gentle in ſummer, but often ſtormy in winter. See Leſke's 9th, 11th, and 12th tables. If all other meteorological tables of a ſeries of years had been arranged with equal ſagacity and preciſion as thoſe of Leche and Dr. Horſeley, a vaſt fund of information might be extracted from them.

At Peterſburgh, during the year 1793, Euler junior found a ſouth or ſouth-weſt wind prevailed 79 days, 52 from October to the end of March, and only 27 in the ſummer months: it was ſtormy in November, December, and January. I have not noted the ſouth-eaſt.

Mr. Stritter alſo found the ſouth wind to predominate at Moſcow during the ſix winter months of that year, (N. Acta Petrop. xi. p. 569;) ſo that the frequency of this wind in high latitudes is certain.

Of Northerly Winds.

In the weſtern parts of our continent and hemiſphere theſe are of all others the leaſt frequent in latitudes above 48°. See La Cotte's and Leche's tables. The cauſe of this unfrequency appears from what has been ſaid of ſouth winds.

But in latitudes below 48° they occur oftener, and ofteneſt in thoſe that are ſtill lower, as La Cotte remarks. An admirable inſtance of Divine Providence, that the warmeſt winds ſhould prevail ofteneſt in winter in the coldeſt regions, and cold winds in the warmeſt!

But it may be aſked, why a ſouth wind ſhould not prevail in the eaſtern parts of our hemiſphere to ſupply the conſtant north-eaſt wind that prevails in the low latitudes of the weſtern ſide? The reaſon is, that on the weſtern ſide the north-eaſt winds of low latitudes are eaſily ſupplied by the contiguous Atlantic, which is open up to the North Pole; and, as here, the upper current ſets and ceaſes, there can be no deficiency of air.

Of oppoſite concomitant Winds.

It has often been obſerved *, but of late, ſince the invention of balloons, evidently proved, that currents of air from different and even oppoſite points of the horizon, prevail at different heights in the atmoſphere over the ſame tracts of land or water. This was originally inferred from the different courſes of the higher and lower clouds; but as ſuch obſervations were often liable to optical deceptions, better proofs were wanting.

Mont Louis is within thirty miles of Perpignan, but about 5000 feet higher. Now in March 1780, north and north-eaſt winds prevailed at Perpignan and a weſterly wind at St. Louis. In Auguſt a north wind prevailed at Perpignan and an eaſt at Mont Louis. Mém. de la Société de Medecine de Paris 1780. Derham ſuſpected †, and Gentil has ſince ſhown, that changes of ſeaſons conſtantly begin in the upper atmoſphere; while a ſtrong wind blows from one point below, a wind from an oppoſite point reigns above, but more gentle, until at laſt (in about three weeks) it is propagated downwards. (Voy. ii. p. 23, 24, in 8vo.) The lower atmoſphere, he ſays, extends to the height of 2880 feet. (Vol. iv. p. 48.) At the commencement of winter, when the ſun approaches the ſouth tropic, and the north air begins to flow in and follow it, it muſt meet with more reſiſtance from the lower denſer air, as its impetuous courſe in an oppoſite direction is more ſlowly altered (this reſpects the monſoons) than in the rarer ſuperior ſtrata; and the ſame effect, but in a different direction, takes place when the ſun approaches to the northern tropic.

It has been ſaid by many, that winds in the ſuperior regions of the atmoſphere are much more violent and impetuous than in the lower. (Sauſſure Hygrom. p. 300: Ulloa's Voy. ii. p. 81: Muſchenbr. § 2612: Bergm. Erde kugel. ii. p. 99: De Luc, &c.) But the contrary has alſo been obſerved by Gentil, above quoted, and Morveau. (Aëroſt. de Dijon.)

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* Ulloa's Voy. ii. p. 62. Engliſh.

† Phil. Tranſ. Abridg. iv. part ii. 125.

Of the Succeſſion of Winds.

Well eſtabliſhed general laws on this head would be extremely uſeful, as we might then foreſee what wind might next be expected. Beſides the *general* ſucceſſion in an open country, it is probable there is a *local*, confined to certain ſituations.

Gentil remarks, that in the ſouthern latitudes of our hemiſphere, a north-eaſt is ſucceeded by an eaſt, ſouth-eaſt, and ſouth. According to La Cotte, the order of ſucceſſion in the middle latitudes is ſouth-weſt, north, weſt, north-eaſt, ſouth, north-weſt, eaſt, ſouth-eaſt. (Roz. Journ. xxxix. p. 267.)

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THE Galvanic experiments made on the 10th and 14th of Auguſt laſt, in the preſence of a great many ſpectators, by Giulio, Roſſi, and myſelf, on the head and trunk of three decapitated criminals, an account of which has been publiſhed, gave riſe to ſeveral queſtions in regard to this agent, and by analogy reſpecting electricity. Theſe two fluids, and the uſes to which they may be applied, have become a common ſubject of converſation among well-informed per-

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ſons,

ſons, and, as is always the caſe with new diſcoveries, their uſes are exaggerated by ſome, and deſpiſed by others. This diverſity of opinion induced our aſſociate Charles Boſſe to propoſe to me two queſtions, which I endeavoured to ſolve in the next letter I wrote to him about the end of September. I could eaſily increaſe the number of cures obtained by means of Galvaniſm and electricity, as well as of the misfortunes occaſioned by the action of theſe fluids and of the inſtances where I obſerved a conſtant relation between the moral part; that is to ſay, the ſtrength of mind, the courage of the victims to juſtice, and the effects of the Galvanic fluid on their bodies: but theſe obſervations belong to the general report of the experiments which we made ſince the 14th of Auguſt. I ſhall therefore only preſent to you the letter which I wrote at that period.

"You aſked me in one of our late academic ſittings, why, after ſo many experiments, made by the firſt philoſophers of the laſt century, on the electric fluid, ſuch a variety of opinions is entertained in regard to its medical action in the human body; and whether Galvaniſm ſeems already to promiſe reſults more uſeful to the healing art. I ſhall here give you my opinion on theſe queſtions, or rather ſubmit to you the inductions which different experiments made by myſelf, or at which I aſſiſted, gave me reaſon to deduce with more certainty than I durſt venture to hope when I began to pay a ſerious attention to this object.

"I conſider Galvaniſm as a modification of electricity—a modification which renders this fluid more active; as the ſmall flame ſeparated by the blow-pipe is far more ardent than the large one from which it is taken. I have read to the claſs ſeveral experiments which ſeem to ſupport this compariſon between electricity and Galvaniſm. Animals which were only ſtunned by the ſtrongeſt ſparks from a magic picture, were killed in leſs than three minutes by a very weak degree of Galvaniſm.

"The fluid of a pile compoſed of 25 plates of ſilver and zinc of the ſize of a crown-piece, intermixed with pieces of paſteboard moiſtened in water ſaturated with muriate of ammonia, oxidates the metals in decompoſing the water, while it is ſcarcely ſenſible in the fingers, and gives only ſmall ſparks. The brilliant electric ſparks which excite in our bodies a ſtrong ſenſation of pricking, neither oxidate the metals nor decompoſe the water if they do not communicate a ſhock. Having made the Galvanic current to paſs through the body of a frog, its fluids were decompoſed, and I ſaw it ſwell up ſo much that it could no longer plunge into the water though poſſeſſed of great vitality, which I never obſerved to be the caſe in frogs when tormented by ſtrong electric ſparks. All theſe facts, to which many more of a ſimilar kind might be added, confirm the great activity of Galvaniſm in compariſon of electricity. Hence it reſults, that the fluid of the Voltaic pile may be very uſeful in caſes in which common electricity would not have ſufficient activity. You are acquainted with ſome of the experiments which I made in conjunction with my colleagues Giulio and Roſſi: we made others, ſtill more intereſting, which have determined phyſiological facts, before doubtful for want of being verified. We then tried an application of it in ſeveral diſeaſes with the greateſt ſucceſs. Three of the caſes are as follow:

"A lady about thirty years of age, after ſevere pains in the head, loſt the ſight of the right eye. C. Roſſi being conſulted in regard to this malady, after a cloſe examination of the eye, which appeared to be as ſound and to look as well as the left, concluded that it muſt ariſe from a palſy of the optic nerve, or what is called a *gutta ſerena*, which ſuffered the patient to ſee only, as it were, through a thick miſt; which increaſed her misfortune, ſince it deranged the ſight of the other eye, ſo that ſhe was always afraid of falling, not being able to diſtinguiſh well with the right eye the objects which ſhe handled. Roſſi being ſick, ſent her to me, that I might make an application of Galvaniſm. I formed a pile of thirty pairs of plates like thoſe already mentioned, and employing gold wire as conductors, I cauſed the Galvanic current to enter near the exterior angle of the eye, and to iſſue ſometimes at the eyebrow, ſometimes by the ophthalmic ramus which paſſes through the orbital foramen, and ſometimes near the interior angle of the ſame eye. The operation was very painful; it cauſed abundance of tears to flow; but, after ſucceſſive Galvanic ſhocks for half an hour, the eye began to ſee a little better. That I might not fatigue my patient too much, and that nature might have time to act, the operation was ſuſpended till the evening, when it was repeated for half an hour. The next day the eye began to diſtinguiſh the figure of bodies. Having repeated the operation for three days following, the lady was not only able to diſtinguiſh the figure of bodies, and people's features, but alſo the pupils of their eyes. Before this operation, in conſequence of a conſultation with Dr. John Baptiſt Anſorini, firſt phyſician of the Hoſpital de la Charité, I had Galvanized a young woman, twenty-ſeven years of age, of a melancholy temperament, who, after ſome ſlight attacks, had a hemiplegia of the right ſide, which affected in particular the arm, the cheek, and the eye. The other ſymptoms were removed by bleeding and the uſe of the remedies uſual in ſuch caſes; but the eye always remained fixed, with pains in the muſcles. The application of Galvaniſm for ten minutes was ſufficient to excite abundant tears and a diſcharge of a watery fluid from the noſe on the ſide which had always been ſhut ſince the attack of the diſorder, and greatly alleviated the pains of the muſcles. She could even turn her eye to both ſides; but ſhe found great difficulty in raiſing or lowering it, with a ſenſation of heavineſs all around the eye. This operation being repeated, after the interval of a day the eye acquired its former freedom of motion, and the patient was freed from every ſenſation of uneaſineſs.

" Theſe two operations were performed in the preſence of ſeveral perſons, and executed almoſt entirely by C. Hyacinth Carena, lecturer on natural philoſophy in the National College of Turin.

" The advantages of Galvaniſm will appear to you ſtill more deciſive, by the cure of a perſon labouring under hydrophobia, performed lately by C. Roſſi, who will give a full and complete account of it in a memoir on which he is now employed.

" A man bit in the finger by a mad dog, came to conſult him, in conſequence of a pain which he felt in the arm, the back, and particularly the finger, which had been bit more than a month. A cauſtic applied to the finger removed the pain; but a few days after it returned, accompanied with ſymptoms of hydrophobia. The patient could no longer look at water without horror; an inflammation in the throat prevented him from ſwallowing even chewed bread, and he experienced a ſtrong propenſity to bite thoſe around him.

" In this ſtate he was brought to C. Roſſi, who obſerving that he could not bear the ſight of water, nor that even of ſhining bodies, provided in another room a pile conſiſting of 50 pairs of plates of ſilver and zinc intermixed with 50 pieces of paſteboard moiſtened with a ſolution of muriate of ammonia. He employed ſlips of brown paper moiſtened as a conductor, on which the naked feet of the patient were placed, and at the moment when he opened his mouth to bite, one end of the arc was thruſt into it, while the other communicated with the pile. The patient ſuffered a great deal from this operation, which after ſeveral ſhocks weakened him ſo much that he could no longer ſupport it. Being ſtretched out on the floor, he was then Galvanized with eaſe: the operation made the ſweat run from him in drops. After ſome time Roſſi cauſed the patient to be conveyed home, and gave orders, that he ſhould be brought back next day, that the operation might be repeated. It was two o'clock in the afternoon when the patient was Galvanized, and at ſix next morning he came to Roſſi himſelf to tell him that he was completely cured, as he experienced no pain or difficulty of ſwallowing, and was entirely freed from his averſion to water and to liquids: no perſuaſion, however, could induce him to ſubmit to a new operation.

"But a few days after, ſome ſlight pains having given him reaſon to apprehend a new attack of hydrophobia, he returned to Roſſi, who by repeating the operation made all the ſymptoms diſappear. This cure was alſo effected in the preſence of ſeveral perſons. The patient was endowed with ſo great ſenſibility, that for more than a month after he felt in the ſhoulders a ſenſation of the Galvanic ſhocks, which I felt only as far as the articulation of the finger, though I am not one of the leaſt ſenſible. You ſee by theſe trials what are the advantages which may be hoped for from Galvaniſm. I entertain no doubt that a mean ſo active may preſerve from the grave many individuals, by Galvanizing them at the moment when the play of the vital organs is ſuſpended by an accidental cauſe.

"This will become more evident by an explanation of the medical action of electricity on the human body.

"Several celebrated writers have claſſed electricity among thoſe remedies which are moſt certain and moſt active; others have ſhown the inutility, and even danger, of this fluid conſidered as a remedy; and both ſeem to be ſupported by well atteſted facts.

"Nothing, however, can be more eaſily explained, if we reflect, that moſt of thoſe who have applied electricity to medicine have been guided by quackery, without conſulting the nature of the diſeaſe, or of the agent which they employed. For this reaſon I ſtated in the memoir which is about to appear in the Tranſactions of the Academy, that the greateſt circumſpection ſhould be obſerved in the uſe of Galvaniſm, which, like electricity, may be attended with bad conſequences; and even advanced, that the latter, though a very good remedy of itſelf, has done more hurt than good by improper application.

"I ſhall not here ſpeak of the chemical properties which during the enthuſiaſm of novelty has been aſcribed to electricity, ſuch as that of transfuſing into the human body the action of remedies encloſed in glaſs tubes, by rubbing it with them.

"It is well known that it is the fate of new diſcoveries to be exaggerated, ſo that their adverſaries eaſily find reaſons to oppoſe them; but after ſome time things come to their proper level, eſtabliſhed on a more complete knowledge of the agent; and thoſe well acquainted with the properties of electricity are able to diſtinguiſh the caſes in which it may be adminiſtered with advantage, from thoſe in which it would be only prejudicial. Of ten patients affected with the ſame diſeaſe, which undergo the ſame electric treatment, five may be entirely cured, and the other five be exceedingly ill.

"Thoſe who are cured extol electricity as the beſt of remedies; thoſe who have ſuffered, ſay that it only aggravates the evil. Both ſpeak from correct experience, and at the ſame time right and wrong, for attempting too much to generalize; that is to ſay, becauſe they do not diſtinguiſh the cauſe of the diſeaſe which requires or oppoſes the application of electricity. Thus the ſame pain, the ſciatica * for example, may be occaſioned either by a ſtagnation of the fluids, by their too great abundance, or by the want of reaction in the ſolids: it may ariſe alſo from organic defects; an alteration of the fluids; a poiſonous, or, as it is called, *acrid* principle; or from a peculiar virus in the fluids.

"The five patients who labour under a ſtagnation of the fluids, receive the greateſt relief from electricity, which puts them in motion; the other five, tormented by ſciatica occaſioned by vitiated humours, will grow worſe under electric treatment, which will increaſe the acridity of the humours, carrying off a part of the water which kept the poiſon diluted. This theory of the effects of electricity in the human body is founded on the nature of this fluid, and on its properties eſtabliſhed by numerous experiments. The electric fluid tends always to put itſelf into a ſtate of equilibrium, and this tendency is ſo ſtrong, that it penetrates to a certain diſtance in the air, and extends along idio-electric bodies. It is this tendency which cauſes water when electrified to flow from capillary ſyphons, whereas a very few drops only flow without this electrization. It is by the ſame tendency that the electric ſpark, when it paſſes from one conducting body to another by non-conducting ſubſtances, carries with it in its paſſage conducting particles which ſerve it as a vehicle, if the ſolidity of the bodies does not oppoſe a very ſtrong reſiſtance. This property, proved by the common effects of thunder, and by ſeveral experiments, ſerves to account for the great evaporation of electrified liquids, and for the greater perſpiration of animals and vegetables which have been electrified. It is then evident, that whenever a ſtagnation of the fluids takes place, if other ſymptoms do not oppoſe it, electricity will be a good remedy: on the other hand, if the diſeaſe ariſes from vitiated fluids, or a virus diluted in them, electricity in this caſe, either by the evaporation of the diluting fluid, or by the greater alteration it may produce, will be hurtful. From what has been ſaid, it is evident that electricity and Galvaniſm ought to be employed with the greateſt circumſpection, and that the nature of the diſeaſe ought to be compared with the nature and properties of theſe fluids, to aſcertain whether the application of them is proper or not. It is to be obſerved alſo, that this remedy, in conſequence of its activity, may be dangerous, like all other remedies, however good, if abuſed.

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* Dr. Balbis obſerved to me, that ſciatica of every kind may be accounted for without recurring to the hypothetic alteration of the fluids. I replied, that I wiſhed only to compare my theory with the principal theories of ſciatica, without concerning myſelf with their probability.

"I could adduce ſeveral inſtances of misfortunes occaſioned by the abuſe of electrization, even in caſes in which a little time before it had been indicated; but I ſhall mention only one fact in regard to Galvaniſm:—A young woman was cured by Galvaniſm of pains which ſhe experienced in the muſcles of the face. After the cure, having continued to Galvanize herſelf, ſhe did hurt, which increaſed with the application of the Galvaniſm, and did not ceaſe till ſhe abandoned herſelf entirely to the powers of nature, aſſiſted by good nouriſhment. The patient then, who is incapable of forming a proper opinion reſpecting the ſtate of his health, ſhould conſult a good phyſician, one of thoſe who do not deſpiſe natural philoſophy and the new diſcoveries, in order that he may never ſuffer by the application of electricity or of Galvaniſm, which, as Boerhaave ſays of another very active remedy, *Mira præſtat in multis incurabilibus; at prudenter à prudenti medico abſtine ſi methodum neſcis* *.

LVIII. *Account of ſome Remains of a Species of gigantic Oxen found in America and other Parts of the World.* By *Mr.* Rembrandt Peale †.

AMONG the remains of gigantic and unknown animals found in America, we have lately diſcovered one of the ox or buffalo kind, which was taken from the bed of a creek falling into the Ohio, 12 or 14 miles north of Bigbone-lick, and preſented by Samuel Brown, of Kentucky, to the Philoſophical Society at Philadelphia. By permiſſion of the ſociety I made a plaſter caſt of this extraordinary bone, which I have now with me in London, and of which I ſend you an accurate drawing, repreſenting the back part of the head, with the condyles of the neck, and the pith or internal part of the left horn at the baſe. (See Plate VI.) The right horn is broken off, and all the fore part of the head; but from the fragment remaining it is a reaſonable conjecture that the buffalo to which it belonged was about 10 or 11 feet high. The horn at the baſe meaſures 21 inches in circumference, and tapers very gently towards the extremity, where it is broken off; ſo that the horn could not have been leſs than ſix feet in length. From the middle ſuture on the head to the baſe of the horn meaſures 7½ inches; conſequently the two horns were 15 inches diſtant; which muſt have been increaſed when they were partly covered with fleſh, ſkin, and hair.

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* Elem. Chemiæ, pars iii. proceſſus 198.

† Communicated by the Author.

It is very extraordinary that bones of this kind have been occaſionally found in Siberia, in Italy, Germany, and other parts of Europe, though not quite ſo large as this American bone; which circumſtance, ſtrengthened with others of a ſimilar nature, muſt prove, either that theſe great animals have inhabited thoſe various countries, or that their remains have been forcibly ſcattered by the action of water.

Buffon informs us *, that in the pariſh of Haux, a mile and a half from Langoiran, in the ſplitting of a great rock, ſome large bones, moſtly petrified, were diſcovered; probably of the ox kind, but of a very great magnitude. He likewiſe mentions †, that in 1772 there was found near Rome an ox's head in a ſtate of petrifaction. The length of the forehead between the two horns was 2 feet 3 inches; the diſtance between the orbits of the eyes 14 inches; that from the upper portion of the forehead to the orbit of the eyes, 1 foot 6 inches; the circumference of one horn, 18 inches; the length of the ſame following the curve, 4 feet. "This inſtance is ſufficient to prove," ſays Buffon, "that there have been prodigious giants among this ſpecies of animals; but it is further confirmed by other facts." He then enumerates ſeveral bones of the ſame kind in the muſeum at Paris ſimilar to ſome which I have remarked in the Britiſh muſeum.

In the Philoſophical Tranſactions ‡ there is an engraving and an account of a bone of this kind, found near the city of Dirſchaw,

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* Quarto Supplement, vol. v. p. 486.

† Page 543.

‡ Vol. xxxvii. p. 427.

Dirſchaw, of very large dimenſions, and having the ſame characters which diſtinguiſh the one found on the Ohio.

Until the diſcovery of this bone in America, the tradition of the Indians concerning the great buffalo has been conſidered as entitled to very little attention. Many interpreted it as having entire reference to the mammoth, whoſe preeminent ſize was obvious, and whoſe carnivorous teeth were well calculated to excite terror; but I have now no heſitation in believing that the tradition, which, with ſuch little variation, prevails through all North America, mentioning the antient exiſtence of a great buffalo, is a tradition really handed down to them from their forefathers, but, like all other traditions, clouded with fable: yet it is not improbable, ſince we find the remains of the mammoth and the great buffalo in the ſame country, that the diſtinct ideas of each have been in time confounded, the terrible *power* of the one with the *name* of the other.

It has been too much the cuſtom, whenever any large bones have been found in Europe, to call them all elephants' bones; and in America, to think them all belonging to the mammoth: but from the progreſs now made in this inquiry there is reaſon to hope for additional light on this intereſting ſubject, whether it be conſidered as a foundation to theological faith, or regarded as a conſpicuous monument in the hiſtory of the world.

LIX. *Deſcription of the* Nymphæa Cærulea. *By* JULIUS CÆSAR SAVIGNY, *of the Inſtitute of Egypt*[*].

It is well known that the lotus of the antient Egyptians was one of the moſt celebrated plants of antiquity. Riſing every year with the waters of a river which overflowed its banks only to fecundate the earth; ſpringing up amidſt plains formerly deſert, which it embelliſhed with its beautiful flowers; and cultivated to ſerve as food to the leaſt ſenſual, but the moſt numerous claſs of the inhabitants; it was judged worthy of homage by one of the firſt people in the world, who conſidered it as the happy ſign of abundance, and as a ſacred pledge of the favour of the gods.

It is to the genus of the *nymphæa* that the modern botaniſts have referred the lotus, which has been deſcribed by moſt of the antient hiſtorians, and which is engraven on all the antique monuments of Egypt. Two ſpecies of this genus, one with white and the other with azure flowers, ſtill ornament, during the time of the inundation, the ſurface of the canals and rivers, and in general of all the fields of Lower Egypt, which are covered by the waters of the river. Plants ſo ſimilar were no doubt beheld by the Egyptians with equal veneration; and though Linnæus, who was well acquainted with the former, gave it the name of *lotus*, it is to be preſumed that the ſecond procured the ſame advantages and ſerved for the ſame myſteries. Its flowers have more ſplendour and a ſweeter ſmell, and the azure colour they exhibited might become, among a people ſo religious, the emblem of the reſidence of the deity on the waters.

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[*] From *Annales du Muſeum National,* No. 5.

The *nymphæa lotus* is very well deſcribed, and a good figure is given of it, in ſeveral Syſtems of Botany; but the caſe is not the ſame with thoſe the flowers of which are azure-coloured. It was ſcarcely obſerved by a few travellers, and if mentioned, it was only in a vague and incorrect manner. Forſkal, who travelled through Egypt in 1761, and who gave a Flora of that country, did not obſerve it. His ſilence in this reſpect is the more aſtoniſhing, as the plant in queſtion and the *nymphæa lotus* grow together and uſually intermix their leaves. It is not even indicated in the laſt edition of the *Syſtema Naturæ*, publiſhed by Gmelin. This author, however, collected with great attention all the ſcattered ſpecies to be found in the different works which have hitherto appeared *.

It is therefore indiſpenſably neceſſary that a name and ſpecific character ſhould be aſſigned to this nymphæa; it is alſo neceſſary that a correct deſcription ſhould be given of it: this, in ſome meaſure, will be enriching botany with a new plant; and this, indeed, is the object which I propoſe in the preſent memoir.

But as this ſpecies has a great reſemblance to the *nymphæa lotus*, and as it is poſſible that they may have often been confounded, I have thought it will be of advantage to give a comparative deſcription of both. By theſe means the reader will be better enabled to comprehend the characters by which they are diſtinguiſhed, and which render it neceſſary to form of them two ſeparate ſpecies.

The root of the *nymphæa lotus*, and that of the other ſpecies, which from the colour of its flowers I have called the azure nenuphar (*nymphæa cærulea*), conſiſt of long, white, pulpy filaments, the upper extremities of which adhere to round tubercles. In ſeveral provinces of Egypt theſe tubercles,

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* Wildenow in his *Species Plantarum* does not ſpeak of this nymphæa.

cles, and particularly thoſe of the firſt ſpecies, are plucked up after the inundation: they are uſed as food; they have an inſipid earthy taſte, and form a delicate diſh.

The leaves of both are large, pretty numerous, ſub-orbicular, divided into two lobes from their baſe to the place where the petiole is inſerted; that is to ſay, nearly to the middle of the diſk. They are not ſo thick as thoſe of our nenuphars in Europe: they have a darker green colour, and a ſhining appearance above; and below are often tinged with purple or violet. Theſe leaves are ſupported by radical cylindric petioles, ſlightly compreſſed and ſometimes very long, which float on the ſurface of the water.

The flowers are very large, and are often more than four inches in diameter. Their pedicles, which ariſe from the root, are uniflorous and differ very little from the petioles.

The calyx of theſe flowers is compoſed of eight leaves diſpoſed in two rows, and coloured in the inſide: the more interior ones are the moſt coloured, and have ſome reſemblance to the petioles.

The latter, from twelve to twenty in number, are alſo diſpoſed in ſeveral ſeries.

The ſtamina are of a yellow colour with large filaments, and petaliform.

The radii of the ſtigma vary, for the moſt part, in number from twelve to twenty-five. They have the ſame colour as the ſtamina, are compreſſed and ſlightly bent at the ſummit.

Such is the common appearance of the roots, the leaves, and the flowers. Let us now examine the differences they exhibit.

The leaves of the *nymphæa lotus* are bordered quite round with very acute indentations, firm and almoſt like prickles: their lobes, for the moſt part, are exactly parallel and ſometimes even; they converge, and mutually cover each other: their inferior ſurface is charged with very prominent ribs, which form a very apparent reticulation: their petioles are rough.

On the other hand, thoſe of the *nymphæa cærulea* have their edges ſcarcely ſinuated; their lobes are more pointed, and commonly divergent. The inferior ſurface exhibits ribs ſcarcely ſenſible, the principal ones of which only are ſomewhat prominent; the reſt being concave and leſs elevated than the diſk. The petioles of theſe leaves are exceedingly rough.

In the *nymphæa lotus* the leaves are always more open. The folioles of the calyx are oval, lanceolated, greeniſh externally,

nally, without ſpots, but marked with ſeven paler ribs very diſtinct.

The folioles of the calyx in the *nymphæa cærulea* are much narrower, lanceolated, and almoſt cuneiform: the exterior ſide of them is of a dark green colour, conſtantly varied with an infinite number of points and ſmall lines of a purpliſh colour, and have no apparent ribs.

The petals of the *nymphæa lotus* are oval, lanceolated, and very unequal; thoſe of the laſt being much ſmaller than the reſt: their colour is a pure milk-white, tinged with a greeniſh purple colour on the outſide, but rarely.

In the *nymphæa cærulea* the petals are lanceolated all perfectly equal, of a bright white colour, tinged, in particular towards the ſummit, with the moſt beautiful azure, inclining ſlightly however, ſometimes, to violet.

The ſtamina of the *nymphæa lotus* have antheræ very much compreſſed, lanceolated, without any appendix at the ſummit, and have ſcarcely the length of the filaments.

On the other hand, the antheræ of the *nymphæa cærulea* are very little compreſſed, lineal and longer than the filaments; their ſummit is terminated by a ſubulated blueiſh appendix ſimilar to a ſmall petal.

In the laſt place, the ſummits of the radii of the ſtigma are longer and ſubulated in the *nymphæa lotus*; ſhorter, oval, and lanceolated, in the *nymphæa cærulea.*

The ſmell exhaled from the flowers of each kind is alſo very different: that of the *nymphæa cærulea* is exceedingly ſweet and agreeable; that of the *nymphæa lotus* is ſtronger, more pungent, and much leſs agreeable.

In regard to the fruit, I did not find in them any very remarkable difference: in both ſpecies it conſiſts of a dry round berry, which is long, covered by the baſes of the folioles of the calyx and thoſe of the petals; truncated and radiated at the ſummit, which is always ſtained by the decompoſition of the ſtamina and interior petals: it is divided into ſeveral cells, each of which correſponds to a radius of the ſtigma, and contains a quantity of round ſeeds of a roſe colour.

The Arabs have very well diſtinguiſhed theſe plants, and given to each a particular name: the former they call *neouſar*, and the latter *bachenin*.

By ſelecting from the deſcription here given the moſt prominent differences, both ſpecies may be characterized as follows:

NYMPHÆA LOTUS,

N. *foliis dentatis, antheris apice ſimplicibus.*

NYMPHÆA CÆRULEA,

N. *foliis repandis, antheris apice ſubulato-petaloïdeis.*

Explanation of the Plate. (VII.)

1. A petal.
2. A ſtamen of the interior ſeries.
3. A ſtamen of the exterior ſeries.
4. The ovarium and ſtigma.
5. The ſeeds.
6. A ſection of the pedicle.

LX. *On Mr.* GREATHEAD'S *Life-Boat**.

SIR,

A CONSIDERABLE time has elapſed ſince I had the honour to lay before the Society a model of the life-boat of my invention.

I have now encloſed a particular account of its conſtruction in a letter from Mr. Hinderwell, explaining upon what principle it is built, ſo as to render it ſuperior to any other form of a boat for the dangerous enterpriſes for which it was intended, and has been uſed.

I am, Sir, your humble ſervant,
HENRY GREATHEAD.

South Shields,
Jan. 1, 1802.

To Mr. Charles Taylor.

SIR,

IT is much to be lamented, that in an age enlightened by ſcience, ſuch a languid indifference ſhould prevail on many important public occaſions; and that the moſt excellent inventions ſhould have to combat the force of inveterate prejudice.

How many valuable diſcoveries have languiſhed in obſcurity! How many uſeful projects have periſhed in embryo, deprived of the foſtering aid of the public, and the patronage of influence and authority! In the claſs of uſeful improvements for the diminution of the dangers incident to a maritime profeſſion, the life-boat, invented by Mr. Greathead, of Shields, has a claim to a diſtinguiſhed patronage. An experimental conviction of its great utility in ſaving the lives of ſhipwrecked ſeamen, and of its perfect ſafety in the moſt agitated ſea, has induced me to advocate the cauſe with a zeal proportioned to its importance; and it is a conſolatory reflection to my own mind, that my exertions have been ſucceſsful in the introduction of a life-boat in the port of Scarborough, and, I truſt, not unprofitable towards promoting a ſimilar eſtabliſhment in other places. The ſervices which have been recently performed at this port by means of the life-boat, in contributing to the preſervation of the lives of the crews of two veſſels, more than compenſate for every labour. I am far from the ambition of aſpiring to any honorary teſtimony on this occaſion. Actuated by the pureſt principle of philanthropy, my ſole object is the benefit of the community, and to endeavour, by ardent recommendations, to excite a ſpirit of emulation in order to introduce the life-boat, with its invaluable properties, into more general uſe. I am induced to ſubmit, with the utmoſt deference and reſpect, to the conſideration of the Society of Arts, &c. the following deſcription of the life-boat, with ſome miſcellaneous obſervations. The conſtruction of the boat, agreeably to Mr. Greathead's plan, is as follows:

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* From the *Tranſactions of the Society for the Encouragement of Arts, &c. for* 1802.—The Society's gold medal and fifty guineas were voted to Mr. Greathead for this invention.

The length is thirty feet; the breadth, ten feet; the depth, from the top of the gunwale to the lower part of the keel in midſhips, three feet three inches; from the gunwale to the platform (within), two feet four inches; from the top of the ſtems (both ends being ſimilar) to the horizontal line of the bottom of the keel, five feet nine inches. The keel is a plank of three inches thick, of a proportionate breadth in midſhips, narrowing gradually toward the ends to the breadth of the ſtems at the bottom, and forming a great convexity downwards. The ſtems are ſegments of a circle, with conſiderable rakes. The bottom ſection, to the floor-heads, is a curve fore and aft, with the ſweep of the keel. The floor-timber has a ſmall riſe curving from the keel to the floor-heads. A bilge-plank is wrought in on each ſide next the floor-heads with a double rabbet or groove of a ſimilar thickneſs with the keel; and on the outſide of this are fixed two bilge-trees, correſponding nearly with the level of the keel. The ends of the bottom ſection form that fine kind of entrance obſervable in the lower part of the bow of the fiſhing-boat, called a *colle*, much uſed in the North. From this part to the top of the ſtem it is more elliptical, forming a conſiderable projection. The ſides, from the floor-heads to the top of the gunwale, flaunch off on each ſide, in proportion to about half the breadth of the floor. The breadth is continued far forward towards the ends, leaving a ſufficient length of ſtrait ſide at the top. The ſheer is regular along the ſtrait ſide, and more elevated towards the ends. The gunwale, fixed on the outſide, is three inches thick. The ſides, from the under part of the gunwale along the whole length of the regular ſheer, extending twenty-one feet ſix inches, are caſed with layers of cork to the depth of ſixteen inches downward; and the thickneſs of this caſing of cork being four inches, it projects at the top a little without the gunwale. The cork on the outſide is ſecured with thin plates or ſlips of copper, and the boat is faſtened with copper nails. The thwarts, or ſeats, are five in number, double banked, conſequently the boat may be rowed with ten* oars. The thwarts are firmly ſtanchioned. The ſide oars are ſhort†, with iron tholes and rope grommets, ſo that the rower can pull either way. The boat is ſteered with an oar at each end; and the ſteering oar is one-third longer than the rowing oar. The platform placed at the bottom within the boat is horizontal, the length of the midſhips, and elevated at the ends for the convenience of the ſteerſman, to give him a greater power with the oar. The internal part of the boat next the ſides, from the under part of the thwarts down to the platform, is caſed with cork; the whole quantity of which affixed to the life-boat is nearly ſeven hundred weight. The cork indiſputably contributes much to the buoyancy of the boat, is a good defence in going alongſide a veſſel, and is of principal uſe in keeping the boat in an erect poſition in the ſea, or rather of giving her a very lively and quick diſpoſition to recover from any ſudden eant or lurch which ſhe may receive from the ſtroke of a heavy wave. But excluſive of the cork, the admirable conſtruction of this boat gives it a decided pre-eminence. The ends being ſimilar, the boat can be rowed either way; and this peculiarity of form alleviates her in riſing over the waves. The curvature of the keel and bottom facilitates her movement in turning, and contributes to the eaſe of the ſteerage, as a ſingle ſtroke of the ſteering oar has an immediate effect, the boat moving as it were upon a centre. The fine entrance below is of uſe in dividing the waves, when rowing againſt them; and, combined with the convexity of the bottom and the elliptical form of the ſtem, admits her to riſe with wonderful buoyancy in high ſea, and to launch forward with rapidity, without ſhipping any water, when a common boat would be in danger of being filled. The flaunching, or ſpreading form of the boat, from the floor-heads to the gunwale, gives her a conſiderable bearing; and the continuation of the breadth, well forward, is a great ſupport to her in the ſea; and it has been found by experience, that boats of this conſtruction are the beſt ſea-boats for rowing againſt turbulent waves. The internal ſhallowneſs of the boat from the gunwale down to the platform, the convexity of the form, and the bulk of cork within, leave a very diminiſhed ſpace for the water to occupy; ſo that the life-boat when filled with water contains a conſiderable leſs quantity than the common boat, and is in no danger either of ſinking or overturning. It may be preſumed by ſome, that in caſes of high wind, agitated ſea, and broken waves, that a boat of ſuch a bulk could not prevail againſt them by the force of the oars; but the life-boat, from her peculiar form, may be rowed a-head when the attempt in other boats would fail. Boats of the common form, adapted for ſpeed, are of courſe put in motion with a ſmall power, but for want of buoyancy and bearing are overrun by the waves and ſunk, when impelled againſt them; and boats conſtructed for burden meet with too much reſiſtance from the wind and ſea when oppoſed to them, and cannot in ſuch caſes be rowed from the ſhore to a ſhip in diſtreſs. An idea has been entertained, that the ſuperior advantages of the life-boat are to be aſcribed ſolely to the quantity of cork affixed. But this is a very erroneous opinion; and, I truſt, has been amply refuted by the preceding obſervations on the ſupereminent conſtruction of this boat. It muſt be admitted, that the application of cork to common boats would add to their buoyancy and ſecurity; and it might be a uſeful expedient, if there were a quantity of cork on board of ſhips, to prepare the boats with in caſes of ſhipwreck, as it might be expeditiouſly done, in a temporary way, by means of clamps, or ſome other contrivance. The application of cork to ſome of the boats of his majeſty's ſhips* might be worthy of conſideration; more particularly as an experiment might be made at a little expenſe, and without inconvenience to the boats; or may prevent pleaſure boats from upſetting or ſinking.

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* Five of the benches are only uſed, the boat being generally rowed with ten oars.

† The ſhort oar is more manageable in a high ſea than the long car, and its ſtroke is more certain.

The life-boat is kept in a boat-houſe, and placed upon four low wheels, ready to be moved at a moment's notice. Theſe wheels are convenient in conveying the boat along the ſhore to the ſea; but if ſhe had to travel upon them on a rough road, her frame would be exceedingly ſhaken. Beſides, it has been found difficult and troubleſome to replace her upon theſe wheels on her return from ſea. Another plan has therefore been adopted. Two wheels, of nine feet diameter, with a moveable arched axis, and a pole fixed thereto for a lever, have been conſtructed. The boat is ſuſpended near her centre between the wheels under the axis, toward each extremity of which is an iron pin with a chain attached. When the pole is elevated perpendicularly, the upper part of the axis becomes depreſſed, and the chains being hooked to eye-bolts on the inſide of the boat, ſhe is raiſed with the utmoſt facility by means of the pole, which is then faſtened down to the ſtem of the boat.

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* The launches.

The Scarborough boat is under the direction of a committee. Twenty-four fiſhermen, compoſing two crews*, are alternately employed to navigate her. A reward, in caſes of ſhipwreck, is paid by the committee to each man actually engaged in the aſſiſtance; and it is expected that the veſſel receiving aſſiſtance ſhould contribute to defray this expenſe. None have hitherto refuſed.

It is of importance that the command of the boat ſhould be intruſted to ſome ſteady, experienced perſon, who is acquainted with the direction of the tides or currents, as much ſkill may be required in riſing them to the moſt advantage in going to a ſhip in diſtreſs. It ſhould alſo be recommended, to keep the head of the boat to the ſea as much as circumſtances will admit; and to give her an accelerated velocity to meet the wave. Much caution is neceſſary in approaching a wreck, on account of the ſtrong reflux of the waves, which is ſometimes attended with great danger. In a general way, it is ſafeſt to go on the lee quarter; but this depends upon the poſition of the veſſel; and the maſter of the boat ſhould exerciſe his ſkill in placing her in the moſt convenient ſituation. The boatmen ſhould practiſe themſelves in the uſe of the boat, that they may be the better acquainted with her movements; and they ſhould at all times be ſtrictly obedient to the directions of the perſon who is appointed to the command.

The great ingenuity which has been diſplayed in the conſtruction of the life-boat, leaves ſcarcely any room for improvement; but ſome have ſuppoſed that a boat of twenty-five feet in length, with a proportionate breadth, would anſwer every purpoſe of a larger one. A boat of theſe dimenſions would certainly be lighter, and leſs expenſive; but whether ſhe would be equally ſafe and ſteady in a high ſea I cannot take upon myſelf to determine.

Mr. Greathead, of South Shields, the inventor, undertakes to build theſe boats, and to convey them to any port in the kingdom. He is a worthy man, in whom a confidence may be repoſed, and will build upon moderate terms of profit.

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* Two crews are appointed, that there may be a ſufficient number ready in caſe of any abſence.

THOMAS HINDERWELL.

Deſcription of the Life-Boat.

EEE (Plate VIII.), the ſheer or curve of the boat.

II, the two ſtems or ends.

K, the keel.

LL, the aprons, to ſtrengthen the ſtems.

MM, the ſheets, or places for paſſengers.

NN, timber-heads, or boat-faſtenings.

OOOOO, the tholes on which the oars are ſlung by grommets.

T, flooring under the rowers feet.

Fig. 2. a croſs ſection of the life-boat.

FF, the outſide coatings of cork.

GG, the inſide cork filling.

HH, the outſide planks of the boat.

I, one of the ſtems of the boat.

K, the keel.

NN, the timber heads.

P, the thwarts, or rowers ſeats.

R, one of the ſtanchions under the thwarts, each being thus firmly ſupported.

S, a ſection of the gang-board, which croſſes the thwarts, and forms the paſſage from one end of the boat to the other.

T, the floor-heads, or platform for the rowers feet.

VV, the two bilge-pieces, nearly level with the keel.

WW, the gunwales.

X, a ring-bolt for the head-faſt, there being another alſo at the other end.

Y, platform for the ſteerſman.

Fig. 3. a truck or carriage with four wheels, to convey the boat to and from the ſea.

a, an oblong frame of wood conſiſting of two long pieces, hollowed a little to admit the body of the boat, and ſecured by the croſs pieces bb.

cccc, four low wheels, each ſunk or hollowed in the middle, to run better upon a rail-way or timber-road.

dd, two indents made in the ſide timbers, that the bottom of the boat may lie firm therein.

ee, two ſmall rollers, moveable in the croſs timbers for the keel of the boat to ſlide upon.

ff, the

ff, two long rollers, one at each end of the frame, to aſſiſt in raiſing the boat upon or ſliding it off the truck or carriage.

Management of the Life-Boat from the Boat-Houſe to the Sea, and vice verſâ, *as practiſed at Loweſtoffe, in Suffolk.*

The life-boat may be launched from any beach, when wanted, with as much eaſe as any other boat, by proper aſſiſtance. The diſtance from the boat-houſe at Loweſtoffe to the ſhore is one hundred yards, and the boat's crew can run her down in ten minutes. When the ſea does not tumble in upon the beach very much, the boat may be eaſily launched by laying the ways as far as poſſible in the water, and hauling the carriage from under her.

When there is a great ſea on the beach, the boat muſt be launched from the carriage before ſhe comes to the ſurf, on planks laid acroſs, as other boats are launched; the people ſtanding on the ends to prevent the ſea moving them; then, with the aſſiſtance of the anchor and cable (which ſhould be laid out at ſea for the purpoſe), the boat's crew can draw her over the higheſt ſea.

Upon the boat returning to the ſhore, two double blocks are provided; and, having a ſhort ſtrop fixed in the hole, in the end of the boat next the ſea, the boat is eaſily drawn upon the carriage. The boat's crew can run her any diſtance upon a clear ſhore by the carriage of Mr. Greathead's contrivance.

Account of, and Inſtructions for, the Management of the Life-Boat.

The boats in general of this deſcription are painted white on the outſide, this colour more immediately engaging the eye of the ſpectator at her riſing from the hollow of the ſea than any other. The bottom of the boat is at firſt varniſhed (which will take paint afterwards), for the more minute inſpection of purchaſers. The oars ſhe is equipped with are made of fir of the beſt quality, having found by experience that a rove-aſh oar that will dreſs clean and light is too pliant among the breakers; and when made ſtrong and heavy, from rowing double banked, the purchaſe being ſhort, ſooner exhauſts the rower, which makes the fir oar, when made ſtiff, more preferable.

In the management of the boat, ſhe requires twelve men to work her; that is, five men on each ſide, rowing double banked, with an oar ſlung over an iron thole, with a grommet (as provided) ſo as to enable the rower to pull either way; and one man at each end to ſteer her, and to be ready at the oppoſite end to take the ſtern oar when wanted. As, from the conſtruction of the boat, ſhe is always in a poſition to be rowed either way, without turning the boat, when manned, the perſon who ſteers her ſhould be well acquainted with the courſe of the tides, in order to take every poſſible advantage: the beſt method, if the direction will admit of it, is to head the ſea. The ſteerſman ſhould keep his eye fixed upon the wave or breaker, and encourage the rowers to give way as the boat riſes to it; being then aided by the force of the oars, ſhe launches over it with vaſt rapidity, without ſhipping any water. It is neceſſary to obſerve, that there is often a ſtrong reflux of ſea, occaſioned by the ſtranded wrecks, which requires both diſpatch and care in the people employed, that the boat be not damaged. When the wreck is reached, if the wind blows to the land, the boat will come in ſhore without any other effort than ſteering.

I would ſtrongly recommend practiſing the boat, by which means, with experience, the danger will appear leſs, from the confidence people will have in her from repeated trials.

LXI. *A general View of the Coal Mines worked in France, of their different Products, and the Means of circulating them. By* C. Lefebvre, *Member of the Council of Mines, of the Philomatic Society, &c. &c.*

[Continued from p. 240.]

Department of Ille and Vilaine.

No coals are dug up in this department. It may receive this fuel by ſea in the northern part; and the mines of Montrelais and North, in the department of Loire-Inferieuſe, may ſupply the ſouthern part.

Department of Indre.

This department is in the ſame ſituation as the preceding in regard to the want of coal mines. It might be ſupplied from the mines in the department of La Creuſe, if the river of that name, which is navigable for boats only to Argentan, were rendered navigable higher up.

If the navigation of the Cher were improved, it might alſo ſupply with coals the eaſtern part of the department of Indre, becauſe the coal mines in the environs of Montluçon would then become an object of importance.

Department of Indre and Loire.

There are no coal mines in this department; but it receives the products of the mines of various departments by the river Loire, which traverſes it in its longeſt direction.

Department of Iſere.

The ſouthern part of this department (22) contains ſome coal mines; and particularly in the environs of the communes of La Motte, Pierre-Chatêl, La Mure, Saint-Barthelemi-de-Sechilienne, &c.

They ſupply a mineral combuſtible of a moderate quality; but is exceedingly valuable in a country where wood is daily becoming ſcarcer.

Theſe coal mines are almoſt all worked in an irregular manner, without attention to the lives of the workmen or to economy.

The annual product of theſe mines may be eſtimated at from a million to twelve hundred myriagrammes.

The price at the mines is from 15 to 20 cents per myriagramme; but the difficulty of land-carriage raiſes the price at Grenoble to from 60 to 80 cents.

Department of Jura.

Several indications of coals have been announced in this department; and ſome attempts were made to work mines, which gave riſe to well-founded hopes: but theſe attempts are now abandoned, and no mines are worked in this department.

In the preſent ſtate of things, the mines of Blanzy and Saint-Berain, in the department of Saône and Loire, furniſh coals to this department by the canal of Charolois and Doubs. The ſouthern part is ſupplied from the mines of Rives-de-Gier.

Department of Landes.

Indications of coal have been announced in the neighbourhood of Dax; but from the nature of the ſoil it is probable that it is foſſil wood. As this department has no coal mines worked, it is ſupplied with this fuel by ſea, and particularly from the port of Bayonne.

Department of Loire and Cher.

This department (23) has no coal mines. It receives this fuel by the river Loire from the mines of Haute-Loire and Allier.

Department of Haute-Loire.

Very abundant coal mines are worked in the cantons of Braſſac-Sainte-Florine, Freugères, Vergongheon, and Lempdes, the product of which is conſiderable. That of Groſmenil, ſituated in the laſt-mentioned commune, which had been perforated by a number of ſmall pits in ſuch a manner that the ſtrata of coal was inundated, is now in the hands of a company, who are clearing it of water, and making preparations for working it to a greater depth.

There is reaſon to think, that this mine alone, when in a productive ſtate, will furniſh as much as all the other mines of the country do at preſent. The annual product, however, of the latter is from fifteen to eighteen hundred thouſand myriagrammes. Theſe coals are of an excellent quality. The means of their conveyance are the navigation of the Allier and the Loire, the canal of Briare and of the Seine; which gives for their circulation an extent of more than 140 leagues. A large quantity of theſe coals is conſumed at Paris. The mean price of them at the pits is from 15 to 20 cents, and when delivered at Paris from 30 to 40 cents per myriagramme.

Department of La Loire.

The ſouth-eaſt part of this department (24) contains a great number of coal mines, worked in an extent of more than ten miles in length and five or ſix miles in breadth. The principal communes in which theſe mines are ſituated are thoſe of Rives-de-Gier, Saint-Chamond, Saint-Etienne, Le Chambon, Firnini, Roche-Moliere, &c.

In conſequence of the number and richneſs of the ſtrata found in theſe cantons, an immenſe quantity of this fuel has been extracted for ſeveral centuries paſt; but in order to obtain it ſooner, and with more eaſe, a great many pits have been ſunk, by means of which the coals neareſt the ſurface were dug out. The whole country is perforated with theſe apertures. This irregular method rendered the working of the inferior ſtrata much more laborious and expenſive.

The preſent product of the different mines in this department are ſtated at thirty millions of myriagrammes annually; and this, in all probability, is below the truth. If the mines were better worked, it is certain that the quantity might be quadrupled.

The quality and price of theſe coals is exceedingly various. Thoſe of the firſt quality coſt at the mine from 10 to 12 cents per myriagramme; thoſe of mean quality are ſold for 7 or 8 cents, and the worſt for 5.

The medium price of theſe coals tranſported to Lyons is 15 or 18 cents per myriagramme: the price at Marſeilles is 35 cents.

Department of La Loire Inferieure.

This department (25), as has been ſeen, may receive coals from the mines ſituated towards the upper part of the courſe of the Loire as well as from thoſe of Allier. The coals of the mine of Deciſe, which will be mentioned in ſpeaking of the department of La Nievre, are alſo conveyed to different places on the Loire. The coals alſo of the mines of Montrelais, ſituated two or three leagues to the north of Varades and Ingrande, are conveyed on the lower part of the Loire. They are embarked in particular at the latter place to be tranſported to the communes on the banks of that river deſcending to Nantes, where the conſumption is conſiderable.

The coals annually extracted from theſe mines may be ſafely eſtimated at a million of myriagrammes. They are of a good quality. The mean price on the ſpot is 5 cents per myriagramme; when ſent to Nantes, the ſame quantity coſts 25 cents.

In this department there are alſo peat-moſſes, the product of which is abundant, and of great utility to the inhabitants. The moſt conſiderable are found in the marſhes of Montoir, to the north of Nantes. More than eight thouſand individuals are employed in digging peat from theſe moſſes.

Department of Loire and Cher.

There are no mines worked in this department. It receives coals by the Loire, which traverſes it; and might conſume thoſe alſo brought by the Cher from the mines ſituated in the environs of Commentry and Montluçon, in the department of Allier, if the navigation of the Cher were improved.

Department of Loiret.

There are no coal mines worked in this department; but it is abundantly provided with coals by the navigation of the Seine.

Department of Lot.

Abundant coal mines exiſt in the environs of Figeac, towards the eaſt extremity of this department. They are badly worked by the proprietors of the ground; but they are ſuſceptible of improvement, if means of conſumption were opened for their products.

Department of Lot and Garonne.

No coal mines are worked in this department. Thoſe extracted from the mines of Carmeaux, in the department of Tarn, are brought to it on the Garonne; and it receives by the Lot thoſe of the department of that name, which would be ſupplied much cheaper if the Lot were rendered navigable towards Figeac and the department of Aveyron.

Department of La Lozere.

The diſcovery of ſome ſtrata of coal in this country would be of great utility. Wood daily becomes ſcarcer; and the communication with the coal countries in the neighbouring departments is difficult and expenſive.

Several indications of coal have been announced; particularly towards Canourgue, towards Mende, and in the neighbourhood of Meyrmey. Some ſpecimens ſent to the Council of Mines announce only foſſil wood.

Department of Lys.

This department has no coal mines worked. It receives thoſe of the departments of the North and of Jemappes.

Department of La Manche.

There are ſeveral indications of coal in this department; and particularly in the Foreſt of Briquebec, near Valonges; in the commune of Pleſſis, near Fretot; in that of Moon, and that of Semilly, in the diſtrict of Saint-Lô.

Strata of coal have been found in the commune of Pleſſi; but hitherto they are ſo intermixed with ſtrata of ſchiſt, that if worked they would not pay the expenſes.

Department of La Marne.

This department, as far as is yet known, has no coal mines. Collections of foſſil wood, and of peat exceedingly pyritous, are frequently found under ſtrata of marly earth. Theſe ſubſtances have often been announced as coal, but they do not poſſeſs the qualities of that fuel. They kindle ſlowly, and become totally incandeſcent; but they give very little flame, and for the moſt part none at all. This ſubſtance is called by the inhabitants *earth coal.*

The Valley of La Veſle furniſhes abundance of peat of an excellent quality. This river, which takes its ſource on the eaſt of Chalons, paſſes Rheims and Braine, and diſcharges itſelf into the Aiſne above Soiſſons, traverſing an extent of eighteen leagues. It every where runs over a bed of peat, and may afford great reſources to the neighbouring communes if that ſubſtance were properly dug up.

Department of La Haute Marne.

There are no coal mines worked in this department. Several indications of coal have been announced, but hitherto the ſpecimens have turned out to be bituminous foſſil wood. This department is at a diſtance from coal countries, and has on eaſy communication with them; but it is abundant in wood.

Department of La Mayenne.

This department is ſupplied with coals by the Loire. Boats can go up the Mayenne as far as Laval; and the Sarthe as far as Mons.

Department of Mayenne and Loire.

There are ſeveral ſmall coal pits worked in the canton (26) of Saint-Aubin-de-Luigné, in the territories of Chaudefond, Montjean, and ſeveral other places adjacent. They are worked very irregularly, and the product of them is not known.

The mine of Saint-George's-Châteloiſon, ſituated between Vihiers and Doué, to the eaſt of the latter commune, is more important and worked in a more regular manner. The product amounts to about 300,000 myriagrammes per annum; but it might be rendered more conſiderable.

Department of La Meurthe.

A diſcovery of coal has been announced in the neighbourhood of Nancy; but the ſpecimens ſent were only bituminous foſſil wood. At preſent this department has no coal mines actually worked.

Department of the Meuſe.

No coal mines are worked in this department (27). It can receive coals from La Saare by the Moſelle, and the canal which communicates with the Meuſe between Toul and Pagny: and the coals of the department of Ourthe may be conveyed on the Meuſe to its northern part.

Department of La Meuſe Inferieure.

This department poſſeſſes very important coal mines in the neighbourhood of Rolduc. The annual product of them amounts to more than 13,500,000 myriagrammes; and they are far from being properly worked.

The coals are of different qualities; ſome of them are very good. Their mean price at the mine varies from 5 to 14 cents

LXII. *Memoir on the Stones ſaid to have fallen from the Heavens. Read in the French National Inſtitute by* C. VAUQUELIN *.

WHILE all Europe reſounded with the report of ſtones fallen from the heavens, and while philoſophers, divided in opinion on this ſubject, were forming hypotheſes to explain the origin of them, each according to his own manner, Mr. Edward Howard, an able Engliſh chemiſt, was purſuing in ſilence the only route which could lead to a ſolution of the problem. He collected ſpecimens of ſtones which had fallen at different times, procured as much information as poſſible reſpecting them, compared the phyſical or exterior characters of theſe bodies; and even did more, in ſubjecting them to chemical analyſis by means as ingenious as exact.

It reſults from his reſearches, that the ſtones which fell in England, in Italy, in Germany, in the Eaſt Indies, and in other places, have all ſuch a perfect reſemblance that it is almoſt impoſſible to diſtinguiſh them from each other; and what renders the ſimilitude more perfect and more ſtriking is, that they are compoſed of the ſame principles and nearly in the ſame proportions.

Before the laſt reſults of the labour of Mr. Howard were known in France, I had employed myſelf on the ſame object; and I have the ſatisfaction to find in his memoir, which has been ſince printed, that they perfectly agree with thoſe which I had obtained.

I ſhould have abſtained from any public notice of an object which has been treated of in ſo able a manner by the Engliſh chemiſt, if he himſelf had not induced me to do ſo during his reſidence at Paris; had not the ſtones which I analyſed been from another country; and had not the intereſt excited by the ſubject rendered this repetition excuſable.

It is therefore to gratify Mr. Howard, to give, if poſſible, more weight to his experiments, and to enable philoſophers to place full confidence in them, rather than to offer any thing new, that I publiſh this memoir. One of the ſtones which I examined was tranſmitted to me by C. Saint-Amans: it fell at Créon, in the pariſh of Juliac, on the 24th of July 1790, about nine in the evening. This ſtone appeared in the air under the form of a fire-ball, which was viſible in almoſt the whole of the ſouth of France. A very correct account was given of it at that time in the abbé Bertholon's *Journal d'Hiſtoire Naturelle*, together with the proces-verbal of the municipality of the place, which confirmed the fall of this ſtone.

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* From the *Journal des Mines*, No. 76.

Another ſtone was given to me by M. Darcet junior: it fell at Barbotan, near Roquefort, in the month of July 1789. The brother of the late Darcet, curé in the neighbourhood, ſent it to him, with the proces-verbal drawn up reſpecting this extraordinary phænomenon. C. Lomet, who is known to ſeveral members of the Inſtitute, was at Agen on the day when this kind of meteor appeared in the atmoſphere. The following is the account which he gave me of it:

"It appeared as a very bright fire-ball, the light of which was as pure as that of the ſun; it had the ſize of a common air-balloon, and was long enough viſible to throw the inhabitants of the country into the greateſt conſternation; after which it burſt, and diſappeared. A few days after, ſome peaſants brought ſtones, which they ſaid were the reſult of the fall of the meteor: but at that period they were laughed at. What they ſaid was conſidered as fables; and thoſe to whom the ſtones were offered, would not accept of them. The peaſants would have now more reaſon to laugh at philoſophers."

The third kind of theſe ſtones is that brought from Benares in the Eaſt Indies, which fell on the 19th of December 1798, exhibiting the very ſame phænomena as were obſerved under ſimilar circumſtances in other countries. It was given to me by C. De Drée and by C. Saint-Amans, who brought it from England.

All theſe ſtones have a ſimilar appearance, and one might readily believe that they had been detached from the ſame maſs. Their ſurface is blackiſh, ſmooth, and, as it were, varniſhed by a commencement of fuſion. The inſide is of a whitiſh gray colour marked with a greater or leſs number of brown ſpots, or ſpots of a darker gray colour than the reſt of the maſs. Thoſe, however, found at Benares, and in Yorkſhire, are whiter in the interior part than thoſe found in France. There are obſerved in them white pyrites, the fracture of which is very much lamellated; globules of metallic and ductile iron, ſome of which weigh 46 Engliſh grains; but this iron has a whiter colour, and a greater degree of hardneſs, than common iron. The cauſe of this difference will be ſeen hereafter.

I wiſhed to have been able to ſubject to a ſeparate anaylſis each of the conſtituent principles of theſe ſtones ſenſible to the eye: but they are ſo intimately mixed, that it was impoſſible to ſeparate them exactly. By patience, however, I was able to obtain a ſufficient quantity of the globules of iron and of pyrites to aſcertain the nature of them.

A. A hundred parts of the ſtones of Benares, pulverized in a mortar of hard ſtone, and ſifted through a fine ſilk ſieve to ſeparate the coarſeſt particles of the iron which cannot be pulveriſed, were treated with dilute nitric acid. During this operation there was diſengaged a large quantity of nitrous gas; the acid aſſumed a yellowiſh green colour; the ſtony powder became whitiſh, and, dividing itſelf, increaſed conſiderably in volume, ſo that it reſembled gelatinous ſilex. Sulphureous particles were obſerved floating at the ſurface of the liquor.

B. When the action of the nitric acid ceaſed, water was added to the mixture; the liquor being filtered, the undiſſolved matter was waſhed, and after deſiccation in the open air it weighed no more than 64 parts.

C. As the matter treated in this manner was ſtill ſlightly coloured, it was boiled with muriatic acid, which, as is well known, unites more eaſily with oxidated iron: it aſſumed alſo a greeniſh yellow colour, and the powder became much whiter. After this ſecond operation the ſtone, when waſhed and calcined in a platina crucible, weighed only 47 parts.

D. The nitric and muriatic acids which had been ſucceſſively poured over the ſtone as above mentioned were mixed together and then precipitated by ammonia, of which an exceſs was added. Being heated for ſome time, the liquor was filtered, and the precipitate was waſhed and calcined: it had a brown colour, and weighed 38 parts.

E. The ammoniacal liquor had a ſlight blue colour inclining to violet. Alkaline carbonates produced in it no precipitate; on the other hand, cauſtic alkalies formed a white precipitate very abundant, but the liquor loſt none of its violet colour. This precipitate, when waſhed and dried in the air, had a greeniſh ſhade, and weighed 18 parts; calcination reduced it to 13. This matter readily combined with ſulphuric acid, and the reſult was a ſolution, which by ſpontaneous evaporation produced priſmatic cryſtals reſembling in taſte and ſavour thoſe of the ſulphate of magneſia. It however had a ſlight greeniſh colour, and at the end of ſome minutes produced in the mouth a metallic ſavour. To know whether the colour and ſavour of the ſulphate of magneſia aroſe from ſome metallic ſubſtances it was diſſolved in water, and hydroſulphuret of ammonia being poured over it, a pretty voluminous black precipitate was produced. After waſhing and deſiccation, however, it weighed only two parts. We ſhall return hereafter to this matter.

F. The ammoniacal liquor, from which the magneſia had been ſeparated by means of cauſtic potaſh, was mixed with a ſolution of ſulphurated hydrogen. By this mixture there was formed a very voluminous black precipitate, which when waſhed and dried weighed ſeven parts. This part being united with the three parts obtained alſo by ſulphate of magneſia, and kept at a red heat for ſome minutes, exhaled the odour of ſulphureous acid, and acquired a very dark green colour inclining to brown: the whole then weighed ſcarcely three parts.

A ſmall quantity of this matter fuſed with borax gave glaſs of a hyacinth colour; put into ſulphuric acid diluted with water, it diſſolved only in part, and a ſmall quantity which had a metallic appearance refuſed to combine with it; but the addition of a few drops of nitric acid, by oxidating it, favoured its ſolution: by ſpontaneous evaporation it furniſhed elongated cryſtals of a very beautiful green colour.

By the properties which this matter exhibited it is evident that it was nothing elſe but oxide of nickel; for it is the only one among the metals which poſſeſſes the property of colouring borax of a hyacinth red colour by fuſion, of giving to ammonia a purpliſh blue colour, of forming with ſulphuric acid a priſmatic ſalt of a green colour, and of producing by its combination with acids and ammonia triple ſalts which are not precipitated by fixed alkalies.

An examination of the principles ſucceſſively obtained by the different means employed for analyſing the ſtone of Benares, proves that it is compoſed of ſilex, magneſia, and iron, a part of which appears to be oxidated, nickel and ſulphur. Iron, nickel, and ſulphur, according to every appearance, form a particular triple combination, which ſeems only to be interpoſed between the earthy parts. I am, however, inclined to believe, that one part at leaſt of theſe ſubſtances is really in combination with the earths; for at the moment when the ſtone is attacked by the acids, the ſilex ſhows itſelf in a ſtate of very great diviſion, and like a kind of jelly. The proportions in which theſe matters are found in the ſtone of Benares are nearly as follows:

Silex - - -	48
Oxidated iron - -	38
Magneſia - -	13
Nickel - - -	3
Sulphur an indeterminate quantity	
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If we might ſuppoſe that the iron contained in this ſtone exiſts in the perfect metallic ſtate, we might eaſily find, by the increaſe of weight which it acquires in conſequence of oxygenation, the proportion of ſulphur; but as it is certain that this metal is oxidated in it in an unknown quantity, that of the ſulphur cannot be determined.

A hundred parts of this ſtone, freed as far as could poſſibly be done from ferruginous matters, were treated with muriatic acid diluted with a little water. The mixture produced immediately, with a ſtrong effervеſcence, a gas which had the odour of ſulphurated hydrogen gas. The matter aſſumed the form of a gelatinous maſs, which ſeems to prove, as already ſaid, that the ſilex exiſts in this ſtone in a ſtate of combination either with iron or with other ſubſtances.

When the effervеſcence had ſubſided, and the mixture had boiled for ſome time, it was diluted with water and filtered. The liquor had a green colour analogous to that of muriate of iron, but a little darker. The reſiduum when waſhed was white, and weighed 49 parts.

The muriatic ſolution and waſhings being united together were precipitated by ammonia added in exceſs, and were ſtirred for ſome time with the precipitate.

It has been remarked, that oxidated iron, precipitated in this manner by ammonia, has a more intenſe colour than that of iron obtained under the ſame circumſtances. This iron, waſhed and dried, weighed 42 parts.

The liquor containing the ſuperabundant ammonia in the precipitation of the iron had acquired a violet colour, which neither the heat nor the contact of the air made it loſe. Alkaline carbonates produced in it no alteration; on the other hand, fixed cauſtic akalies formed in it a white precipitate pretty abundant, which when waſhed and calcined weighed twelve parts. This matter combined with ſulphuric acid gave ſulphate of magneſia, coloured green by a ſmall quantity of nickel which the magneſia had carried down with it.

The liquor from which this earth had been ſeparated by potaſh had loſt a part of its violet colour; it, however, ſtill formed a black precipitate with ſulphurated hydrogen. From what I had before obſerved, I entertained no doubt that the ſubſtance which coloured the magneſia green, and was precipitated black by hodro-ſulphuret of ammonia, was nickel. I therefore ſeparated, by means of this reagent, the portion which remained in ſolution and that which was mixed with the ſulphate of magneſia: I united them, and having calcined them to ſeparate the ſulphur, I obtained a green oxide, which weighed three parts and a half.

The method employed to ſeparate the iron and the magneſia is founded on this circumſtance, that a ſolution of that earth containing an exceſs of acid is not precipitated by ammonia, becauſe there is formed a triple ſalt, which cannot be decompoſed by a ſuperabundance of that alkali; but the acid muſt be in ſuch quantity that the ſalt reſulting from its combination with the ammonia may be able to ſaturate the ſalt of magneſia exiſting in the liquor. Care was taken to obſerve this

Alkaline carbonates produce no precipitation of the ſubſtances which enter into combination with the triple ſalt here mentioned, though it muſt, however, be decompoſed; but there is formed another kind, compoſed of magneſia, carbonic acid, and ammonia, which remains in ſolution. But one thing I did not foreſee was the ſimultaneous precipitation of a portion of nickel and magneſia by the cauſtic potaſh; for, as Mr. Howard remarked, this metal is found there in complex combination, the oxide of which ought to be diſſolved by the ammonia in proportion as it becomes free: the precipitation of this metal muſt be produced by its affinity for magneſia, at leaſt this is what appears moſt probable.

After theſe two analyſes of the ſtone of Benares, and the conformity of the reſults which they furniſhed, though effected by methods ſomewhat different, there can be no doubt that it is really compoſed of iron, ſilex, magneſia, nickel, and ſulphur, as Mr. Howard announced.

Having therefore confirmed by theſe two means, and by others, which it is needleſs to mention, the reſults given by the Engliſh chemiſt in regard to the compoſition of the ſtone of Benares, I ſubjected to ſimilar experiments two other kinds of ſtone which fell in France, in order to aſcertain whether they would furniſh the ſame principles of analyſis as their external characters ſeemed to announce.

To avoid a repetition of the details already given of the analytical methods employed, I ſhall only obſerve, that the ſtones which fell at Barbotan and Juliac, treated with ſulphuric, nitric, muriatic, &c. acids, exhibited in the ſeries of operations to which they were ſubjected, and by the nature of the elements which they furniſhed, the moſt perfect ſimilarity to the preceding. It may therefore be conſidered as fully proved, that the ſtones ſaid to have fallen from the clouds in different countries are compoſed of principles perfectly ſimilar, and conſequently that we muſt have recourſe to a common cauſe to explain their formation, and to ſuppoſe alſo a common ſource from which nature derived the elements of them. I muſt, however, acknowledge, that the ſpecimens of the ſtones which have fallen in France are a little more charged with iron than thoſe of other countries; but as this metal exiſts in them, for the moſt part, in diſtinct globules which cannot be pulverized, they could have no influence on the reſults of the analyſis, as the earthy part was ſifted through a fine ſieve.

All the ſtones which have fallen to the earth, and particularly in France, contain, as I have already mentioned ſeveral times, globules of iron in a metallic ſtate more or leſs diſtinct, ſome of which weigh from forty to ſixty Engliſh grains. It appeared to me of importance to ſubject this iron to ſome analytical trials in order to aſcertain the nature of their conſtituent principles; but before I give the reſults it may be of utility that I ſhould firſt give an account of their phyſical characters. Theſe globules are much whiter than common iron; their colour approaches that of tin; their hardneſs is alſo greater, and conſequently they are more difficult to be forged.

This metal diſſolves eaſily, and with effervefcence, in all thoſe acids which diſſolve common iron; but inſtead of giving pure hydrogen gas, it furniſhes hydrogen gas very ſenſibly ſulphurated. A part only of this hydrogen gas, indeed, is combined with the ſulphur; for, having made it to paſs through water and cauſtic alkalies, the greater part of this fluid was not diſſolved; and after having made the different liquids here mentioned to paſs in the ſame manner, no ſenſible ſigns of ſulphur were given by reagents; but the water and the alkalies were manifeſtly hydro-ſulphurated, ſince they then precipitated black the greater part of metallic ſolutions, and particularly lead. This ſulphurated hydrogen gas exhibited to me a phænomenon which, as far as I know, has never been before obſerved in its combination with water—I mean a very ſpeedy decompoſition which it experienced in a flaſk perfectly cloſed in which it had been preſerved for ſome days. At the end of that time there were at the bottom of the water a great many ſmall white laminæ, and the water had no odour: it no longer precipitated ſolutions of lead. This gas then had experienced complete decompoſition.

The ſolution of this iron in muriatic acid was precipitated by ammonia, of which a ſuperabundance was added. The liquor when filtered had a colour inclining to purple; the oxide of iron, when waſhed and calcined, was of a brown colour, and had ſenſibly increaſed in weight. The ammoniacal liquor when ſubjected to evaporation depoſited ſlight traces of iron, but as long as there was ammonia in exceſs it retained its blue colour, which, as ſoon as this free alkali was evaporated, was changed to a meadow green. The liquor conſtantly preſerved this colour, which ſtill acquired greater intenſity until complete evaporation, and without forming any depoſit, which proves that nickel exiſted in the combination in the ſtate of a triple ſalt.

Fixed cauſtic alkalies occaſion no precipitation in this liquor; but they make it reſume its blue colour by decompoſing the muriate of ammonia and laying bare its baſe, which then rediſſolves the nickel.

The hydro-ſulphurets formed in it an abundant black depoſit; and this was the method employed for obtaining the nickel ſeparate. The hydro-ſulphuret of this metal, calcined in a platina crucible, left a powder of a dark meadow-green colour, which had all the properties of oxide of nickel.

The iron contained in the ſtones which have fallen from the heavens is therefore combined with ſulphur and nickel; whence it appears very probable that theſe two ſubſtances, which always preſent themſelves in a quantity more or leſs conſiderable in the analyſis of the whole ſtones, ariſe from a ſimilar combination, which it is impoſſible, whatever care may be taken, to ſeparate exactly from the earthy parts.

The preſence of nickel and of ſulphur in this iron explains why it is whiter, harder, and leſs ductile than common iron.

Though I did not aſcertain very exactly the quantities of theſe ſubſtances in the iron, I think I may aſſert that they do not amount to more than five or ſix per cent. In regard to the pyrites diſſeminated here and there throughout theſe ſtones, it is formed, as Mr. Howard ſays, of iron, ſulphur, and a ſmall quantity of nickel; but I was not able to aſcertain in what proportions theſe three ſubſtances are united, becauſe I had not a ſufficient quantity to ſubject them to an exact analyſis.

It appears to reſult from all the information obtained, and from teſtimonies worthy of credit, 1ſt, That maſſes, ſometimes of conſiderable ſize, have fallen to the ſurface of the earth: 2d, That theſe maſſes penetrated with fire move in the atmoſphere like inflamed balls, which throw light and heat, and to a great diſtance: 3d, That they ſeem to have received a motion parallel to the horizon, though they really deſcribe a curve: 4th, That they fall in a ſtate of ſoftneſs or fuſion like paſte, as is atteſted by their varniſhed ſurface, and the impreſſions formed on them by the bodies which they meet with: 5th, That ſome of them have fallen in England, Germany, Italy, France, and the Eaſt Indies: 6th, That all ſpecimens of the ſtones which have fallen in France are a little more charged with iron than thoſe of other countries; but as this metal exiſts in them, for the moſt part, in diſtinct globules which cannot be pulverized, they could have no influence on the reſults of the analyſis, as the earthy part was ſifted through a fine ſieve.

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Fixed cauſtic alkalies occaſion no precipitation in this liquor; but they make it reſume its blue colour by decompoſing the muriate of ammonia and laying bare its baſe, which then rediſſolves the nickel.

The hydro-ſulphurets formed in it an abundant black depoſit; and this was the method employed for obtaining the nickel ſeparate. The hydro-ſulphuret of this metal, calcined in a platina crucible, left a powder of a dark meadow-green colour, which had all the properties of oxide of nickel.

The iron contained in the ſtones which have fallen from the heavens is therefore combined with ſulphur and nickel; whence it appears very probable that theſe two ſubſtances, which always preſent themſelves in a quantity more or leſs conſiderable in the analyſis of the whole ſtones, ariſe from a ſimilar combination, which it is impoſſible, whatever care may be taken, to ſeparate exactly from the earthy parts.

The preſence of nickel and of ſulphur in this iron explains why it is whiter, harder, and leſs ductile than common iron.

Though I did not aſcertain very exactly the quantities of theſe ſubſtances in the iron, I think I may aſſert that they do not amount to more than five or ſix per cent. In regard to the pyrites diſſeminated here and there throughout theſe ſtones, it is formed, as Mr. Howard ſays, of iron, ſulphur, and a ſmall quantity of nickel; but I was not able to aſcertain in what proportions theſe three ſubſtances are united, becauſe I had not a ſufficient quantity to ſubject them to an exact analyſis.

It appears to reſult from all the information obtained, and from teſtimonies worthy of credit, 1ſt, That maſſes, ſometimes of conſiderable ſize, have fallen to the ſurface of the earth: 2d, That theſe maſſes penetrated with fire move in the atmoſphere like inflamed balls, which throw light and heat, and to a great diſtance: 3d, That they ſeem to have received a motion parallel to the horizon, though they really deſcribe a curve: 4th, That they fall in a ſtate of ſoftneſs or fuſion like paſte, as is atteſted by their varniſhed ſurface, and the impreſſions formed on them by the bodies which they meet with: 5th, That ſome of them have fallen in England, Germany, Italy, France, and the Eaſt Indies: 6th, That all theſe ſtones have a reſemblance to each other by their phyſical characters and their chemical compoſition.

What are the cauſes which can produce ſtones of this kind, and communicate to them ſo rapid and ſo ſingular a motion? How comes it they are always penetrated with fire? Theſe are queſtions for which at preſent it would be difficult to aſſign plauſible reaſons.

But whatever theſe cauſes may be, if multiple they muſt be of the ſame nature, ſince all the ſtones which have fallen in countries ſo different have in every reſpect a reſemblance to each other.

Do they owe their origin to volcanoes? But where are theſe volcanoes? We are not yet acquainted with them; and ſtones ſimilar to thoſe in queſtion have never been found among the productions of any of the known volcanoes. Is the atmoſphere the medium in which they are formed? But how can we conceive that ſubſtances ſo heavy as earths and metals could exiſt in ſufficient quantity, and remain long enough ſuſpended in a fluid ſo light as air? If we ſuppoſe that theſe bodies exiſted in the atmoſphere, whence did they originally come, and what means were ſo powerful as to unite them, and to form of them maſſes ſo heavy and ſo voluminous?

The opinion which makes them come from the moon, however extraordinary it may appear, is, perhaps, the leaſt improbable; and if it be true, that no direct proofs can be given of this opinion, it is equally certain that no well-founded reaſoning can be oppoſed to it.

The moſt prudent courſe to be purſued in this ſtate of things is freely to acknowledge, that we are entirely unacquainted with the origin of theſe ſtones, and of the cauſes which produced them.

LXIII. *Conjectures on the Stones which have fallen from the Atmoſphere.* *By* EUSEBIUS SALVERTE *.

THE antients never entertained any doubt in regard to what they obſerved. As the natural ſciences among them were only collections of facts, no theory compelled them to contradict their obſervations. We accuſe them of often having ſeen without examining; but though we examine better ourſelves, we are often obliged to abjure our ſcepticiſm, and at length to ſee what the antients ſaw before us.

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* From the *Annales de Chimie*, No. 133.

Among the phænomena to which this reflection may be applied, there is none more remarkable than the fall of ſtones, produced, according to the antients, by thunder. This is what they called thunder-bolts, an expreſſion long ago baniſhed to the domains of poetry, and which it would appear ought to acquire a place in the language of philoſophy.

The antient hiſtorians all make frequent mention of the production of theſe ſtones. No doubt was entertained reſpecting them in the middle ages; but the difficulty of accounting for them induced us not only to ſuſpend our belief until called forth by more regular obſervation, which was very prudent, but alſo, which was leſs reaſonable, to carry with us in this reſearch a predetermination to ſee nothing, or to deny what we had ſeen.

Notwithſtanding this diſpoſition, inſtances have been ſo multiplied before our eyes, and in ſo uniform a manner, that it becomes difficult not to admit the general fact, whatever opinion may be adopted in regard to the cauſe. Stones abſolutely foreign to the ſoil where they were found, and all having the greateſt reſemblance to each other, have been collected, in Portugal in 1796, in Alſace in , in Yorkſhire in 1796, at Sienna in 1794, at Benares in 1798, in Bohemia in 1753, near Paris in 1768. Several have been found ſtill warm; and uniform tradition ſtates, that they were ſeen to fall from the atmoſphere in the time of lightning, and particularly during the burſting of luminous meteors, the production of which often accompanies ſtorms. The papers of Mr. Howard and of Count Bournon contain very ſatisfactory details in regard to every thing obſerved in regard to the nature and fall of theſe ſtones.

The *Journal de Phyſique, Brumaire, an* xi, contains a memoir of C. Patrin, who, denying the celeſtial origin of theſe ſtones, ſuppoſes that they have only been uncovered and forced from the earth by the contact of thunder. But before this hypotheſis can be eſtabliſhed, would it not be neceſſary that in the places where theſe ſtones have been found, and in others, ſimilar ones ſhould have been previouſly diſcovered at the depth of a foot, or of ſome inches below the ſurface? For why ſhould they ſhow themſelves at the ſurface of the earth only after thunder? Why ſhould they conſtantly eſcape the plough and the ſpade of the farmer, the pick-axe of the ditcher, and the reſearches of the mineralogiſt?

To admit that theſe ſtones have been thrown up into the atmoſphere by volcanoes, ſeems to be attended with the ſame difficulty; for all thoſe analyſed by Vauquelin contain a large proportion of iron and nickel, in the native ſtate, mixed with

magneſia.

magneſia. But nothing is more rare in the volcanic productions, with which we are acquainted, than magneſia, if we except iron allayed with the nickel in the native ſtate. Beſides theſe ſtones have been collected at ſuch a diſtance from all volcanoes, that the power which could ſo long ſupport and convey through the air ſuch enormous maſſes, would be ſtill more inexplicable than their ſpontaneous production.

Chemiſts have proved, 1ſt, That theſe ſtones, collected in places and at times diſtant from each other, have a reſemblance in regard to the ſubſtances of which they are compoſed, and differ only by the proportions of their elements: 2d, That they differ from all the other mineralogical compounds found at the ſurface of the globe. It is natural, therefore, to aſſign to them all an origin of the ſame kind, and very different from that by which the minerals hitherto obſerved have been produced.

Struck with theſe ſingular characters, C. Delaplace and Biot mentioned, during the diſcuſſion which took place in the National Inſtitute in conſequence of Vauquelin's analyſis, a very bold opinion, but which is rendered probable by the celebrity and character of its authors. According to them, theſe ſtones may be the product of a volcanic eruption in the moon. Comparing the maſs and denſity of the moon with that of the earth, and calculating the diſtance between our planet and its ſatellite, they have eſtabliſhed that a volcanic eruption might project a body to ſuch a diſtance from the moon as to be within the ſphere of the earth's attraction. It is beſides proved, by aſtronomical obſervation, that the atmoſphere of the moon is exceedingly rare, and conſequently could oppoſe very little reſiſtance to the elevation of ſtones projected from the moon. To this may be added, that as the moon has ſcarcely any atmoſphere, this circumſtance may ſerve to explain why the metals contained in theſe ſtones are not in the ſtate of oxide.

But the ſame obſervation which has diſcovered volcanoes in the moon, ſhews them in a ſtate of luminous ignition, and hitherto luminous ignition has never been known to take place without oxygen. All the gaſes abſorb caloric, and on becoming concrete, ſuffer it to eſcape. Oxygen alone poſſeſſes the property of abſorbing light, and of emitting it when it combines with bodies during combuſtion. It thence follows, 1ſt, That metallic ſubſtances projected from the moon ought to be oxidated, at leaſt in great part: 2d, That the obſervations which prove the great tenuity or little extent of the atmoſphere of the moon, ought to induce us to believe that the volcanoes which burn at its ſurface are in a ſtate of tranquil inflammation, maintained by a very thin ſtratum of atmoſpheric air, very different from that of our volcanoes in a ſtate of eruption; and, conſequently, that they cannot be endowed with a very ſtrong projectile force: for on the earth this force is produced by the expanſibility of gas compreſſed by the weight of the atmoſphere. But gaſes diſengaged from the lunar volcanoes ought to expand without any obſtacle, and without any exploſion where there is ſcarcely any atmoſphere. Beſides, the exiſtence of theſe gaſes is doubtful, ſince their production would ſoon have formed around the moon an atmoſphere of greater denſity and extent, which is contrary to obſervation. This explanation therefore muſt be abandoned, or we ſhould be forced to admit a ſeries of phænomena, not only foreign but contrary to what we daily obſerve: a ſolution more ſimple, and deduced from facts, which daily take place before our eyes, will perhaps appear more proper for ſolving the problem. It is well known that there are no metals which may not be volatilized by heat: it is known alſo that hydrogen gas exerciſes its diſſolving power on charcoal, iron, and the moſt fixed ſubſtances. If we ſuppoſe that, during the burning proceſs of volcanoes, or the leſs perceptible but more aſſiduous proceſs of the decompoſition of organic bodies, iron and nickel are ſublimated by heat and elevated with the hydrogen gas which diſſolves them, this gaſeous ſolution will ſoon reach the upper regions of the atmoſphere. There, as I have ſhown, with ſome degree of probability, in my *Conjectures on the Diminution of the Waters, &c.* exiſts, and is continually renewed, a ſtratum of hydrogen produced by the continual decompoſition of the water, and which cauſes the greater part of the phænomena that accompany thunder and the aurora borealis. During ſtorms, that is to ſay, when the equilibrium is reſtored with an exploſion between the electricity of the earth and that of the atmoſphere, what muſt take place? The hydrogen inflames and exhibits ſome of thoſe luminous meteors, the exiſtence of which, according to conſtant tradition, ought, as appears, to precede the formation of ſtones. The gas in burning abandons the metal it has diſſolved, and reduces that which was in the ſtate of oxide. The ſtrong heat produced at this moment fuzes the metal, and molecular attraction collects it into maſſes of greater or leſs ſize, which, when they fall to the earth, retain for ſome time a portion of the caloric developed during their formation. The ſurface only, which, by traverſing the atmoſphere in a ſtate of ignition, may have abſorbed oxygen, is ſlightly oxidated: the interior part is metal in a native ſtate.

C. Patrin obſerves, in ſupport of his hypotheſis, that the diſcharge of an electric battery, on a fragment of the ſtones found at Benares, produced on it a black trace ſimilar to the black vitrified cruſt with which they were covered. This fact indicates what takes place in the atmoſphere, where theſe ſtones are formed amidſt a very powerful electricity, which produces the vitrification and oxidation of their ſurface.

The ſame author ſays, with Mr. Howard, that, ſince no doubt is now entertained of thunder and the electric fluid being the ſame thing, the idea of a *thunder ſtone* is become ridiculous. Nothing could be more true, if thunder were never any thing more than an electric exploſion: but the French chemiſts, and particularly Fourcroy, have eſtabliſhed that hydrogen acts a conſiderable part in the phænomena by which thunder is accompanied. I am even of opinion that its exiſtence ought to be oftener employed in explaining the varieties which continually render the general phænomenon complex.

The moſt remarkable and moſt common are perhaps thoſe luminous meteors, the aſpect of which is always brilliant and often alarming, and which uniform tradition, at periods and in places very diſtant from each other, aſſign as the cauſe of the ſtones which have fallen from the heavens: whether they ſeem to be confounded with the ſtars, and exhibit to the vulgar eye ſtars thrown from their uſual courſe, or whether they accompany thunder, and increaſe or modify the action of its devouring flames, or whether, under the form of fire balls, they rapidly traverſe the atmoſphere, and approach near enough to us to make us experience a heat proportioned to the vivacity of their light, they muſt be conſidered as the effect of the inflammation of hydrogen gas, either pure or charged with ſubſtances in ſolution, which modify their appearance and products. On theſe principles we may point out the ſuppoſed cauſes of the difference which exiſts between ſtones that have fallen from the atmoſphere at different periods. A very violent ſhock of electricity, or great heat, have produced thoſe which are moſt vitrified: thoſe leſs ſo have been formed by a weaker degree of electricity, and perhaps by ſpontaneous inflammation. Thoſe which contain the largeſt quantity of metal in the native ſtate, are the product of a greater proportion of hydrogen gas. It may be readily conceived, that when the hydrogen gas has been previouſly combined with a great deal of oxygen, the inflammation muſt be inſtantaneous, and the products very much oxidated, while the ſtrata of hydrogen, almoſt pure, burning only at the ſurface, form durable meteors, the continued inflammation of which gives riſe to very different compounds.

It will perhaps be aſked, how the other ſubſtances, ſuch as ſilex and magneſia, are found in theſe ſtones combined with the metals? I might, in anſwer, mention the height to which earths reduced to an impalpable powder are volatilized by volcanoes, ſo as to be carried away by the winds. I might obſerve, alſo, that magneſia, ſoluble in the hydroſulphurets, is ſoluble alſo, in all probability, in ſulphurated hydrogen. But, as I have nothing as yet except conjectures to offer on this ſubject, I muſt here ſtop. I, however, flatter myſelf that I have done ſome ſervice, by ſhewing that a phænomenon, obſervations of which had been rejected becauſe thought impoſſible, has received, at leaſt in part, a plauſible and natural explanation perfectly rational; and that, when placed beyond a doubt, it will be arranged among thoſe ſeries of facts of which ſcience is compoſed.

That I might not render the hypotheſis complex, I have ſaid nothing of the different gaſes, ſuch as ſulphurated hydrogen, carbonated hydrogen, and carbonous oxide, which muſt be continually conveyed into the regions of the atmoſphere by the decompoſition of bodies. The exiſtence of the firſt of theſe gaſes explains, however, the pyritous ſtate of the iron, and the preſence of ſulphur in ſome one of its parts.

LXIV. *Obſervations on the Decompoſition of the Acetite of Lead, by Zinc in the metallic State. Read in the Society of Pharmacy by* L. ANTHONY PLANCHE*.

IN conſulting the different authors who have treated on the hiſtory of metals, and their chemical action on different bodies, we cannot help conſidering zinc as exceedingly valuable, both on account of its utility in the arts, and the eſſential ſervices which it daily renders to chemiſtry.

C. Fourcroy, who in his *Syſteme des Connoiſſances Chimiques*, has collected the moſt numerous and beſt atteſted facts of this ſcience, has deſcribed all its properties with that clearneſs and method which are peculiar to him.

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* From the *Journal de Chimie*, No. 133.

It is therefore not ſo much a new fact that I here preſent as a development of one already known, which, as it forms part of a general theory, deſerves to be particularly noticed as one of the curious phænomena in chemiſtry. But to proceed in order, I think it neceſſary to refer to the work of that celebrated chemiſt already mentioned, where it is ſaid, in the ſixth ſection on metals, under the head Zinc, that

" Zinc, by its ſtrong attraction for oxygen, decompoſes the greater number of ſalts and of metallic ſolutions; and that it precipitates from them the metals under the metallic form by completely unburning them, or under that of oxides leſs oxidated than they were before."

This definition is no doubt juſt, and ſeems to be perfectly complete. It is ſuſceptible, however, of the greateſt development; and particularly in regard to the different phænomena exhibited by zinc in contact with acetite of lead. It is the union of theſe phænomena which forms the principal ſubject of the preſent obſervation, or rather the reſult of the ingenious experiment of Dr. Black, which was communicated to me a few years ago, and to which I have made ſome additions.

Dr. Black put into a glaſs jar, containing about four and a half pints, twelve or fifteen decagrammes of cryſtallized acetite of lead. The jar was filled with water, and the mixture being ſtirred, it was left at reſt for fifteen or twenty minutes, or until the greater part of the ſuperabundant ſalt, after the ſaturation of the liquor, was ſpontaneouſly precipitated. A piece of zinc of an indeterminate weight was then ſuſpended in the ſlightly turbid liquor by means of a hemp or ſilk thread fixed to the cover of the jar, and the veſſel was depoſited in a place where it was not expoſed to be ſhaken. At the end of from twenty to twenty-five days decompoſition was completely effected; the zinc, ſuſpended in the middle of the liquor, become diaphonous, was covered by a kind of very brilliant metallic vegetation, which was often continued to the bottom of the veſſel, and which Dr. Black called the tree of Saturn.

Having had occaſion to repeat this experiment a great number of times, it conducted me to ſeveral obſervations, in conſequence of which I reſolved to make ſome changes in it, which, as they do not alter in any manner the chemical nature of the operation, contribute in a ſingular manner to the beauty of the reſult. Theſe changes chiefly relate to the hemp and ſilk threads, which, being eaſily deſtroyed by remaining in the liquor, muſt neceſſarily ſuffer the zinc to fall, and along with it the new metal which covers them. I therefore ſubſtituted for theſe threads a piece of braſs wire which appeared to me to unite all the neceſſary conditions, both on account of its ſtrength, and the property it has as containing zinc to exerciſe its attraction on the acetite of lead. A ſecond inconvenience ariſes in ſtrictly following the proceſs above deſcribed. It often happens, when the ſize of the piece of zinc is too large, or when the veſſel is not exactly of the proper form, that the decompoſition takes place with great rapidity; each molecula of the new metal proceeds immediately to the zinc, adheres to it in a confuſed manner, and at the end of two or three days, and ſometimes of twenty-four hours, the cryſtallized portion ſuddenly detaches itſelf and is precipitated to the bottom of the veſſel to give place to a new cryſtallization. The proceſs is then divided into ſeveral periods, which appears to be a matter of indifference in a chemical point of view, but greatly leſſens the intereſt excited by this curious experiment.

I obviated this inconvenience by fixing, in a perpendicular direction, to the lower part of the piece of zinc, the ſame braſs wires twiſted into a ſpiral form, and diſpoſed in ſuch a manner as to be about three centimetres diſtant from the bottom of the veſſel.

By adopting this modification, as each molecula of the zinc formed a portion of the braſs, the ſurface of which was in contact with the ſaturation, it became covered with a great number of ſmall, brilliant, metallic laminæ, diſpoſed at firſt alternately and horizontally, and then croſſing each other in every direction *.

When it is required that the braſs, though covered with metal, ſhould retain the forms it has firſt acquired, it will be proper to diminiſh the quantity of the acetite of lead.

About ſix months ago, I repeated this experiment with common and with diſtilled water, in order that I might examine the products. The reſult of this examination is as follows:

Having ſelected two glaſs jars of equal capacity, I introduced into each four decagrammes of very pure cryſtallized acetite of lead.

The two veſſels having been marked No. I. and II., I poured into the firſt 54 decagrammes of common water, and as much diſtilled water into the ſecond. Theſe quantities

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* The configuration of theſe laminæ is ſubject to an infinite variety, which ſeems to depend, 1ſt, On the form and different dimenſions of the veſſel in which the experiment is performed: 2d, And perhaps alſo on regular quantities of the zinc and acetite of lead employed. I have ſeen the laminæ ſometimes circular, ſometimes hexagonal, and ſometimes alſo affecting the figure of a leaf of fern or of oak.

were ſufficient to fill each jar to the commencement of the neck, the height of which was about three centimetres. I then fixed to two pieces of cork deſtined to cloſe the mouths of the jars a bit of zinc weighing 22·9456 grammes (ſix gros) by means of a piece of braſs wire exceedingly fine, but of ſuch a length as to allow the metal to be immerſed only ſome millimetres in the ſolution.

I then adjuſted, and luted to the aperture of each veſſel, a bent tube, which proceeded under the receiver of a pneumatic tub. Theſe two experiments, begun on the 29th of Thermidor, year 9, the centigrade thermometer being at 14° above zero, exhibited the following phænomena.

Six hours after the apparatus was arranged, the jar No. I. was ſtill ſomewhat turbid, while that of No. II. was tranſparent, and the whole ſuperabundant ſalt, after the ſaturation of the liquor, was entirely precipitated. At this period the zinc in the ſecond jar was covered with ſmall metallic ſcales, which could not be perceived in the firſt.

Next day both liquors were perfectly tranſparent, and the decompoſition ſeemed to have made equal progreſs in both veſſels. I only remarked, that the interior ſides of the jar No. I. were lined with a whitiſh ſaline ſtratum, which extended from the middle of the veſſel to the bottom. The ſmall ſcales adhering to the ſides and bottom of the piece of zinc, by increaſing in volume, had aſſumed the form of a leaf of fern in the jar No. I., and was exceedingly delicate in No. II.

I obſerved alſo at the upper ſurface of the bit of zinc a kind of metallic moſs *, having that livid aſpect which characterizes plates of lead expoſed for ſome time to the contact of the air. The ſaline ſtratum of No. I. daily decreaſed to the eighth, when it entirely diſappeared. In the courſe of theſe two experiments there was diſengaged a very ſmall portion of elaſtic fluid, which when examined was found to be atmoſpheric air. The quantity of this air appeared to be more conſiderable in the apparatus marked No. I.; and this appeared to me to ariſe from the common water employed, which always contains more or leſs of it. The remainder of the operation exhibited nothing remarkable but an increaſe in the volume of the reduced metal until the period of the decompo-

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* This metallic moſs in a tree of Saturn, prepared a year before, was found to be covered by a very fine red powder, which occupied both the ſurface of the zinc and that of a portion of the undecompoſed acetite of lead which covered the horizontal bottom of the glaſs veſſel. I conſider this red powder as produced by carburet of iron, from which zinc is never free, and which during the operation has been carried to the ſtate of carbonate.

decompoſition, which took place in the experiment with diſtilled water on the 16th of Fructidor, year 9, and in that with common water on the 24th of the ſame month. During the whole time the temperature varied only one or two degrees:

As the reſults appeared to be the ſame in regard to the reſt of the operation, I ſhall proceed to examine the experiment made with diſtilled water.

1ſt, The piece of zinc, weighing 22·9456 grammes before its immerſion in the liquor, was completely freed from all the metal with which it was covered. In this ſtate it weighed no more than 15·9345 grammes (4 gros 12 grains). Its ſurface was covered with a gray pulverulent ſtratum, the greater part of which ſtill retained its metallic brilliancy, while the other ſeemed to have undergone a commencement of oxidation. When entirely freed from this ſtratum, which could be eaſily detached with the finger, and which appeared to me to be nothing but zinc, having its moleculæ in part ſeparated, it was reduced to the weight of 13·380 grammes (3 gros 37 grains).

2d, The portion of the new metallic alloy, zinc and lead *, ariſing from the decompoſition of the acetite, was waſhed ſeveral times in diſtilled water. Being ſubjected to the action of a good preſs, and then to gradual percuſſion on a piece of ſteel, it poſſeſſed greater ductility than lead alone. When dried in cloſe veſſels, and then expoſed to ſudden heat in an iron ſpoon, a portion of it was ſpeedily converted into a greeniſh yellow oxide. The other portion, reduced without any addition, furniſhed a ſmall metallic button of the weight of 9·0183 grammes (2 gros 38 grains), and containing zinc, but much leſs weight for weight than was contained in the above matter before it was heated.

I muſt obſerve, that, notwithſtanding all the precautions which I took to ſeparate, either by the help of a preſs and unſized paper, or by deſiccation, all the water interpoſed between the moleculæ of the metal, I was not able to ſucceed. It appears probable, that under this circumſtance the ſpeedy oxidation of the new metallic alloy was owing, 1ſt, To the decompoſition of the laſt moleculæ of water, which could not be entirely expelled: 2d, To the preſence of the atmoſpheric air of the veſſels: 3d, To the contact and decompoſition of the ſame fluid during the fuſion of the metal.

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* The formation of this metallic alloy agrees with the experiments of Vauquelin, from which it reſults, that in ſeveral caſes, when a metal is precipitated from its ſolution by another, the precipitated metal partakes in ſome degree of the precipitant. This phænomenon takes place in particular during the precipitation of the white metals by zinc.

Examination of the Liquor with ſome Reagents.

1ſt, The liquor taken from the jar was colourleſs and perfectly tranſparent.

2d, It gave a ſtrong green tint to blue vegetable colours.

3d, When tried with very pure ſulphuric acid it formed no precipitate, which convinced me that it held in ſolution no lead.

4th, A portion properly evaporated furniſhed ſmall cryſtals of acetite of zinc in argenteous leaves, which affected no regular form.

5th, Ammonia produced in it a white flaky precipitate, not very abundant. At the moment even of contact there is emitted a ſtrong odour of flour paſte, which is ſpeedily diſſipated by agitation.

6th, This precipitate at the end of ſome hours had acquired a ſemi-gelatinous conſiſtence, and the addition of a new quantity of alkali rediſſolved it in part.

7th, The carbonates of potaſh, ſoda, and ammonia, formed in it white precipitates more or leſs abundant.

8th, Lime water recently prepared precipitated from it the zinc in the ſtate of a white oxide, paler and more homogeneous than the preceding.

9th, The precipitate obtained by carbonate of potaſh, waſhed ſeveral times with diſtilled water, and carefully dried, was exceedingly white and light.

10th, Being brought into contact with concentrated ſulphurous acid, it diſſolved in it completely, with a diſengagement of carbonic acid, and a total diſappearance of the ſulphurous odour; which ſeems to ſhow a very great relation between the new ſalt reſulting from this combination and the ſulphate of zinc, for the diſcovery of which we are indebted to Vauquelin and Fourcroy.

RECAPITULATION.

It reſults from theſe facts, 1ſt, That the decompoſition of the acetite of lead by zinc exhibits ſeveral phænomena not obſerved in that of other ſalts and metallic ſolutions by the ſame metal.

2d, That diſtilled water ſeems to favour this decompoſition, as it is effected in that fluid a third ſooner than in common water.

3d, That common water may, however, be preferable in experiments of mere curioſity, as it checks the decompoſing action of the zinc, and diſpoſes the metallic moleculæ to aſſume that arrangement which is peculiar to them; a phænomenon which agrees with the laws of cryſtallization.

I ſhall terminate theſe obſervations by propoſing to the

College of Pharmacy to adopt among its experiments, which form a part of its annual public courſe of chemiſtry, the tree of Saturn, as proper for proving in a preciſe manner the ſtrong attraction of zinc for oxygen. This experiment, which is not very expenſive, always ſucceeds, and deſerves a place in mineralogical cabinets along with the tree of Diana.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

THE meetings of the 12th and 19th of May were occupied by the reading of a paper, by Mr. Chenevix, on a metallic ſubſtance, of which we have ſpoken in our laſt Number. It was announced to the public by its author as a new ſimple metal *ſui generis*, under the title of *Palladium*, or *new Silver*.

Mr. Chenevix made a number of experiments on this ſubſtance, and found that, in moſt reſpects, it was really different from all the known metallic bodies. In a few, however, it bore ſome reſemblance to platina. But it was not poſſible to conceive that platina could be reduced to the ſpecific gravity of 11·8; could be fuſible at ſo moderate a heat; could combine with ſulphur; could be diſſolved by nitric acid, and be precipitated by green ſulphate of iron. Theſe are the properties mentioned in the printed notice we received laſt month, and which we inſerted in our Journal. Mr. Chenevix found them to be truly ſtated.

Reflecting, however, upon the extraordinary anomalies that happen when ſubſtances are united, it occurred to him that ſome metal might ſo far influence platina as to promote its precipitation by green ſulphate of iron, and be itſelf drawn down along with it. Mercury was the firſt that preſented itſelf to his mind, as the moſt eaſily reduced. He poured a ſolution of green ſulphate of iron into a ſolution of platina: no precipitate. Into a ſolution of muriate of mercury: no precipitate. He mixed the two ſolutions, and a precipitate, exactly reſembling that produced in palladium, immediately enſued. He reduced this precipitate; and, after ſeveral trials, at length ſucceeded in obtaining a ſubſtance exactly ſimilar to palladium. From all his experiments, which are much too numerous for this extract, Mr. Chenevix does not heſitate to pronounce palladium to be a ſcandalous impoſition, and an attempt to defraud the public as well as to diſgrace ſcience.

From ſyntheſis and analyſis it appears that palladium of the ſpecific gravity of 11·5 contains about one-third of mercury; and when of the ſpecific gravity of 12·5, about one-fourth. As the quantity of this metal diminiſhes, the ſpecific gravity increaſes; the ductility is leſs; and after 12·5 it begins to be no longer acted upon by nitric acid.

Mr. Chenevix gives many methods for preparing a ſubſtance ſimilar to palladium; but the following is the leaſt liable to fail:—Take a certain quantity of platina, reduced from the triple ammoniacal ſalt, and diſſolve it in nitro-muriatic acid: add ſomething more than twice the weight of the platina of red oxide of mercury, and take particular care to ſaturate all exceſs of acid: then pour it into a ſolution, alſo ſaturated, of green ſulphate of iron; heat the mixture, and a black precipitate will be formed after ſome time. This precipitate weighs rather more than two and a half the original quantity of the platina; but, expoſed to a red heat, a part of the mercury is volatilized. Fuſed at a ſtronger heat, it yields a metallic button, which in different experiments gives a different ſpecific gravity. This experiment is of a very delicate nature, and does not always ſucceed; and Mr. Chenevix recommends perſeverance and repeated trials to all who would form palladium. It is not often to be obtained of the exact ſpecific gravity announced by the author of the fraudulent advertiſement; and the ſpecific gravity is not conſtantly the ſame in all his ſpecimens. The difficulty is, to unite a ſufficiently large portion of mercury with the platina to produce perfect palladium. It may be obtained by this method as low as 10·5, ſometimes as high as 15. Mr. Chenevix mentions ſome methods of combining platina and mercury in the dry way, ſuch as count Muſhin Puſkin's amalgam fuſing with cinnabar, lime, and borax, &c.

He then relates ſome experiments upon platina; upon its oxides, which are two, viz. one of 7 the other of 13 per cent. of oxygen; upon the affinities of the oxide for the acid and aromatic ſalts formed by them. He gives numerous examples of the affinities of metals for each other; and of anomalous precipitations, when two or more metals are preſent in the ſame ſolution. He ſays, that the fixation of mercury by platina, together with other facts related in this paper, will tend to make us particularly cautious in pronouncing upon diſcoveries of ſuppoſed new metals, and promote ſcepticiſm even upon our preſent knowledge. He adduces inſtances of the difference that exiſts between a ſolution of platina and a ſolution of mercury merely mixed, and a ſolution of theſe metals after they have been united. In the former caſe they are eaſily ſeparated; in the latter there is no method of completely diſuniting them. He concludes with a curious ſtatement of the approximation of ſeveral metals and of ſeveral earths to each other in their chemical properties, which follow

low the direct order of their tendency to enter into ſaline combination.

The whole of the facts related in this paper are of the higheſt intereſt to the ſcience, as they prove to what a degree the properties of ſimple bodies, hitherto thought to be uncombinable, may be altered when they are united; and ſhow the difficulties that attend our pronouncing as to the ſimplicity or the compoſition of any ſubſtance, whatever be the character that diſtinguiſh it in chemical experiments.

ROYAL SOCIETY OF GÖTTINGEN.

The Mathematical Claſs of this Society has propoſed the following prize queſtion, the prize for which is to be adjudged in the month of November 1803:

As it is of great importance in pyrometric diſquiſitions as well as in the application of them, and alſo in making reſearches in regard to the nature of light and heat, to aſcertain the various degrees of heat which different ſubſtances, ſooner or later, acquire when expoſed to the ſolar rays; and, as little certain information has hitherto been acquired on this ſubject, the ſociety recommends it to the care and attention of philoſophers.

1ſt, To inveſtigate by nice experiments, and by calculation founded on them, in what manner bodies of different ſubſtances, but of the ſame figure and volume, (globes of an inch diameter will, perhaps, be beſt for this purpoſe,) are heated by the ſolar rays under the ſame ſtate of the atmoſphere, the ſame intenſity of light, and the ſame initial temperament during each minute of obſervation, &c.

2d, To determine, either by direct obſervation, which is chiefly deſired, or from the obſerved law of the increaſing heat, to what degree of temperament any body taken at the end of the experiment, that is, when the increaſe of the heat ceaſes, would have attained.

The Hiſtorical Claſs has propoſed the following queſtion, the prize for which is to be adjudged in the month of November 1804:

As many obſervations have been carefully made by the old and by modern philoſophers, particularly ſince the 16th century, in regard to meteors; as various ingenious opinions have been given reſpecting their origin and nature, and the laws to which they are ſubject; and as many things perhaps occur in theſe which might be of utility to improve meteorology, or which at leaſt may be worthy of further examination, the ſociety requires a continued accurate hiſtory of meteorology from the firſt attempts of the Greeks and Romans in this kind of ſtudy down to the preſent period.

The ſociety, however, by no means deſires that the com-petitors will include in it any of thoſe opinions of the antient or modern writers which, being founded on ſuperſtition or ignorance, would excite only diſguſt. It rather wiſhes that they would ſelect, explain, and examine the opinions of Ariſtotle, Theophraſtus, Pliny, Seneca and Ptolemy, among the antients, and thoſe of the philoſophers of the 16th and 18th centuries, particularly Teleſius, Patricius, Bruno, lord Bacon, Kepler, Gaſſendi, and Deſcartes, a knowledge of which may be of uſe to meteorology at preſent: it requires alſo, that the manner in which the obſervations were made, and the inſtruments employed, may be briefly deſcribed, marking at the ſame time the climate. An inquiry may alſo be made reſpecting the origin of that opinion, which became ſo prevalent, in regard to the influence of the planets on meteors: and, in the laſt place, the author may briefly and generally determine how far we can approach nearer to the truth by improving phyſical aſtronomy and meteorology.

The prize for each of theſe queſtions is fifty ducats, and the memoirs muſt be tranſmitted to the ſociety before the month of September each year.

GALVANIC SOCIETY, PARIS.

Galvanic facts, in proportion as they are accumulated, afford a hope of the happieſt reſults being ſoon obtained from the application of this ſtimulus. Thoſe which we are here about to detail relate partly to theory and partly to the medical employment of this fluid.

In a late ſitting of the Galvanic Society, C. Gautherot mentioned an obſervation made by him anterior to that period, which was both curious in itſelf, and of importance, on account of the inductions that may be drawn from it. As it is neceſſary to be known before a proper opinion can be formed of thoſe by which it was accompanied, we think it proper to lay before the reader the two following experiments:

Experiment I.

The perſon who performs this experiment places in his mouth the upper ends of two wires of platina, or of any other metal not oxidable, and immediately brings the other two ends into contact with the two extremities of a weak Galvanic apparatus, to aſcertain the degree of its influence. When this arrangement has been made, the perſon will experience an effect more or leſs remarkable, according as the tongue is more or leſs exerciſed in perceiving the peculiar ſavour of the Galvanic fluid. But if he places theſe two ends of the platina wires one upon the other without deranging thoſe placed in the mouth, he will again experience, though in a weaker degree, the ſenſation of the ſavour.

Experiment II.

If the Galvanic apparatus has produced only a *minimum* of effect, which muſt be the caſe, 1ſt, When the apparatus conſiſts of only one ſtory; 2d, When the ſubſtances which compoſe this ſtory are by their nature not much calculated for developing the Galvanic effect, or when they can produce it only in the weakeſt degree; it may happen under both theſe circumſtances that the organ of taſte will not be affected by any ſavour. But in this caſe it will be ſufficient to let the two lower ends of the platina wires reſt for a moment on the two extremities of the apparatus, and then to apply theſe two wires to each other without deranging their extremities, which are placed in the mouth. This new contact will neceſſarily produce a more intenſe effect than the former, and if it be required to increaſe it ſtill further, it may be done by applying alternately the lower ends of the platina wires, firſt to the two extremities of the battery, and then to each other. Theſe repeated contacts will develop the peculiar ſavour of the fluid, which is the certain ſign of its influence.

This ingenious proceſs, then, puts the tongue, that diſpoſable inſtrument, in poſſeſſion of a new *galvanoſcope*, which, as appears to us, will be to Galvaniſm what Volta's condenſer is to electricity, ſince by its means the ſlighteſt ſhades of the Galvanic effect may be detected.

C. Nauche, preſident of the ſociety, in the ſame ſitting communicated ſome facts in regard to the medical application of Galvaniſm in caſes of hemiplegia. It reſults from them, that the means of giving a ſtrong ſtimulus to the parts affected, ſuch as the hand or leg for example, is, to make one of the extremities of the pile to communicate with the former, and the other extremity with the ſpinal apophyſes of the ſixth and ſeventh cervical vertebræ. If the leg be affected, the foot and twelfth dorſal vertebra muſt be made to communicate with the two extremities of the pile. The vital action will then ſeem to be conveyed as if by undulation to all the muſcular organs in order to reſtore motion.

The preſident obſerved alſo, that in caſes of great extremity, where it is neceſſary to increaſe in an extraordinary degree the excitement of the organic forces, one extremity of the pile muſt be brought into communication with the ſummit of the vertebral column, and the other with the ſpinal apophyſes of the firſt lumbar vertebræ. The patient ſubjected to this experiment will perceive flaſhes, certain ſavours and ſenſations, more or leſs painful, in the ſtomach, in the inteſtinal canal, and in the viſcera of the breaſt and the abdomen. The muſcles of the trunk and thoſe of the extremities will be violently contracted.

A fact no leſs important, and which he confirmed alſo, is, that the application of Galvaniſm increaſes in a particular manner the action of the parotid, the lachrymal, and maxillary glands, that of the kidneys and of the whole lymphatic ſyſtem. To produce this action the apparatus muſt be directed, not to the glandulous organs, but as much as poſſible to the origin of the nervous trunks which diſtribute themſelves thither.

It may be readily conceived what advantage ſkilful anatomiſts may derive from an application of Galvaniſm to the different ſyſtems of the animal economy, eſpecially if to anatomical knowledge they unite a thorough acquaintance with the theory of rational medicine. TOURLET.

LXVI. *Intelligence and Miſcellaneous Articles.*

MINING.

THE mines of Cornwall and its neighbourhood now yield ſo large a part of the metals ſo valuable to the arts, copper and tin, of all that the world produces of theſe ſubſtances, that the preſent ſtate, and probable future one of productiveneſs, is highly intereſting to thoſe connected with the uſeful manufactures.

Tin, though the oldeſt object of the miner's ſearch in Cornwall, now employs by far the leſſer number of hands and amount of capital; the quantity raiſed of courſe is much diminiſhed. Generally ſpeaking, the tin mines have not been found to hold their richneſs to ſo great a depth beneath the ſurface as copper ſeems to do; conſequently many, which were highly productive mines of this metal, now are ſtopped, or are worked to little advantage.

Polgooth, the greateſt tin mine now in the world, though it produces large quantities of ore, yet it is attended with ſo great an expenſe from its depth, that is underſtood to pay the adventurers now but very little profit. A report of cobalt being found in this mine has been heard, but we do not know whether it is entitled to credit; we wiſh it may, but, as far as we have had opportunities of judging of the minerals produced in this diſtrict, it does not appear to us probable that theſe ſcarcer ones will be found in quantities ſufficient to repay any great expenſe in ſearching for them. The tin mines in the weſtern part of the county, between Penzance and the Land's End, are doing well for their proprietors in general; and though they have for a few years produced but little, they now are throwing up good quantities of ore.

The quality of the metal lately brought to market has been found, we underſtand, much inferior to what it ought to be: this may be accounted for in the following way:—Grain tin particularly has been greatly in demand; in order to get more of the metal under this form the ſmelters have of late applied ores to produce it from, which are generally underſtood not to be ſufficiently free from other metals for this purpoſe, and thus the grain tin, though increaſed in quantity, has been leſs pure: at the ſame time the common tin, being robbed of the better part of the ores from which that has generally been ſmelted, has ſuffered a like degradation in quality. It is to be hoped that the ſmelters will ſee very ſoon their true intereſt in this reſpect, and that the riſk of injuring the trade may no longer be continued for the proſpect of temporary gain.

The quantity of copper produced has not much fluctuated of late, though on the whole it may be expected, unleſs unlooked-for diſcoveries be made, to leſſen; as ſeveral large mines have already ſtopped, the produce not being ſufficient to counterbalance the enormous outgoings their great depth occaſioned. Some other mines that have for many years, and even of late, produced almoſt as much as any in this part of the world, are expected ſoon to be abandoned, partly for the reaſon above ſtated, and partly becauſe they have failed in quantity of ore in the lode or vein. On the other hand, ſome mines have bettered in their proſpects: Dolevath, after having been brought into work at an enormous expenſe, has afforded a courſe of copper of great magnitude, and though the ore is not rich in proportion of metal, yet its quantity is large enough to make this diſcovery highly valuable. Wheal Towan, in St. Agnes, continues its productive ſtate; and Wheal Crowndale, near Taviſtock in Devon, is yielding a great deal of copper ore from a ſmall depth under the ſurface.

Like tin, it has been found that copper is of late inferior in purity to what it was formerly, at leaſt it is aſſerted that the ſheathing of ſhips would a few years ſince remain in uſe a much longer time than any that can now be got will do. This probably ariſes in both metals from the ſame cauſe, from the greatneſs of the demand, which tempts the ſmelter to let improper alloys remain in a certain degree in his metal; but it well becomes them to reflect, whether this may not ſooner than they expect be the means of deſtroying the demand all together, by driving the conſumers to ſome ſubſtitute which may anſwer their purpoſe better. F. P. M. S.

ANTIQUITIES.

M. Groteſend has publiſhed an addition to his explanation of the Perſepolitan Inſcriptions. The author thought it neceſſary to prefix to this memoir an explanation of other larger inſcriptions, in order that an opinion may be eaſier formed of it. In the firſt place, the alphabet of the wedge-form writing of the firſt ſort has two fundamental ſtrokes, which may be called the bow and the arrow, or rather the chiſel and the rule. It appears to have been invented chiefly for lapidary inſcriptions. It has no round ſtrokes, and is not only very convenient for that purpoſe, but very beautiful, on account of its ſimple and regular compoſition. The author mentions as particular properties of this alphabet: 1ſt, The principal ſtrokes of the letters are perpendicular; the horizontal are the acceſſory ſtrokes. The latter are always on one ſide, or over the former; only in the letter *g*, there are at the top two wedges which croſs each other. 2d, The points of the arrows, or wedges, are always turned to the bottom or to the right, when the characters are written proceeding from the left. This poſition is natural. 3d, Each letter has no more than three wedges, or two angles, as principal ſtrokes, and at moſt three acceſſory ſtrokes; however, when an angle forms the principal ſtroke, there is at times an acceſſory ſtroke on the other ſide. 4th, The angles and principal wedges are all of an equal height, only that in two letters, *m* and *o*, the middle wedge is ſhorter, in order to prevent their being confounded with the *t* and the *a*, which are ſimilar. In regard to the tranſverſe, or acceſſory ſtrokes, one of them is often ſhortened; or one ſtroke, which ought to be at the top, is often placed at the ſide for want of room. 5th, In letters which have only one principal ſtroke, the tranſverſe ſtrokes are always on one ſide; where there are two principal ſtrokes, the tranſverſe ſtrokes are in general at the top; where there are three, theſe acceſſory ſtrokes are alſo on the ſide. 6th, When ſeveral tranſverſe ſtrokes are over one or two principal ſtrokes, the latter are ſhortened that the letter may not paſs the line. The alphabet itſelf is repreſented under a double point of view; firſt, according to the compoſition of the ſtrokes, where they begin by the ſimpleſt ſigns, the wedge with one or two acceſſory ſtrokes; and then according to the order of the letters of the Zendic alphabet. In the laſt table of Anquetil the author obſerved letters which he thinks defective, according to a compariſon with the plates of Bruyn and Niebuhr. In the laſt place, the author remarks, that this wedge alphabet has no reſemblance to any of the known alphabets; and that it would be vain to attempt to determine the meaning of the ſingle letters according to their reſemblance with the Zendic or other letters; the author, however, finds that the letters which reſemble each other in the Pehlvi writing at

Nakſchi Ruſtan, reſemble each other alſo in the wedge writing.

Secondly, What the author obſerves in regard to the Zendic language may be reduced to the following objects:—1ſt, The Zendic among the Perſian dialects is what the Shanſcrit is among the dialects of the Hindus, and has a great reſemblance to that language. Neither of them is formed, but they are abundant in words; both have a great many elevated vowels, and, on the other hand, have very harſh combinations of conſonants. The two languages have alſo ſeveral things common in the radical words and in the flexions, and are very different from other languages. 2d, The Zendic has a great variety of grammatical forms, and a conſtruction without rules; like the Greek and the German it abounds in compound words. The moſt important thing in this ſection is an example of the declenſion of the noun taken from inſcriptions, and which in ſeveral parts deviates from the declenſion of Anquetil. The ſingular, for example, has in the genitive and dative the termination *âbê*, (Anquetil has *tſchao* or *ao*;) the accuſative has *o*, (Anquetil has *m*,) *e*, and *etſchao*, (inſtead of the *bio* and *bietſcha* in Anquetil.) Theſe differences would be ſtriking did we not reflect, that the two ſorts of declination are taken from partial examples, and that we are leſs acquainted with the Zendic grammar than with its dictionary.

ASTRONOMY.

Table of the geocentric motion of the two new planets for June 1803.

	Geocentric Motion of Pallas.					Geocentric Motion of Ceres Ferdinandea.				
	Right Aſcenſion.			Declin. North.		Right Aſcenſion.			Declin. South.	
June 2	19^h	7^m	13^s	26°	2′	18^h	45^m	0^s	22°	29′
5	19	5	27	26	16	18	43	2	22	43
7	19	3	30	26	31	18	40	57	22	56
11	19	1	23	26	45	18	38	44	23	5
14	18	59	3	27	0	18	36	27	23	12
17	18	56	35	27	14	18	34	6	23	16
20	18	53	56	27	29	18	31	38	23	19
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fourth. As the quantity of this metal diminiſhes, the ſpecific gravity increaſes; the ductility is leſs; and after 12·5 it begins to be no longer acted upon by nitric acid.

Mr. Chenevix gives many methods for preparing a ſubſtance ſimilar to palladium; but the following is the leaſt liable to fail:—Take a certain quantity of platina, reduced from the triple ammoniacal ſalt, and diſſolve it in nitro-muriatic acid: add ſomething more than twice the weight of the platina of red oxide of mercury, and take particular care to ſaturate all exceſs of acid: then pour it into a ſolution, alſo ſaturated, of green ſulphate of iron; heat the mixture, and a black precipitate will be formed after ſome time. This precipitate weighs rather more than two and a half the original quantity of the platina; but, expoſed to a red heat, a part of the mercury is volatilized. Fuſed at a ſtronger heat, it yields a metallic button, which in different experiments gives a different ſpecific gravity. This experiment is of a very delicate nature, and does not always ſucceed; and Mr. Chenevix recommends perſeverance and repeated trials to all who would form palladium. It is not often to be obtained of the exact ſpecific gravity announced by the author of the fraudulent advertiſement; and the ſpecific gravity is not conſtantly the ſame in all his ſpecimens. The difficulty is, to unite a ſufficiently large portion of mercury with the platina to produce perfect palladium. It may be obtained by this method as low as 10·5, ſometimes as high as 15. Mr. Chenevix mentions ſome methods of combining platina and mercury in the dry way, ſuch as count Muſhin Puſkin's amalgam fuſing with cinnabar, lime, and borax, &c.

He then relates ſome experiments upon platina; upon its oxides, which are two, viz. one of 7 the other of 13 per cent. of oxygen; upon the affinities of the oxide for the acid and aromatic ſalts formed by them. He gives numerous examples of the affinities of metals for each other; and of anomalous precipitations, when two or more metals are preſent in the ſame ſolution. He ſays, that the fixation of mercury by platina, together with other facts related in this paper, will tend to make us particularly cautious in pronouncing upon diſcoveries of ſuppoſed new metals, and promote ſcepticiſm even upon our preſent knowledge. He adduces inſtances of the difference that exiſts between a ſolution of platina and a ſolution of mercury merely mixed, and a ſolution of theſe metals after they have been united. In the former caſe they are eaſily ſeparated; in the latter there is no method of completely diſuniting them. He concludes with a curious ſtatement of the approximation of ſeveral metals and of ſeveral earths to each other in their chemical properties, which follow

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petitors will include in it any of thoſe opinions of the antient or modern writers which, being founded on ſuperſtition or ignorance, would excite only diſguſt. It rather wiſhes that they would ſelect, explain, and examine the opinions of Ariſtotle, Theophraſtus, Pliny, Seneca and Ptolemy, among the antients, and thoſe of the philoſophers of the 16th and 18th centuries, particularly Teleſius, Patricius, Bruno, lord Bacon, Kepler, Gaſſendi, and Deſcartes, a knowledge of which may be of uſe to meteorology at preſent: it requires alſo, that the manner in which the obſervations were made, and the inſtruments employed, may be briefly deſcribed, marking at the ſame time the climate. An inquiry may alſo be made reſpecting the origin of that opinion, which became ſo prevalent, in regard to the influence of the planets on meteors: and, in the laſt place, the author may briefly and generally determine how far we can approach nearer to the truth by improving phyſical aſtronomy and meteorology.

The prize for each of theſe queſtions is fifty ducats, and the memoirs muſt be tranſmitted to the ſociety before the month of September each year.

GALVANIC SOCIETY, PARIS.

Galvanic facts, in proportion as they are accumulated, afford a hope of the happieſt reſults being ſoon obtained from the application of this ſtimulus. Thoſe which we are here about to detail relate partly to theory and partly to the medical employment of this fluid.

In a late ſitting of the Galvanic Society, C. Gautherot mentioned an obſervation made by him anterior to that period, which was both curious in itſelf, and of importance, on account of the inductions that may be drawn from it. As it is neceſſary to be known before a proper opinion can be formed of thoſe by which it was accompanied, we think it proper to lay before the reader the two following experiments:

Experiment I.

The perſon who performs this experiment places in his mouth the upper ends of two wires of platina, or of any other metal not oxidable, and immediately brings the other two ends into contact with the two extremities of a weak Galvanic apparatus, to aſcertain the degree of its influence. When this arrangement has been made, the perſon will experience an effect more or leſs remarkable, according as the tongue is more or leſs exerciſed in perceiving the peculiar ſavour of the Galvanic fluid. But if he places theſe two ends of the platina wires one upon the other without deranging thoſe placed in the mouth, he will again experience, though in a weaker degree, the ſenſation of the ſavour.

Experiment

Experiment II.

If the Galvanic apparatus has produced only a ***minimum*** of effect, which muſt be the caſe, 1ſt, When the apparatus conſiſts of only one ſtory; 2d, When the ſubſtances which compoſe this ſtory are by their nature not much calculated for developing the Galvanic effect, or when they can produce it only in the weakeſt degree; it may happen under both theſe circumſtances that the organ of taſte will not be affected by any ſavour. But in this caſe it will be ſufficient to let the two lower ends of the platina wires reſt for a moment on the two extremities of the apparatus, and then to apply theſe two wires to each other without deranging their extremities, which are placed in the mouth. This new contact will neceſſarily produce a more intenſe effect than the former, and if it be required to increaſe it ſtill further, it may be done by applying alternately the lower ends of the platina wires, firſt to the two extremities of the battery, and then to each other. Theſe repeated contacts will develop the peculiar ſavour of the fluid, which is the certain ſign of its influence.

This ingenious proceſs, then, puts the tongue, that diſpoſable inſtrument, in poſſeſſion of a new *galvanoſcope*, which, as appears to us, will be to Galvaniſm what Volta's condenſer is to electricity, ſince by its means the ſlighteſt ſhades of the Galvanic effect may be detected.

C. Nauche, preſident of the ſociety, in the ſame ſitting communicated ſome facts in regard to the medical application of Galvaniſm in caſes of hemiplegia. It reſults from them, that the means of giving a ſtrong ſtimulus to the parts affected, ſuch as the hand or leg for example, is, to make one of the extremities of the pile to communicate with the former, and the other extremity with the ſpinal apophyſes of the ſixth and ſeventh cervical vertebræ. If the leg be affected, the foot and twelfth dorſal vertebra muſt be made to communicate with the two extremities of the pile. The vital action will then ſeem to be conveyed as if by undulation to all the muſcular organs in order to reſtore motion.

The preſident obſerved alſo, that in caſes of great extremity, where it is neceſſary to increaſe in an extraordinary degree the excitement of the organic forces, one extremity of the pile muſt be brought into communication with the ſummit of the vertebral column, and the other with the ſpinal apophyſes of the firſt lumbar vertebræ. The patient ſubjected to this experiment will perceive flaſhes, certain ſavours and ſenſations, more or leſs painful, in the ſtomach, in the inteſtinal canal, and in the viſcera of the breaſt and the abdomen. The muſcles of the trunk and thoſe of the extremities will be violently contracted.

A fact no leſs important, and which he confirmed alſo, is, that the application of Galvaniſm increaſes in a particular manner the action of the parotid, the lachrymal, and maxillary glands, that of the kidneys and of the whole lymphatic ſyſtem. To produce this action the apparatus muſt be directed, not to the glandulous organs, but as much as poſſible to the origin of the nervous trunks which diſtribute themſelves thither.

It may be readily conceived what advantage ſkilful anatomiſts may derive from an application of Galvaniſm to the different ſyſtems of the animal economy, eſpecially if to anatomical knowledge they unite a thorough acquaintance with the theory of rational medicine. TOURLET.

LXVI. *Intelligence and Miſcellaneous Articles.*

MINING.

THE mines of Cornwall and its neighbourhood now yield ſo large a part of the metals ſo valuable to the arts, copper and tin, of all that the world produces of theſe ſubſtances, that the preſent ſtate, and probable future one of productiveneſs, is highly intereſting to thoſe connected with the uſeful manufactures.

Tin, though the oldeſt object of the miner's ſearch in Cornwall, now employs by far the leſſer number of hands and amount of capital; the quantity raiſed of courſe is much diminiſhed. Generally ſpeaking, the tin mines have not been found to hold their richneſs to ſo great a depth beneath the ſurface as copper ſeems to do; conſequently many, which were highly productive mines of this metal, now are ſtopped, or are worked to little advantage.

Polgooth, the greateſt tin mine now in the world, though it produces large quantities of ore, yet it is attended with ſo great an expenſe from its depth, that is underſtood to pay the adventurers now but very little profit. A report of cobalt being found in this mine has been heard, but we do not know whether it is entitled to credit; we wiſh it may, but, as far as we have had opportunities of judging of the minerals produced in this diſtrict, it does not appear to us probable that theſe ſcarcer ones will be found in quantities ſufficient to repay any great expenſe in ſearching for them. The tin mines in the weſtern part of the county, between Penzance and the Land's End, are doing well for their proprietors in general; and though they have for a few years produced but little, they now are throwing up good quantities of ore.

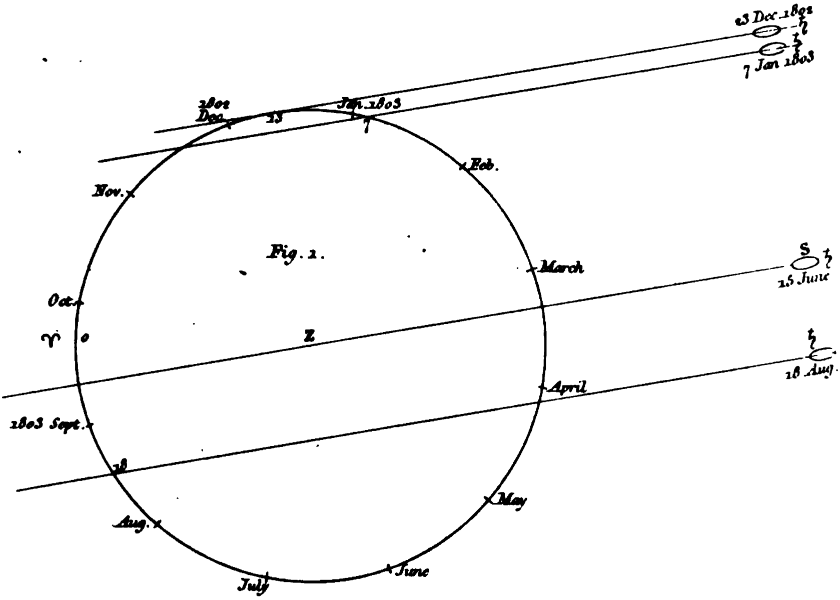
The quality of the metal lately brought to market has been found, we underſtand, much inferior to what it ought to be: this may be accounted for in the following way:—Grain tin particularly has been greatly in demand; in order to get more of the metal under this form the ſmelters have of late applied ores to produce it from, which are generally underſtood not to be ſufficiently free from other metals for this purpoſe, and thus the grain tin, though increaſed in quantity, has been leſs pure: at the ſame time the common tin, being robbed of the better part of the ores from which that has generally been ſmelted, has ſuffered a like degradation in quality. It is to be hoped that the ſmelters will ſee very ſoon their true intereſt in this reſpect, and that the riſk of injuring the trade may no longer be continued for the proſpect of temporary gain.

The quantity of copper produced has not much fluctuated of late, though on the whole it may be expected, unleſs unlooked-for diſcoveries be made, to leſſen; as ſeveral large mines have already ſtopped, the produce not being ſufficient to counterbalance the enormous outgoings their great depth occaſioned. Some other mines that have for many years, and even of late, produced almoſt as much as any in this part of the world, are expected ſoon to be abandoned, partly for the reaſon above ſtated, and partly becauſe they have failed in quantity of ore in the lode or vein. On the other hand, ſome mines have bettered in their proſpects: Dolevath, after having been brought into work at an enormous expenſe, has afforded a courſe of copper of great magnitude, and though the ore is not rich in proportion of metal, yet its quantity is large enough to make this diſcovery highly valuable. Wheal Towan, in St. Agnes, continues its productive ſtate; and Wheal Crowndale, near Taviſtock in Devon, is yielding a great deal of copper ore from a ſmall depth under the ſurface.

Like tin, it has been found that copper is of late inferior in purity to what it was formerly, at leaſt it is aſſerted that the ſheathing of ſhips would a few years ſince remain in uſe a much longer time than any that can now be got will do. This probably ariſes in both metals from the ſame cauſe, from the greatneſs of the demand, which tempts the ſmelter to let improper alloys remain in a certain degree in his metal; but it well becomes them to reflect, whether this may not ſooner than they expect be the means of deſtroying the demand all together, by driving the conſumers to ſome ſubſtitute which may anſwer their purpoſe better. F. P. M. S.

ANTIQUITIES.

M. Groteſend has publiſhed an addition to his explanation



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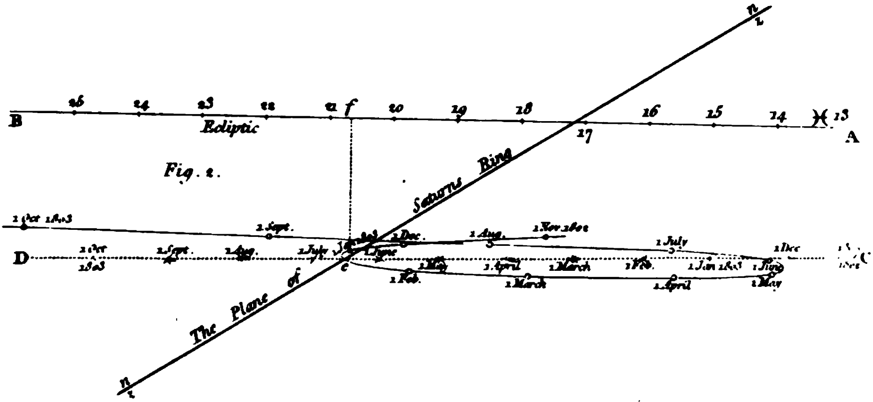

Fig. 2.

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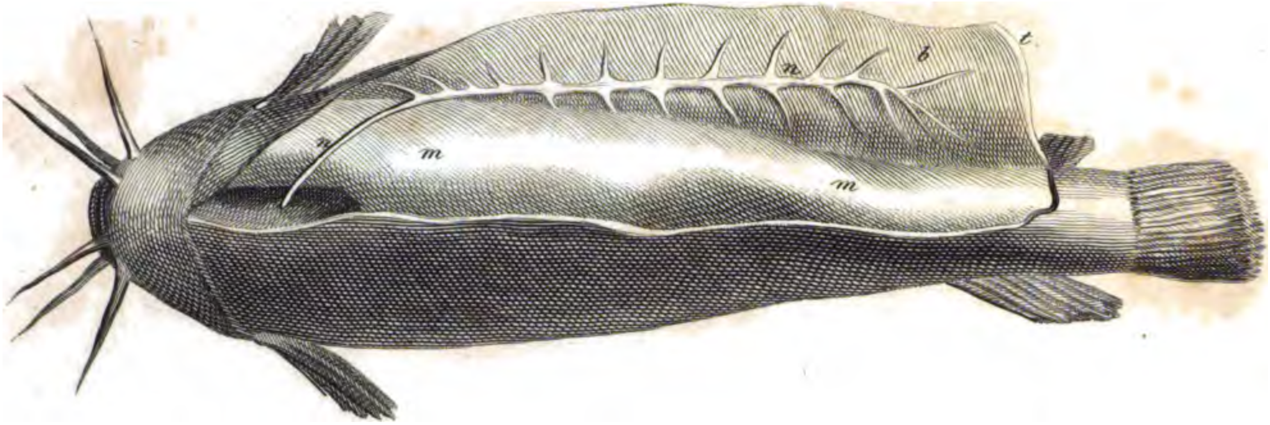

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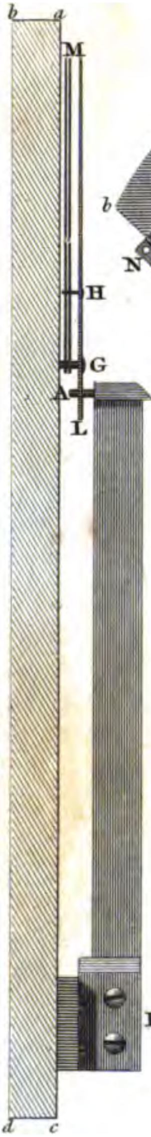

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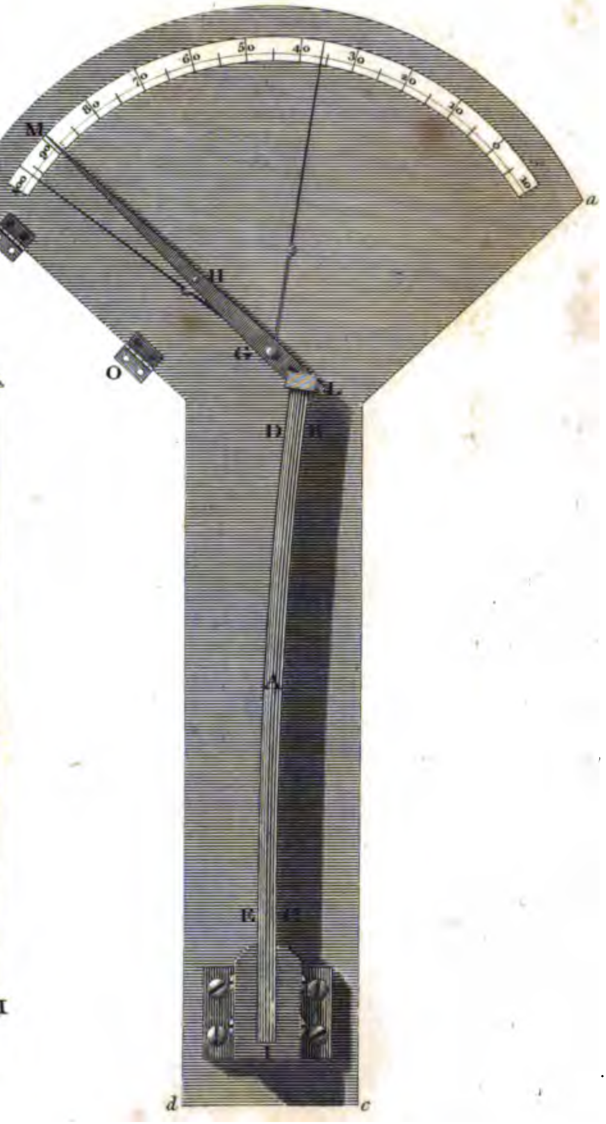

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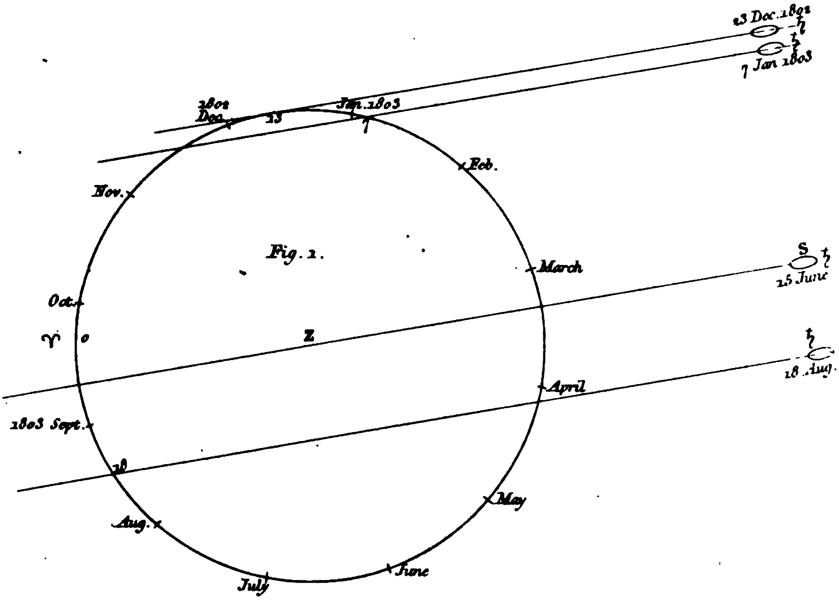

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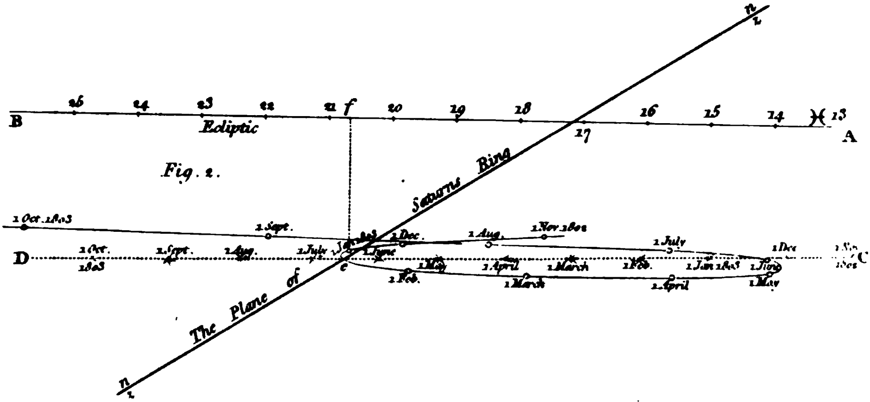

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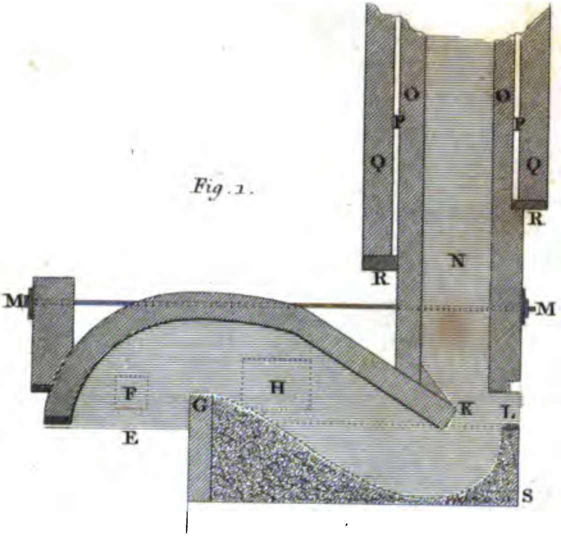

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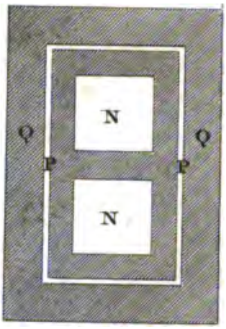

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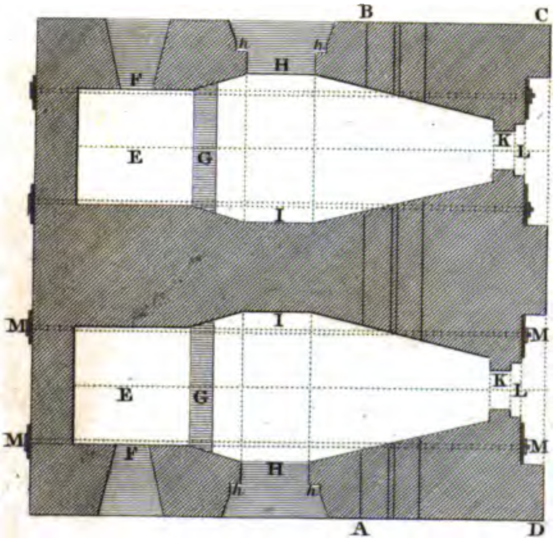

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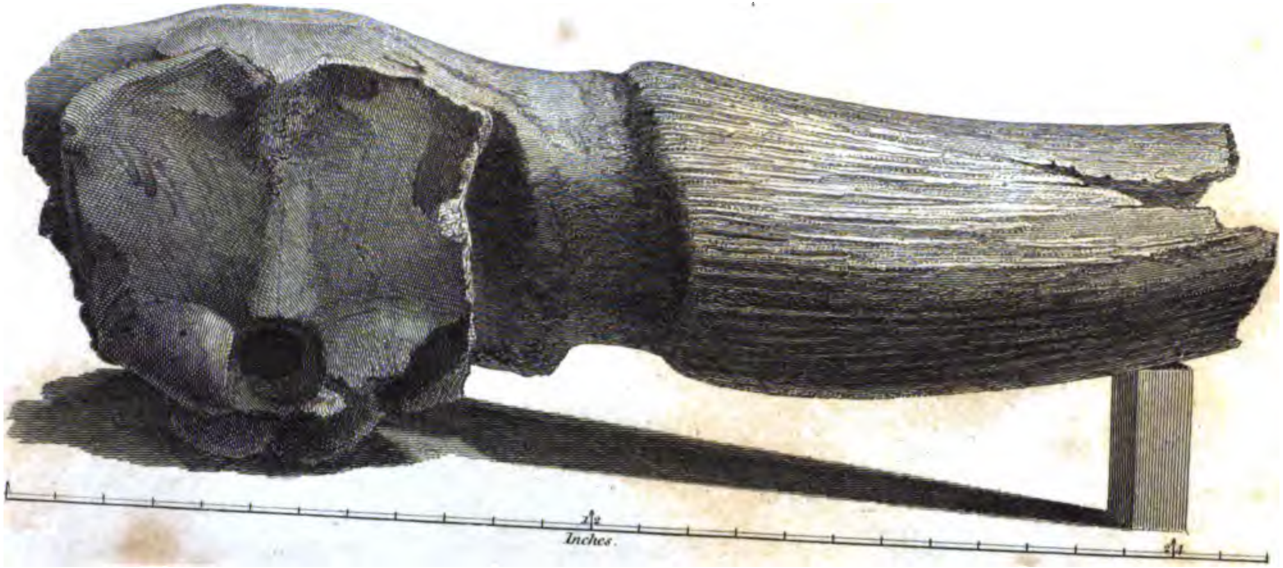

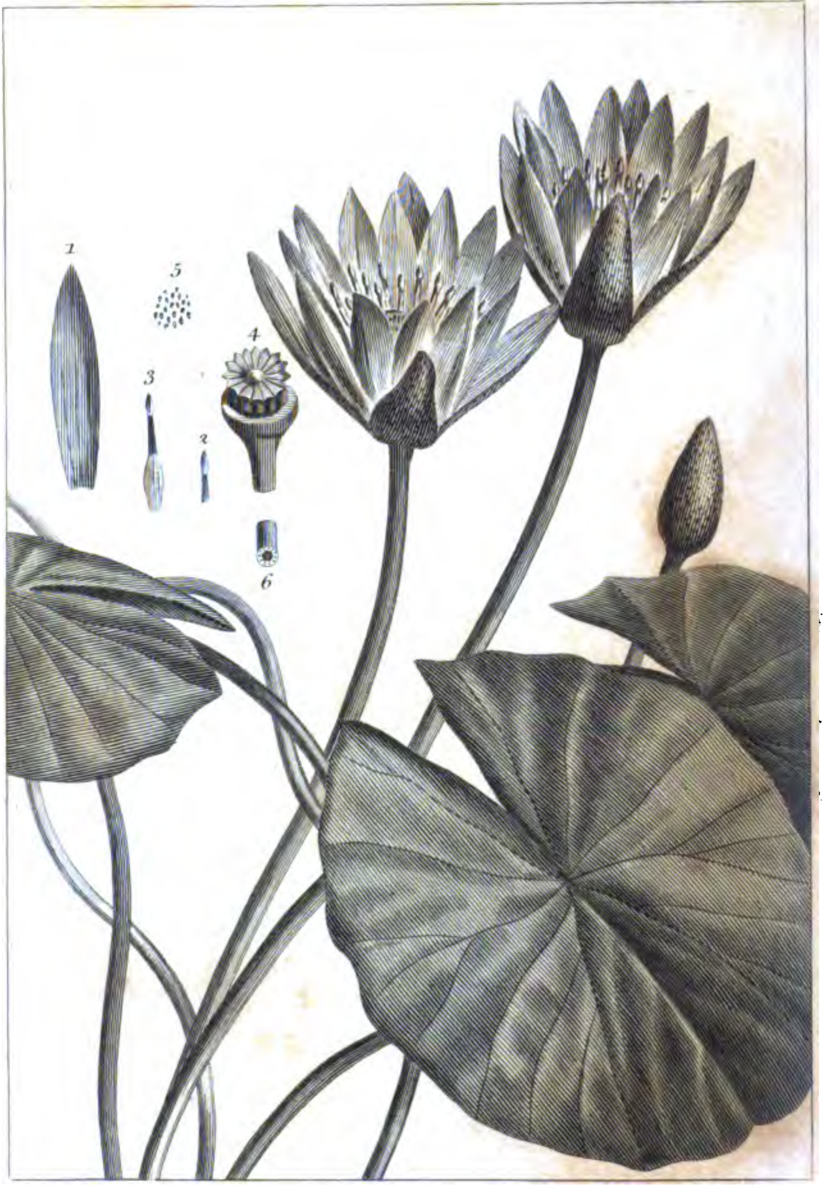

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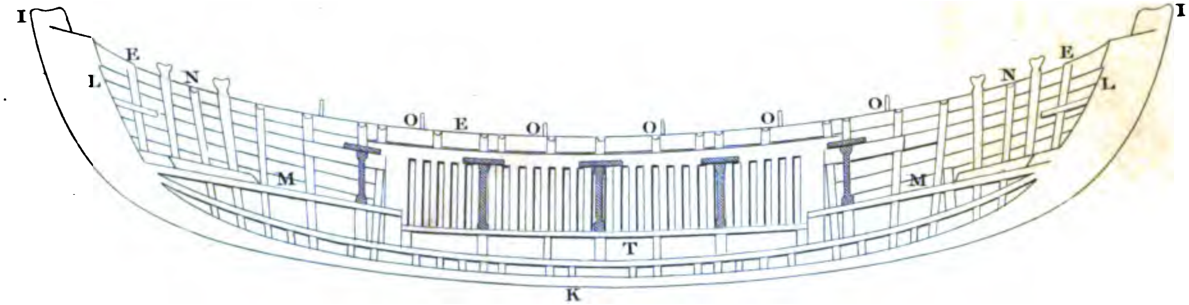
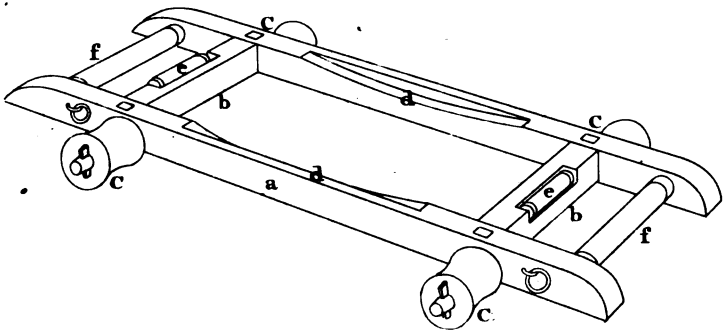
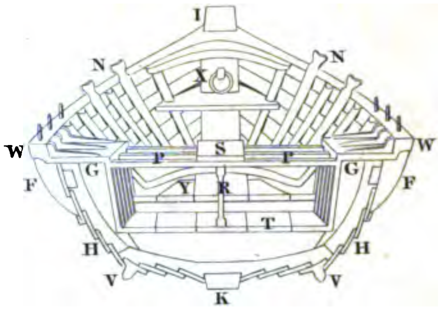
Philo. Mag. Pl. VIII. Vol. XV.







Lowry sculp.

Engraved by C. Knight, from an original in the poſſeſsion of M^{r} Colnaghi

M^{r} Ramsden

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PHILOSOPHICAL MAGAZINE:

COMPREHENDING
THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
AGRICULTURE, MANUFACTURES,
AND
COMMERCE.

BY ALEXANDER TILLOCH,
HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE

PHILOSOPHICAL MAGAZINE.

I. *Researches respecting the Organization of Leaves. By* A. JURINE, *Member of the Society of Physics and Natural History at Geneva*[*].

WHEN two naturalists study the same subject without communicating to each other the result of their labours, two certain advantages arise to science. The first is, that when they agree in their manner of considering the same object, their opinion excites a greater degree of confidence: the second is, that when their opinions differ it induces them to make new researches.

The important discoveries published by Saussure, Hedwig, and other botanists, in regard to the organization of leaves, inspired me with a desire of examining after them objects so interesting. I therefore repeated their observations, and made new ones, which gave me different results. Of these I proposed to publish a detailed account, when I read in the *Journal de Physique* the memoirs of C. Mirbel on the anatomy of vegetables; and as my ideas in regard to these objects approach very near to his, I shall here only give an extract, as it were, of my researches on that subject, excluding all hypothetical reasoning.

Though the opinion of the celebrated men above mentioned is of great weight, I will venture to say that it ought not to be so far respected as to make one afraid of entertaining a contrary opinion, when it is founded, as I believe, on facts well established. When my ideas, therefore, are different from theirs, I shall enter into some details, in order to show better on what the diversity of our opinions is founded.

The existence of the epidermis of leaves is a question of too much importance in the physiology of vegetables to be neglected. I have therefore examined it with the greater care, as C. Mirbel has given an opinion entirely different from that of most botanists.

---

[*] From the *Journal de Physique*, Ventose, an. 11.

De Saussure found that the covering of leaves, to which botanists have given the name of *epidermis*, is not a simple membrane, but a real bark composed of three distinct parts; namely, *an epidermis properly so called, a cortical reticulation, and cortical glands*[*].

He defines the epidermis in the following words[†]:—"It is an exceedingly fine membrane, always transparent and colourless, in which no fibre, no pore, and no organization can be discovered;" though he allows that this membrane opens opposite to absorbent and excretory vessels.

Hedwig gives the name of *pellicle* of the leaves to what Saussure calls their bark. The *cuticular lymphatic vessels* of the former compose the cortical reticulation of the latter; and the evaporating *pores* or *conduits* of Hedwig are the cortical glands of Saussure. In regard to the epidermis, as this author does not mention it, there is reason to believe that he has confounded it with the pellicle[‡].

To enable the reader to comprehend better what I have to say in regard to the epidermis, I shall define what I mean by *utriculæ*, as I consider this definition necessary to prevent misconception.

The utriculæ are membranous vesicles filled with a particular juice, and contiguous to each other, in a greater or less extent of their surface, according to the form which they affect. The utriculæ constitute the greater part of leaves. They vary in their form and size; some of them being round, elongated, prismatic, or irregular.

All leaves seem to have a bark or exterior covering destined to contain the parenchyme and the vessels. In several plants indeed, and particularly the orchis, this kind of bark is seen to separate itself spontaneously by the effect of a peculiar alteration in the leaf. The surface of the leaves may also be frequently removed by employing caution: some then think they see the epidermis in this fine pellicle; and they are the more convinced of it, as they compare it to that of animals. But if this pellicle be examined with attention, it will be found, as I have always seen, that it is formed by the exterior stratum of the utriculæ, the lateral faces of which are contiguous. I shall, however, except the leaf of the *fritillaria*[§], from which

[*] Observations sur l'Ecorce des Feuilles et des Petales; Geneve 1765.

[†] Chap. v. page 94.

[‡] Sammlung seiner zerstreuten Abhandlungen und Beobachtungen über Botanische-œconomische Gegenstænde D. I. Hedwigs; Leipsic 1790.

[§] *Fritillaria regalis* Linn.

I was able to separate a pellicle, which carried with it only the exterior face of these utriculæ, as seen fig. 3 and 4.*

To find the epidermis of Saussure I repeated the same processes which he employed, varying them different ways; but instead of epidermis I always observed that the surface of the leaves was formed only by the external face of the exterior utriculæ; that this face was distinguished by slight rugæ, produced, no doubt, by exposure to the air; that it was indeed a little thicker than the other faces of the same utriculæ, and on that account exceedingly proper for discharging the functions of an epidermis. It needs not be objected that this thickness supposes an epidermis connected with the exterior face of the utriculæ, for in this manner we ought to admit nothing which is not supported by rigorous proof: but though I was never able to separate the epidermis from the surface of the leaves, I am in doubt whether I should thence conclude that it does not exist.

C. Mirbel entertained the same opinion as I do † in regard to the non-existence of the epidermis in leaves, and expresses it in the following manner:—"The exterior cells (which are my utriculæ) closely united exhibit contiguous surfaces, which many authors have considered as an epidermis. It was natural that they should believe in the existence of this organ, since they considered the vegetable as composed of fibres, vessels, and utriculæ. All these parts, in their opinion, being only joined or weakly united, ought to have a common connection or covering to retain them in their respective places: but, according to the facts which I have established, the existence of the epidermis does not appear to be necessary; and the more I observe, the more I am convinced that it does not exist. The sides of the cells exposed to the contact of the air and of the light undergo an alteration to which the interior parts are not exposed; they become dry, and are even separated sometimes from the cellular tissue. It is thus that they become a distinct covering, and it is then that they are destroyed: but if this pretended epidermis be carefully removed in the green parts, its continuity with the cellular tissue will be perceived; the vascular reticulation of Saussure is seen adhering to that exterior membrane; and this reticulation, when better observed, appears to be only the lateral sides of the cells, while the epidermis or membrane, which serves them as a base, is nothing else than the junction of the exterior sides *."

---

* The different figures referred to in this article will be found in Plates I and II.

† Journal de Physique, Prairial, an. 9, p. 443.

The observations even of Saussure seem to confirm my opinion in regard to the non-existence of the epidermis, since he expresses himself in this manner †:—"The system of the vessels which constitutes the bark of the leaves, and which touches on the one hand the parenchyme and the ribs of the leaves, and on the other the epidermis properly so called—this system of vessels generally adheres to its epidermis much stronger than to any thing else. It, however, sometimes happens that a part of the reticulation remains adhering to the parenchyme of the bark; but this is very rare. Continued maceration in cold or boiling water till the leaf is dissolved, separates very rarely the cortical reticulation from its epidermis. I never saw this separation take place but in the *sonchus spinosus*."

May it not thence be inferred that this author was never able to separate the epidermis without removing with it the vessels, except in the leaves of the sonchus? This appears to me evident. But if I have always found in this fine pellicle the utricular organization, and if I am able to prove that the vessels of its cortical reticulation are nothing else than the lateral faces of the exterior utriculæ, may I not reasonably conclude that this epidermis is merely the external face of the exterior utriculæ?

The *cortical reticulation* of Saussure, covered by the epidermis, is composed of fibres or filaments, which by anastomosing form the meshes of this reticulation, and the form of these meshes varies according to the different plants.

This author says ‡: "In all the leaves which I have observed, four, five, six, and even seven filaments terminate at one and the same mesh: though six be the most frequent number, it does not thence follow that these meshes are regular hexagons; for the fibres which compose this reticulation are subject to such frequent inflections, and proceed winding in so irregular a serpentine manner, that in the greater number of species the meshes have no regular or constant form. The filaments which compose these meshes appear transparent in their axes, and they are seen to anastomose with each other wherever they meet, and not to cross or to form knots in any manner. As these characters are suited to vessels filled with a transparent fluid much rather than to simple solid fibres, I have always considered them as vessels, and shall give them that name in future. The fineness and transparency of these vessels induce me to consider them as lymphatic vessels."

---

* In my opinion, C. Mirbel is mistaken when he applies to the epidermis of leaves what Malpighi said respecting the epidermis of trees, namely, that it is formed by the desiccation of the utriculæ. Malpighi expresses himself in regard to leaves as follows, p. 37: "Tota exaratorum moles quibus folia coagmentantur, lævi superextensa cuticula seu epidermide abducitur, quæ subjectorum colorem refert, ipsaque contenta custodit et continet."

† Page 40 and 41. ‡ Page 32 et seq.

Hedwig observed the same reticulation, which he says is formed by vessels to which he has given the name of *cuticular lymphatic vessels*.

These two authors have committed an error in taking for vessels the lateral faces of the utriculæ which form the surface of the leaves; but this error, which is owing to an optical illusion, was easily committed. If the pellicle of a leaf, indeed, be removed, and be examined with the microscope, it will readily be conceived, that if the utriculæ which compose this pellicle be viewed from above, their lateral or perpendicular faces will appear shortened; and besides, as these membranes may be more or less inclined on account of their flexibility, the result will be, that the lower ridges will appear to be close to the upper corresponding ones, which will give the illusion of vessels of different diameters, the transparency of the axes of which will depend on that of the membrane comprehended between these ridges.

Fig. 10 will serve to give a better idea of the optical illusion which I have here endeavoured to explain.

As the lateral faces of the exterior utriculæ thus present themselves under the appearance of vessels, it is seen that the result will be a kind of reticulation, the meshes of which will vary according to the form of the utriculæ: for example, they will be rectangular, hexangular, or irregular, according as the utriculæ represent a parallelopipedon, a hexaëdral prism, or any other irregular form.

From what has been said, I entertain no doubt that if the authors I have quoted had examined the pellicle of the leaves in profile or in vertical sections, instead of observing them in front or from above, they would have found that what they take for vessels are merely the lateral faces of the utriculæ.

C. Mirbel in his memoir above mentioned seems to ascribe the errors committed on this subject to the same cause. He says, "The difficulty of observing, by the help of the microscope, the relation which exists between the planes placed in different directions, and at unequal distances,

tances, has fascinated the eyes of observers. The membranes which form the sides of the cells have always been taken for vessels or for fibres."

The utriculæ which compose the surface of the leaves have in general a form different from that of the utriculæ of the parenchyme, and a direction which is often opposite to them. In the *fritillaria*, for example, the form of the exterior utriculæ approaches that of a parallelopipedon very much elongated, in the direction of the length of the leaf (fig. 1. A, and fig. 5.), while the interior utriculæ are nearly spherical, as seen in fig. 7, 8, and 15.

The *erythronium dens canis* has its exterior utriculæ also elongated in the direction of the length of the leaf; they cross at right angles those of the parenchyme, which are cylindrical, and which consequently have their direction according to the breadth of the leaf. This disposition is not well observed but in the superior surface of the leaf.

The leaves of the *lavatera triloba* have their exterior utriculæ festooned like those of fig. 14; but the interior ones exhibit the form of a cylinder, the length of which is in the direction of the thickness of the leaf.

The exterior utriculæ of the leaf of the *silphium perfoliatum* are also festooned; while the utriculæ of the parenchyme are cylindric on the superior side of the leaf, and irregular on the inferior, like those of fig. 18.

In the *orchis maculata* the exterior utriculæ of the leaf are remarkable by their size, and particularly by their height: they alone constitute a great part of the thickness of the leaf.

I might multiply these examples; but I think I have said enough to prove what I have advanced, namely, that the exterior utriculæ of the leaves are in general very different from the utriculæ of the parenchyme.

Having examined the existence of the epidermis and that of the lymphatic vessels which form the cortical reticulation of the leaves, I shall proceed to the cortical glands of Saussure, or the evaporating pores of Hedwig; but I shall first say a few words of the manner in which the exterior utriculæ of the petals present themselves.

In most flowers the exterior utriculæ of the petals are conical; they rise in the form of teats, more or less prominent, on which the rays of light are broken and reflected in such a manner as to produce to our eyes that dark rich velvety appearance which gives them so beautiful an aspect.

To give an idea of the effect produced by these mammellous utriculæ I shall compare them to that of velvet, which exhibits that appearance merely in consequence of the projection of the threads of silk above the woof.

Petals with a smooth surface do not exhibit the same phænomenon, because their exterior utriculæ being plane do not reflect the rays of light like the conical utriculæ; and in flowers, the petals of which are velvety above and smooth below, the utriculæ are seen to exhibit both these forms.

The surface of leaves is furnished with a very large quantity of small whitish points, which are scarcely apparent to the naked sight, but which may be clearly distinguished by the help of a good magnifying glass. These points are the *cortical glands* of Saussure, the *milliary glands* of Guettard, the *evaporating pores* of Hedwig, and the *cortical pores* of Decandolle.

Saussure says that the cortical glands are small oval bodies, often transparent and at other times opake, surrounded by a fibre or vessel of the same form, and with which several vessels of the cortical reticulation anastomose. At the two extremities of each gland is a very delicate vessel, which traverses in a straight line the interval between the gland and the vessel that surrounds it, with which it anastomoses. He considers these glands, on account of their constant position near the surface of the leaf, as organs destined for preparing or secreting the peculiar juices which form the matter for transpiration, or for preparing and assimilating to vegetables the vapours and exhalations which they absorb by their leaves.

Guettard, without penetrating into the organization of these glands, contents himself with giving them the appellation of *milliary*, on account of their great number in certain leaves.

Hedwig first found that these organs are so many small apertures, which he calls *evaporating pores* or *conduits*. He says that each pore is surrounded by a circumference, and that the interval comprehended between the pore and the circumference is the reservoir of the pore. In regard to the use of these potes, he has no doubt that they serve for evaporation or absorption.

Decandolle has also given them the name of *cortical pores*, and seems to have adopted the opinion of Hedwig in regard to the uses of them.

Senebier, in his Treatise on the Physiology of Vegetables, says that he was never able to discover, even with the help of the best microscopes, the pores of Hedwig, which I can ascribe only to an ambiguity occasioned by the meaning of the word, since the pores of Hedwig are only the cortical glands of Saussure, with which the illustrious author of that physiology seems to have been very well acquainted.

Mirbel calls these pores *exterior pores*, in contra-distinction to those with which the interior cells are perforated[*]: "Each pore[†]," says he, "is an oblong fissure surrounded by an elliptical area at which the neighbouring cells terminate. In these pores I can see only cells having a particular disposition, the exterior side of which is rent in a longitudinal direction. When the vegetable is exposed to the air and to light, circumstances necessary for transpiration, the fluids proceed in abundance towards the surface, the cells become elongated to receive them, and pierced to afford them a passage. Hence the formation of the cortical pores: but this elongation of some cells is, in a certain measure, only accidental; it is not invariably determined by the organic plan of the vegetable."

The diversity of opinions which prevails among the authors above mentioned induced me to study these organs with great attention, that I might endeavour to discover their real structure, and in this manner facilitate the explanation of their uses.

I have found that the surfaces of almost all leaves are perforated with a great number of small apertures, to which I shall retain the name of *pores*.

The size of these pores varies very much, according to the plants. In the orchis and lily kind they are very large, while in the sow-bread, the jessamine, the oak, &c. they are very small.

The number of them varies also according to the leaves. Hedwig counted 577 in a square line of the *lilium bulbiferum*. I counted 140 in the same extent of a leaf of the fritillaria, and 150 in that of the aloe; but in leaves where they are very small I was not able to count them very exactly, because they are too numerous.

In most plants the pores have an oval form, the direction of which seems to be subordinate to the form of the exterior utriculæ. Thus in the lily kind, the utriculæ of which are elongated, the large diameter of the pore proceeds in the direction of the length of the utriculæ, fig. 1 and 5; and in the leaves, the utriculæ of which are festooned, the pores extend in every direction, fig. 14. This rule, however, is not constant; for in the white serapias, the utriculæ of which are also festooned, the pores are disposed very regularly in the direction of the length of the leaf.

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[*] Journal de Physique, Fructidor, an. 9, p. 212.

[†] Ibid. Prairial, an. 9, p. 444—448.

All leaves are not in the same manner provided with pores: some have pores on both surfaces, others have them only on one; and some are entirely destitute of them. At the end of this memoir I shall give a catalogue of most of the plants which I have examined, and which I have arranged in three tables according to these divisions.

Trees in general have pores only in the interior surface of their leaves. Those, however, of the fir and the larch have them on both their surfaces; and the *juniperus sabina* and the *common juniper* exhibit pores only on the upper face.

But pores are not found in leaves alone: they are found in the seminal leaves, in most herbaceous stems, and in that of the *cactus flagelliformis* which has no leaves; they are also disseminated throughout the bracteæ, stipulæ, calyces, and in the corolla without a calyx: I have even found them in the corolla with a calyx, though in less quantity; but I will not assert that they are all provided with them. In a word, I have observed them in the antheræ, the pistils, and pericarpium of the *lilium bulbiferum;* and I have no doubt that they are found also in the parts of fructification of other plants.

The existence of pores in the leaves having been fully established by various observations, I shall now show the manner in which I found them to be formed.

Were we to suppose that the pores are apertures formed by chance in the surface of the leaves, we should be in an error. Nature had some end in forming them, and the attentive observer will soon perceive that it has given to these pores a peculiar organization. Each pore indeed, when observed with the microscope, seems to be placed in the middle of a body nearly spherical, composed of two small uniform utriculæ, perfectly similar, opposed to each other by their concave faces and united by their extremities, the result of which is a vacuity or interval of an oval form, to which I have given the name of pore, fig. 1, 5, and 6.

These reniform utriculæ, which I shall call *conjugate,* concur with the exterior utriculæ towards the formation of the surface of the leaf; they are so closely united, that when the pellicle is removed they are removed with it, except in the fritillaria and aloe; which arises, no doubt, from the pellicle being formed, as already said, merely by the superior face of the utriculæ, the lateral surfaces of which adhere to the parenchyme of the leaf, and thus retain the utriculæ *conjugate or conjoined.*

To enable the reader to comprehend better what is here said, I shall refer to fig. 1, which represents a portion of the leaf of the fritillaria with the pellicle removed at B. It will first be observed that the leaf is greener in that part; that the lateral surfaces of the exterior utriculæ remain adhering to the parenchyme, and that they retain the utriculæ *conjoined*, in the centre of which the pore is seen.

Fig. 2 exhibits this pellicle removed, and pierced with as many oval apertures as there were *conjugate* utriculæ in the leaf.

The size of these apertures clearly proves that, besides the pore, a part of the *conjugate* utriculæ remains exposed to the air, which thus concurs with the exterior utriculæ to the formation of the surface of the leaf. Fig. 5 and 6 give a pretty correct idea of the manner in which the *conjugate* utriculæ are enveloped by the exterior utriculæ, and of the part of these utriculæ which remains uncovered.

The apertures here mentioned are not of the same form in the different plants: in the fritillaria they are always oval, while in the aloe they are square, fig. 13, which I can ascribe only to the form of the exterior utriculæ, which are elongated in the fritillaria and hexagon in the aloe.

The *conjugate* utriculæ are not united to the parenchyme by their lower part, as the exterior utriculæ are; below them is found a vacuity, as seen fig. 7, which communicates on one side with the pore, and on the other with the *utricular interstices*, which I shall speak of in describing the parenchyme. This vacuity is very remarkable in the leaf of the aloe, both on account of its size and its funnel-like appearance.

The *conjugate* utriculæ are filled with a juice which seems to be of the same nature as that of the utriculæ of the parenchyme; in general it is green and sometimes transparent. It often contains a great number of small globules, which render these utriculæ opake: but if the utriculæ be slightly compressed, all these globules unite as several globules of oil would do, and thus communicate to the utriculæ a part of their transparency.

Every time I observed the pores with the microscope, taking care to moisten the pellicle, they always appeared to me to be almost always filled with a black matter, fig. 1 and 14, which is nothing else than a small bubble of air retained there, and which I was enabled to detach by slightly compressing

pressing the pores with a fine needle, which restored to them their transparency. Are these bubbles atmospheric air, or a peculiar air produced by the plant itself? The following experiment may serve as an answer to this question.

I examined different pellicles taken from the surface of the leaves. I successively compressed the pores, and always observed the bubbles of air which issued to decrease gradually in volume, and at length disappear entirely. Whether they remained attached to the utriculæ, or to the point of the needle, or whether they rose to the surface of the water in which these pellicles were immersed, is not the absorption of this air by water peculiar to carbonic acid gas?

Hedwig entertained no doubt that the pores had the faculty of being able to shut themselves, to oppose the introduction of the floating particles disseminated in too great quantity throughout the atmosphere.

Krocker asserts[*] that the contraction of the pores of the leaves of the *amaryllis formosissima* takes place in a very evident manner; which induced me to examine this fact. For this purpose I placed in the focus of the microscope a fragment of the pellicle of one of these leaves, moistening it sufficiently to obviate its desiccation, and I viewed it very attentively without observing any contraction in the pores; but having substituted for it another fragment of pellicle, placed dry on the glass, I then saw the pores gradually contract and close themselves entirely; which I can ascribe only to the shrinking of the *conjugate* utriculæ, produced by the evaporation of their juices: for, having moistened this pellicle, the pores gradually returned to their natural state.

The result of this observation may give us an idea of what the pores of vegetables may experience when exposed to continued drought; and make us comprehend, at the same time, the beneficent influence which rain and abundant dews have on leaves.

We have considered the pores and *conjugate* utriculæ under a general acceptation: let us now speak of an exception which relates to the family of the gramineous plants, and which did not escape the acuteness of Hedwig.

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[*] *De Plantarum Epidermide—Specimen inaugurale, &c. auctore Antonio Krockero. Halæ*, 1800, p. 11. Sæpius apertas (rimas) majores in plantis conspexi, quæ paulo poſt adhibito microſcopio, ipſarum contractionem evidentiſſimè monſtrabant. Obſervatori quamvis rudi, hoc phænomenon in foliorum tenuium epidermide amaryllidis formosissimæ, quam maximè perſpicuum adparet.

The *conjugate* utriculæ of gramineous plants represented in the pellicle of maize, fig. 9, form a narrow elongated body, instead of being nearly spherical as in other plants. It is placed in the middle of a kind of square area, produced by the disposition of the surrounding utriculæ. The *conjugate* utriculæ are applied to each other by their interior faces so exactly, as to make the pore disappear, and to exhibit in its place nothing but a longitudinal line, at the extremities of which is observed a small circle, which Hedwig considered as an aperture, and which, in my opinion, is formed only by the juice contained in the utriculæ, since I was able to make the appearance of this circle vanish by slight compression. Though the form of the *conjugate* utriculæ in the gramineous plants is always such as I have described, I have however remarked, both in the leaves which envelop the ear of maize, or in the interior face of the sheath of the leaves, and also in that of the leaves of the sugar-cane, *conjugate* utriculæ sufficiently reniform to construct the pore, as seen fig. 11. I have even observed some which had a perfect resemblance to the *conjugate* utriculæ of other plants, as represented fig. 12. Such a variety in the form of the *conjugate* utriculæ of the same plant is very remarkable. The details I have given, in regard to the structure of pores, seem to prove that these organs are essential to the greater part of vegetables: I however foresee that it may be objected, that they are not indispensably necessary; since it is said that by blanching a plant may be deprived of its pores. I shall observe in reply, that this assertion is not verified by my observations; for I have found pores on the blanched leaves of an orchis, which had vegetated below a piece of blackened pasteboard, on the blanched leaves of lettuce taken from the heart of the plant, and on the blanched stems of radishes, French beans, and potatoes, which had grown up in a dark cellar.

It may be objected also, that pores exist only eventually in vegetables, since aquatic plants, which never have any, assume them in those parts which are exposed to the air, and that plants which have pores lose them by vegetating in water.

I shall observe in reply, 1st, That if the leaves of the flower of the mesophyllum have pores, while those of the stem are without them, it is a proof that this flower was destined to rise above the surface of the water, and not a consequence of its vegetation in the air; that if the pedicle of the water-ranunculus has pores, while its stem is deprived of them, it is because it ought to live in the air, and not because it has lived there: 2d, That if the leaves of the nenuphar have pores on their superior surface and not on the inferior, it is because the former is intended to exist in the air, and the other in the water.

I shall here add, that the leaves of the nenuphar are provided with pores in the upper surface before they reach the surface of the water; and I have observed that the lower surface of the same leaves had not acquired pores, though they vegetated a long time in the air, in consequence of the desiccation of marshes; which proves that the contact of the air has no influence in the formation of pores which are seen on vegetables destined to be provided with them.

We are told by C. Decandolle, that having caused mint to vegetate under water, it shot forth leaves destitute of pores. To this observation I shall oppose one which I made on the leaves of the narcissus, which after having shot forth in water were furnished with as many pores as if they had vegetated in the open air, as I had reason naturally to expect, since the leaves, still contained in the bulb, exhibited to me their pores already completely formed.

It appears then that the existence of pores does not depend either on the light or the air; but that these organs are co-existent with the other parts of the vegetable.

[To be continued.]

II. *A general View of the Coal Mines worked in France, of their different Products, and the Means of circulating them.* By C. LEFEBVRE, *Member of the Council of Mines, of the Philomatic Society, &c. &c.*

[Continued from vol. xv. p. 345.]

Department of Oise.

NOTHING has hitherto been found in this department but very abundant strata of highly pyritous peat. Such are those in the commune of Beaurain, Guiscart, and Muyraucourt, Fretoy, and several other places in the environs of Noyon. This peat can be considered only as a bad kind of fuel. By subjecting it to proper treatment, to decompose the pyrites, sulphate of iron (green copperas) and even sulphate of alumine (common alum) may be obtained from it. It is susceptible of spontaneous inflammation when exposed to the air in masses. It is much employed for manure, either before or after incineration.

Department

Department of Orme.

No coal mines are worked in this country. A great many indications have been announced, and some of them deserve to be examined, particularly that in the environs of Séez, at Fontaineriaut, which has already been partially worked, and afforded some hopes.

Department of Ourthe.

This country (33) is one of the richest in coals in Europe, and the mines have been worked since the earliest periods. A great many coals are dug up in the neighbourhood of Liege, and even within the precincts of that city the pits are carried to a great depth; and powerful machines are employed to clear these immense cavities of water, and to draw up the coals.

The product of these mines is estimated at 43 millions of myriagrammes. They furnish coals of every kind. The mean price of those of a good quality is ten cents per myriagramme at the pits.

The coal mines in the department of Ourthe ought to supply Batavia; at least in competition with the English mines. If the suppression of the numerous tolls which shackle the navigation of the Meuse is strictly maintained, it will secure this advantage to the country of Liege.

Department of the Pas-de-Calais.

The coal mines of Hardinghen (34), seven leagues north-east of the port of Boulogne, are the principal ones worked in this department. The annual product of the mines in this department amounts to from six to nine millions of myriagrammes. The coals in general are not of so good a quality as those of the departments to the north of Jemmappes; but mixed with a little of these coals they are of excellent use for forges: when delivered at the ports of Boulogne, Gravelines, and Dunkirk, they are sold for eight cents per myriagramme.

Department of the Puy-de-Dome.

The cantons of Montgie, Brassac, Auzat-sur-Allier (35) situated above the Issoire, exhibit several important coal mines, worked for a long time, and particularly those of Salles, Combelle, and Barre. That of Grosmenil, which had been long abandoned on account of its being inundated with water, has been lately resumed by a company who have found means to overcome this difficulty. The products of these coal mines amount to about a million of myriagrammes per annum. The coals cost about 15 cents per myriagramme at the pits. The mean price at Paris is 33 cents.

Departments of the Pyrenées, Upper, Lower, and Eastern.

These three departments have no coal mines worked. Indications have been announced in the department of the Eastern Pyrenées near Prades, and in the environs of Livia, in that of the Lower Pyrenées, at a small distance from Salies; but hitherto they have afforded very little hopes.

Departments of the Upper and Lower Rhine.

The coal mines worked in these two departments (36) are not of much importance. They are, however, valuable for the resources which they afford to the cantons where they are situated. The product of them amounts to about 200,000 myriagrammes per annum.

At Lamperlosch, in the canton of Soultz, in the Lower Rhine, strata of sand containing asphaltum are worked. This bituminous substance is separated, by particular processes, from the earthy matters with which it is united, and employed in commerce. It is applied to the same purposes as pitch, and when mixed with grease is used for daubing over the pivots of machines, axles, &c.

Department of the Rhine and Moselle.

Several researches have been made in this department in the hope of meeting with coals; but it does not appear that any mines have yet been worked in it. Some of the coals it consumes are brought from the departments of La Saarre and La Roër, but the greater part come from the mines situated on the right bank of the Rhine. It is estimated that these mines furnish annually 150,000 myriagrammes.

Department of the Rhone.

Coal mines are known in several places of this department (37), and particularly in that part which borders on the department of La Loire. The annual product, however, of these mines amounts only to 50 or 60,000 myriagrammes per annum.

Department of La Roër.

Very important coal mines are known at Eschweiller (38), Cornelins-Munster, Weisweiller, Bardenberg, and Heyden. The strata of Weisweiller are reserved. The product of the other mines amounts to about 20,000,000 of myriagrammes per annum. The mean price of these coals at the mine is 11 cents per myriagramme.

[To be continued.]

III. *Observations and Experiments on the Light emitted by rotten Wood in the different Kinds of Gas, and in Fluids.* *By* C. W. Böckman, *of Carlsruhe*[*].

Notwithstanding the great number of accurate experiments and of ingenious observations which have been already made known by several eminent philosophers in regard to the light emitted by rotten wood in the different kinds of gas and other mediums, it is still difficult to explain this phænomenon in a satisfactory manner; and this difficulty is increased, because great variations occur both in the observations, and in the consequences deduced from them.

Thus Spallanzani found a perfect analogy between the luminous appearance of rotten wood and that of phosphorus[†]; and conjectures, that by the putrid fermentation of the wood its hydrogen and carbon come more readily into contact with the oxygen of the atmosphere; and that this combination is a slow combustion, which occasions the luminous appearance of the wood. In the non-respirable gases this, according to Spallanzani's opinion, cannot take place for want of oxygen; and he infers that every kind of rotten wood is not luminous, because the necessary quantity of hydrogen and carbon does not always happen to be extricated at the same time.

On the other hand, Mr. Carradori[‡] is of opinion, from other experiments, that rotten wood emits light without this slow combustion taking place, and that the non-respirable gases make on the wood only a transient impression capable of preventing the efflux of light, which, on the contrary, is promoted and increased by the peculiar action of oxygen gas. The observed decrease in the volume of the oxygen gas he considers as not decisive, because this decrease bright light, and even at the end of twenty-four hours I could observe no decrease of any consequence.

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[*] Scherer's *Allgemeines Journal der Chemie*, vol. v. no. 1.

[†] See an Essay on the Phænomena of natural Phosphorus in Atmospheric Air, Oxygen Gas, and other Kinds of Gas, by L. Spallanzani. Gren's Annalen der Physik, vol. i. p. 1.

[‡] See Annalen der Physik, vol. i. p. 2.

The remaining gas, when subjected to examination by means of Fontana's eudiometer, in which I mixed it with the same quantity of nitrous gas, showed a decrease of 30 degrees. A taper immersed in this gas was immediately extinguished. Phosphorus evaporated strongly in it, and it rendered lime water pretty turbid, without a considerable quantity of the gas being absorbed.

Experiment II.

I filled a bell glass with oxygen gas prepared from oxide of manganese well washed with milk of lime. The wood immersed in it did not emit a stronger light than in atmospheric air or water. At the end of forty-eight hours the light seemed to decrease a little, and on the sixth day it was about a third weaker. The phosphorescence afterwards slowly decreased; on the 14th day it ceased entirely, and was not afterwards revived in the open air. The volume of the gas decreased very little, scarcely 0·2. Having put another piece of wood into the remaining gas, it continued to emit light without being weakened.

On trying the gas which remained in an eudiometer, it showed only a decrease of 21 degrees; and in about 15 seconds, when the red vapour of the nitrous acid had disappeared, I observed a faint whitish vapour from the gas floating over the water. For the sake of comparison I tried a portion of the same oxygen gas in a similar bell closed in the same manner with water, but in which no wood had been immersed, and found that in the same eudiometer it showed a diminution of 266 degrees. Artificial phosphorus, when placed in the remaining gas, became luminous and evaporated. A taper immersed in it was immediately extinguished: it was not inflammable, had no considerable smell of mouldiness, and rendered lime water turbid; but it was not absorbed by it in any considerable degree.

Having repeated this experiment several times, I obtained similar results, or results very little different. The phosphorescence of the wood, however, decreased once on the fourth day, and ceased totally on the seventh; though the gas, when subjected to proof, showed a diminution of from 80 to 120 degrees. This difference may have arisen chiefly from a difference in the nature of the wood; for it is not possible to obtain two pieces exactly the same in every respect. On the wood which had emitted light in oxygen gas I observed no mouldiness, nor any perceptible alteration.

other mediums: I hoped also that during these researches I might fall upon some new fact or idea, as is often the case, which might serve to confirm or to throw some new light on either the one or the other of these opinions. As soon therefore as I had procured some phosphorescent wood I began with it a series of experiments, a part of which, with the consequences deduced from them, I shall here lay before the public, after I have made a few previous remarks.

In regard to the wood itself, it was part of the old rotten trunk of a beech-tree, moderately moist, and without any particular mouldy smell. It was not luminous throughout, but emitted light only from its surface to the depth of a few lines. The luminous parts appeared to have lost in a considerable degree their resinous particles. They were friable, full of fibres, and whiter than those parts of the wood which emitted a weaker light, or had no light at all. I preserved the rotten wood in moist filtering paper in a cellar the temperature of which was from 10 to 12 of Reaumur; and in this place I made my observations during the night. The colour of the light was exactly the same as that exhibited by the light of artificial phosphorus in atmospheric air.

I used for my experiments, in general, small bell glasses capable of containing from 8 to 14 cubic inches each, having a neck at the top exactly shut by corks boiled in wax, through which passed a varnished wire. I stuck a piece of phosphorus on the wire in the inside of the bell; filled the vessel, according to the nature of the gas to be employed, either with water or quicksilver; and then placed it on the pneumatic tub. By the pressure which these fluids exercised on the wood, small air bubbles, which must have been contained in the substance of the wood, from time to time, escaped; and therefore before each experiment I took care to immerse the wood in water till no more air ascended, and by these means prevented the gases from being rendered impure.

Experiment I.

I filled a bell with atmospheric air, and preserved it closed by means of water. During the first two days the rotten wood remained luminous; on the third the light was somewhat fainter; on the fourth it had considerably decreased; and on the seventh the light had entirely disappeared. The wood, when taken out and exposed to the atmospheric air, emitted no light either when dry or when moistened with water. I then introduced into the remaining gas a piece of wood which was strongly phosphorescent: it emitted a bright light, and even at the end of twenty-four hours I could observe no decrease of any consequence.

The remaining gas, when subjected to examination by means of Fontana's eudiometer, in which I mixed it with the same quantity of nitrous gas, showed a decrease of 30 degrees. A taper immersed in this gas was immediately extinguished. Phosphorus evaporated strongly in it, and it rendered lime water pretty turbid, without a considerable quantity of the gas being absorbed.

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On trying the gas which remained in an eudiometer, it showed only a decrease of 21 degrees; and in about 15 seconds, when the red vapour of the nitrous acid had disappeared, I observed a faint whitish vapour from the gas floating over the water. For the sake of comparison I tried a portion of the same oxygen gas in a similar bell closed in the same manner with water, but in which no wood had been immersed, and found that in the same eudiometer it showed a diminution of 266 degrees. Artificial phosphorus, when placed in the remaining gas, became luminous and evaporated. A taper immersed in it was immediately extinguished: it was not inflammable, had no considerable smell of mouldiness, and rendered lime water turbid; but it was not absorbed by it in any considerable degree.

Having repeated this experiment several times, I obtained similar results, or results very little different. The phosphorescence of the wood, however, decreased once on the fourth day, and ceased totally on the seventh; though the gas, when subjected to proof, showed a diminution of from 80 to 120 degrees. This difference may have arisen chiefly from a difference in the nature of the wood; for it is not possible to obtain two pieces exactly the same in every respect. On the wood which had emitted light in oxygen gas I observed no mouldiness, nor any perceptible alteration.

It

It did not appear that the want of moisture was a principal cause of the cessation of the phosphorescence; for I found the wood often moist in a greater or less degree, and especially when it came in contact with the water by which the mouth of the bell was closed.

Experiment III.

I filled several bell glasses with azotic gas as pure as possible, which I had separated from atmospheric air by long continued agitation of an amalgam of lead, or by six months action of a solution of alkaline sulphuret, or by moist garden earth, and which tried in an eudiometer mixed with nitrous gas exhibited no diminution. The phosphorescence of the wood in this gas continued at first without any decrease, and as strong as in oxygen gas; but after from one to four hours it became weaker in the different bells: in some it ceased entirely at the end of an hour and a half, in others not till the end of from five to fourteen hours; the cause of which, in all probability, was the unavoidable diversity in the nature of the pieces of wood. After 24 hours I introduced into several of these bells from half a cubic inch to an inch of fresh azotic gas; but in neither of these cases was there the least appearance of light. But having introduced, with proper care, a new piece of wood, it emitted, in these as well as in the other vessels which had received no mixture of new azotic gas, as strong a light as in atmospheric air, and continued undiminished for some time. In some of the bells it was not extinguished till the end of 2, 4, or 5 hours, though no oxygen gas had been introduced.

Experiment IV.

I put into some of the bells along with the rotten wood small bits of phosphorus; and having introduced some of the above azotic gas, the wood and the phosphorus both began to be luminous. At the end of an hour the luminous appearance of the wood was considerably weakened, and it at length decreased so much that its light could no longer be distinguished any more than that of the phosphorus. At the end of 24 hours, when the light of both substances had already ceased for a considerable time, I introduced, with proper caution, a new piece of wood into the gas in which the former still continued luminous. A proof that by this operation no atmospheric air had been introduced was, that the phosphorus remained dark, and I could observe no luminous vapour in the glass. In about an hour and a half the phosphorescence of the wood had for the most part ceased. At the end of 24 hours I therefore introduced another piece of wood, which exhibited the same phænomena as the preceding. This operation was often repeated in the same gas. When the luminous appearance of the wood became weak, it recovered nearly its original splendour, in the course of a few minutes, on placing it in atmospheric air. I tried the remaining gas in the eudiometer, but could observe no decrease; which proves that the gas had remained free from any mixture of oxygen gas.

When a piece of luminous phosphorus is placed near the wood, it is difficult to determine the moment when the light of the latter becomes entirely extinct: for I found that the phosphorus generally remains luminous a considerable time longer than the wood, and even after its light is extinguished a somewhat luminous vapour arises; so that on account of this vapour it is not easy to ascertain when the phosphorescence of the wood ceases. It is equally difficult to observe, whether, on the admission of oxygen gas to azotic gas containing wood and phosphorus, the light of which is extinct, both these substances begin to be luminous at the same time, or not; for at first the gas is entirely illuminated by the luminous vapour; and besides this, the surface of the rotten wood becomes entirely luminous, in consequence of the phosphoric particles deposited on it; and hence it is difficult to determine whether the light proceeds from itself or from these particles. I often found such pieces of wood when taken out entirely penetrated with particles of phosphorus. This observation may be of utility to the future observer.

Experiment V.

I filled a common bell glass with phosphorated azotic gas, in which a considerable quantity of phosphorus had remained several weeks, at the temperature of from 14 to 24° of Reaumur, and in which fresh phosphorus neither evaporated nor became luminous. A piece of wood placed in this gas continued at first luminous, without any decrease of intensity. In about half an hour however its light became weaker, and in an hour entirely disappeared. Next evening I introduced into the same gas a fresh piece of wood, and observed the same phænomena. On introducing more phosphorus it emitted as little light as before.

Experiment VI.

I prepared impure phosphoric azotic gas by combustion and long exposure to heat, with a sufficient quantity of phosphorus shut up in a close vessel with atmospheric air. In this gas a piece of rotten wood continued luminous for an hour. Having introduced another piece of wood, the phosphorescence was the same as before; and at the same time artificial phosphorus emitted no light.

Experiment VII.

Rotten wood appeared phosphorescent in hydrogen gas, prepared from iron and sulphuric acid, in which phosphorus emitted no light; but in the course of 30 or 40 minutes it considerably decreased, and at length became entirely extinct. By the contact of atmospheric air the light was in some measure revived. As often as a fresh piece of rotten wood was introduced into the remaining gas, it became luminous. This experiment I several times repeated with the same result.

Experiment VIII.

Having placed rotten wood in carbonic hydrogen gas, prepared from the saw-dust of the beech tree, it became luminous at first, as in atmospheric air; but after 45 minutes the phosphorescence gradually decreased, and in about an hour entirely ceased. Every time I introduced a fresh piece of wood into the remaining gas I observed the same result. Artificial phosphorus in this gas gave no signs of light whatever.

Experiment IX.

I introduced rotten wood into phosphorated hydrogen gas above a year old, during all which time a considerable piece of phosphorus had remained in it, and which had been continually exposed to the solar heat. In this gas the wood continued luminous without any decrease of its intensity. At the end of an hour the light began to decrease, and in an hour and a half it had almost entirely ceased. Fresh wood introduced into the remaining gas exhibited the same phænomena: artificial phosphorus however gave no signs of light.

Experiment X.

I prepared fresh phosphorated hydrogen gas, which, as is well known, is so unfavourable to the luminous property of phosphorus, even in small quantity, in azotic gas or atmospheric

spheric air: in this gas the rotten wood was exceedingly luminous. Even at the end of an hour and a half I observed no decrease of the light; and it did not cease entirely till the end of several hours. Having introduced a piece of fresh wood, at the end of 24 hours it was as luminous as in atmospheric air, and the case was the same with the 4th and 5th piece which I afterwards brought into contact with it. At the conclusion of this experiment I could easily inflame the single bubbles of gas by means of a burning coal.

Experiment XI.

I introduced into a common bell glass over mercury strong fuming sulphurized hydrogen gas, disengaged from a solution of alkaline sulphuret and tartaric acid. A piece of rotten wood placed in this gas immediately ceased to be phosphorescent. If taken out when the light began to be extinguished, the light could in some measure be revived by washing it with water in atmospheric air. A fresh piece of wood introduced exhibited the same phænomenon. Want of oxygen gas was not, in some cases where this experiment was repeated, the cause of the light being suddenly extinguished; for the gas was not always perfectly pure. Artificial phosphorus would even at times emit in it a faint vapour.

Experiment XII.

In carbonic acid gas prepared with proper care from chalk and sulphuric acid diluted with water, and in which phosphorus, partly by a natural heat and partly by gentle heating in a vessel with hot water, had entirely ceased to be luminous, a piece of rotten wood retained at first its full phosphorescence: the phosphorescence however in the course of four or five minutes considerably decreased, and in 15 or 20 minutes no light was to be seen. Wood which had thus lost all its luminous appearance, when washed with water, seldom recovered its phosphorescence: if however it retained any light when taken out, it was somewhat strengthened in atmospheric air, but after some time greatly decreased, and at length entirely disappeared.

Rotten wood exhibited almost the same phænomena in carbonic acid gas, prepared with great care, but in which no phosphorus had previously been exposed.

Experiment XIII.

Having brought nitrous gas, mixed with about 0·11 parts of azotic gas, into contact with a piece of rotten wood, it at first appeared perfectly luminous: the light however speedily decreased, and in general ceased entirely at the end of from a minute and a half to three minutes: it was seldom renewed by washing the wood with water in atmospheric air. When a fresh piece of wood was introduced into this gas, its phosphorescence and the duration of the light were the same as before.

Experiment XIV.

I brought muriatic acid gas into contact with phosphorescent wood, and observed that in the course of from one minute to a minute and a half its light ceased entirely. This phænomenon took place several times, as often as a new piece of wood was brought into contact with the gas. It appeared to me, in this experiment, that moist wood sooner became dark than dry wood. The phosphorescence could not be revived by the usual means.

Experiment XV.

Rotten wood, placed in ammoniacal gas newly prepared, shone from one and a half to six minutes; and the decrease of the light was pretty speedy. When I took the wood from the gas, I observed that it had a strong smell of ammonia; and after being washed with water it assumed, in a considerable degree, its luminous property. The moister the wood, the more the phosphorescence decreased; and the gas was absorbed by it in the same proportion.

Experiment XVI.

Rotten wood appeared phosphorescent in newly prepared muriatic gas a shorter time than in ammoniacal gas; and when the light became extinguished, I was not able to revive it. A part of the gas was absorbed by the somewhat moist wood.

[To be continued.]

IV. *Of the general Relation between the Specific Gravities and the Strengths and Values of Spirituous Liquors, and the Circumstances by which the former are influenced*[*].

§ 1. All spirituous liquors may, with respect to their strengths, be regarded as compounds of two ingredients, alcohol, or pure spirit of wine, and water; and as differing only from each other in the proportion in which these substances enter into their composition. The former of these fluids, considered as in a state of chemical purity, or altogether unmixed with any heterogeneous substance, is, however, by no means well known. The most highly rectified spirit which has ever yet been procured has probably still contained no inconsiderable quantity of water, which it is the object of the process of rectification to separate; and though we obtain spirit which is more and more dephlegmated as we advance in our knowledge of practical chemistry, yet we have reason to believe that the ultimate point of absolute purity has never yet been attained.

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[*] From Atkins and Coy's Essay on this subject, of which we gave some account in our last volume.

§ 2. The latter of these ingredients being of no value, it follows that every such compound must, *cæteris paribus*, be appreciated by the quantity of the former which it contains; and we could therefore at once estimate its value if we could determine that quantity. It is not, however, absolutely necessary for this appreciation that we should be in possession of the actual quantity of alcohol which is contained in any given liquor: if we can discover the proportion which that quantity bears to the quantity entering into the composition of any other given liquor, we shall be in possession of its relative value, when compared with that of such other.

§ 3. This proportional value, therefore, will be no less truly ascertained, if, instead of considering alcohol as our standard in this respect, we should take spirit of an inferior strength as being so, and appreciate all spirituous compounds by reference to the quantity of such standard spirit which would be capable of producing or being produced by the given compound, by the addition of water to the strongest of the two till they were reduced to the same degree of strength. The real quantity of alcohol, properly so called, which is contained in any mixture, being, from our ignorance of this fluid in a state of chemical purity, impossible to be ascertained, we have naturally been obliged to have recourse to the latter mode of appreciating the values of spirituous liquors by reference to the relative proportions in which this hitherto unknown substance enters into their composition, which are obtained by comparing all of them with some other spirit of known strength as a standard; and this has accordingly become the practice in every country in which these kinds of liquors form an important article of commerce. The strength of this assumed standard is merely arbitrary, it being sufficient for all purposes that it be only certainly and precisely fixed. This, however, has unfortunately not hitherto been the case in this country, and is still less so in any other: the cares of government, in general, afford to those who are occupied by them but little leisure for abstruse research; and the appreciation of quantity and quality in general has hitherto, therefore, been in a great measure left to individuals.

§ 4. Flavour, odour, colour, and consistence, are the objects of our external senses, and the quality of a liquor in these respects is discoverable by their assistance alone; but a minute difference in the strengths of two kinds of spirit, which are otherwise similar, is not so easily detected: there are means of communicating to a liquor an apparently different strength from that which it really possesses, so long as its taste, smell, and appearance, are relied on as criteria by which it is to be estimated.

§ 5. Water and spirit of wine are of very different specific gravities: that of the former being, in a great measure, fixed and invariable at given temperatures, whilst that of the latter is liable to so much uncertainty that it has not hitherto been ascertained what is the real weight of alcohol, properly so called; the thing itself being, as we have before stated, scarcely known. A very few years ago it was conceived that a spirit whose specific gravity was 820* at 60° of Fahrenheit's thermometer was as perfectly free from any admixture of water as it was possible to render it; and yet we are now able with ease to procure it lighter: the specific gravity of the best alcohol from Apothecaries' Hall being very commonly considerably less. In some cases even a much greater degree of dephlegmation has been attained. Mr. Lewis, of Holborn, a very scientific rectifier, has obtained spirit whose specific gravity was less than 811 at 60°; and Dr. Black is said to have procured it so light as 800, or weighing only 4-5ths of the weight of an equal measure of water.

§ 6. When two fluids of different weights are mixed together, we may easily conclude that the specific gravity of the compound will bear some relation to that of its component ingredients; and it appears, therefore, to have been a very obvious idea that the relative proportion of each in such a mixture would be thus to be discovered. In the present improved state of science we see so much further than our predecessors, that, without considering the elevation of the ground on which we stand, we are almost induced to doubt whether their intellectual powers were equal to our own. The want of resource which our ancestors seem to have discovered in their attempts to ascertain the strengths of these liquors, by the shaking them in a phial, firing them over gunpowder, and a thousand other still more fallacious means, when the object which they had in view was so much more accurately and easily attainable by the simple operation of weighing them, really appears, at first sight, somewhat remarkable. The consideration of all the details relative to this subject involves a number of intricate points; yet the merely ascertaining the weight of a given measure of any liquor by a common pair of scales would doubtlessly have afforded a better indication with regard to its strength than any of the other modes of estimating it which are unconnected with the consideration of this property. It is now, however, sufficiently agreed upon, that the best method of ascertaining the relative values of spirituous liquors, with regard to their difference of strength, is by means of their specific gravities; and the principles of this method, therefore, will form the subject of the present tract.

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* It has been most usual with writers on specific gravities to consider that of distilled water as unity: we, however, have found it more conducive to the facility of denominating them, to consider it as 1000, and which is accordingly done throughout this tract. "Eight hundred and twenty," for example, is more easily expressed in words than ·820.

§ 7. If a given bulk or measure of water and alcohol remained unchanged in every temperature, and whether these two fluids were mixed or separate, the ascertaining the real specific gravity of the latter itself, or the estimation of the quantity of each in any compound of the two, would be a matter of no difficulty: the simple rules of alligation would give all that could be required in this respect. This, however, is not the case: a variation of temperature, or the mixture of two spirituous compounds of different strengths, or of any such liquor with water, occasions a change in the aggregate bulk of the whole which is necessary to be taken into consideration; and it will therefore be requisite to treat separately of the effect of each of these operating causes.

§ 8. It has long been known that all bodies in general, whether solid or fluid, expand by heat and contract by cold; and it follows that the same quantity of any fluid which, when at an elevated temperature, would fill a measure of given dimensions, must fall short of doing so if the temperature should be lowered; and, *e converso*, the same quantity which would be contained in the measure when cold would when heated be more than sufficient to fill it, though the absolute weight of the whole of the fluid would still continue unchanged. In speaking, therefore, of a measure of any liquid, we indicate nothing with respect to its real quantity, unless we at the same time express the temperature at which it is to be measured.

§ 9. If an equal change took place in the bulks of alcohol, and of water, and of every compound of them, by the same elevation or depression of temperature, the application of the necessary correction for this circumstance would be still easy. The difference in this respect is, however, very considerable: water increases only about 1-300dth of its bulk by an elevation of its temperature from 40° to 80° of Fahrenheit's thermometer, whilst alcohol would, by a similar change of temperature, increase in measure no less than 1-43d of the whole, or seven times as much as the other; and liquors of intermediate strengths would be affected in some intermediate degree. To estimate, therefore, the difference in the measure produced by this cause, we must know the strength of the liquor; whilst this, on the other hand, is only to be determined with reference to the former.

§ 10. The effect of which we are next to speak is of a still more curious nature. When two fluids which are capable of chemical combination are mixed together, it generally happens that either heat or cold is produced, the temperature of the compound differing from the mean temperature of its ingredients. The former is most commonly the case; and, when so, it happens in the greater number of instances, and perhaps in all, that a diminution of the bulk or measure of the compound also takes place, which is proportional to the heat so produced: probably in consequence of this separation of caloric.

If 18 gallons of water be mixed with the like quantity of the strongest spirit of wine, the mixture will become considerably warmer, and we shall only get about 35 gallons of the diluted spirit instead of 36: and this kind of effect is also produced in less degree by the addition of water to any weaker spirit, or the mixture of two such liquors of different strengths; the resulting compound being always found to occupy less space than the substances forming it did when separate; and its specific gravity being therefore greater than would be inferred by mere arithmetical calculation from those of its ingredients. This "concentration," as it is very properly called, must of course be considered in the estimation of the strength from the specific gravity of a liquor; and the consideration of it is attended with the same difficulty, as has already been mentioned in the last section, with respect to the effect produced by change of temperature.

§ 11. The uniformity of the relation between the specific gravities and the strengths of spirituous liquors depends on a supposition that they are either altogether composed of alcohol and water, or at least in such a state of purity as to be free from every adulteration which can materially change the specific gravity, whilst the quantity of the former continues the same; for, unless this be the case, we shall of course be unable to deduce their strength from their weight. The substances likely to be found in spirituous liquors, where no fraud is suspected, are, essential oils, sometimes empyreumatic, mucilaginous or extractive matter, and perhaps some saccharine matter. The effect of these, with the exception of the latter, is perhaps scarcely such in the course of trade as to be worth the cognizance of the excise, nor could it easily be reduced to any certain rules. Essential and empyreumatic oils are nearly of the same specific gravity as spirit, or generally rather lighter; and therefore, notwithstanding the mutual penetration, will probably make but little change in the specific gravity of any spirituous liquor in which they are dissolved. The other substances are all heavier than spirit; the specific gravity of common gum being 1482, and of sugar 1606, according to the tables of M. Brisson. The effects of them, therefore, will be to make spirituous liquors appear less strong than they really are. With a view of determining this matter, Dr. Dollfuss evaporated 1000 grains of brandy, and the same quantity of rum, to dryness: the former left a residuum of 40 grains, the latter only of 8½ grains. The 40 grains of residuum from the brandy, dissolved again in a mixture of 100 of spirit with 50 of water, increased its specific gravity ·00041: hence the effect of this extraneous matter upon the specific gravity of the brandy containing it, would be to increase the fifth figure by six nearly, which is about equal to the effect which would be produced in the abovementioned mixture, by the addition of a pint of water to 100 gallons of the spirit; a quantity much too minute for the attention of government. It appears, indeed, somewhat remarkable, both that so large a proportion of residuum should have been left, and that upwards of 1-4th by weight of this extractive matter should not have occasioned a greater difference than 1-2000th part in the specific gravity of this diluted spirit. Saccharine matter operates much more powerfully in this respect. If a quantity of sugar be dissolved in proof spirit, it will become very considerably increased in its weight, and consequently diminished in its apparent strength and value. There will perhaps, however, be but little danger of fraud of this nature, if the purchasers of spirituous liquors are to use the same means of ascertaining their strengths as the officers of the revenue; since that which would apparently diminish them to the one, would also lessen their value in the estimation of the other. The detection of it, if suspected, would of course be by evaporation.

§ 12. We are rather disposed to believe, that if fraud does in fact exist with regard to the adulteration of spirituous liquors, for the purpose of changing their apparent strengths, it is more frequently of a contrary description. It is well known that when alcohol is distilled with any of those acids which retain their oxygen least powerfully, the former is converted into æther; a very different fluid, which is in a great measure immiscible with either water or spirit, and which, being much lighter than either, will float on the surface if attempted to be combined with them. This effect, in a certain degree, appears not unfrequently to take place with respect to spirituous liquors. It is very common to use the mineral acids, particularly the sulphuric, in various processes relative to spirituous liquors, either to give them a "vinous flavour," as it is called, or for neutralizing an alkali with which they have previously become charged in their rectification: and the experiments of the authors of this tract have shown them that it often happens in this case that the superior portion of the product, after it has stood for some time, is considerably the lightest; being doubtlessly composed, either wholly or in part, of a kind of semi-ætherial liquor, or dulcified spirit, which does not intimately combine with the other portions of it. It appears to be a fact well known in the trade, that there are some kinds of spirit which will not bear dilution; that is, which if mixed with water produce compounds which are by no means of such a degree of strength as would have been inferred from the apparent strength of the liquor before such mixture: a circumstance which must proceed from the same cause. The mode of detecting this kind of adulteration, or of counteracting its effect, is by taking the sample for the proof from the lowest part of the vessel in which it is contained.

§ 13. Having thus generally treated of those operating causes which are capable of influencing the weight of spirituous liquors, and which must, therefore, be separately considered when it is required to deduce their strengths from an examination of their specific gravities, we shall proceed to speak more particularly of the modes of comparison hitherto used, and conclude with giving some rules for the adaptation of the valuable tables of Mr. Gilpin to the present standard, together with two short tables of our own, by which the comparative strength and value of any spirit may be found, when its specific gravity and temperature are given.

[To be continued.]

V. *On the Quantity of Iron in Cotton and Linen Cloth: Evil Effects, simple Means of eradicating, &c.: and Observations on Bleaching, the Result of long Experience. By* NICHOLAS GRIMSHAW, *Esq. Member of the Dublin Society**.

EVERY one who has attended to the process of bleaching, must have observed a buff hue in the cloth after it had been spread on the field, or immersed in oxygenated muriatic acid (bleaching liquor).

Having thought much on the subject, tried experiments innumerable, and being of opinion that iron† was the cause, I, on the 22d of September last, cut a yard of calico off one of the pieces then in the first bleaching-liquor, which had been previously boiled in potash, which piece (as well as many of the 100 then in the same kieve) was a deep buff. The yard of calico was put into a hot solution of sumach (astringent), and it soon became a deep drab colour: it was then dyed in madder, which brought it to a deep purple; and on bleaching it for a fortnight, I found the colour as permanent as if a solution of iron (iron-liquor), or sulphate of iron (copperas), had been used as a mordant, but more muddy.

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* From the *Transactions of the Dublin Society*, vol. i. part 2.—That iron is very generally diffused throughout the globe, being frequently found mixed with sand, clay, chalk, and in the ashes of vegetables, and even in the blood of animals, in such abundance, that ſome authors have attributed both the colours of vegetables, and of the vital fluid itſelf, to the iron contained in them, is no new discovery: but, that the bad colour of linen and cotton cloth is owing to the evil effects of the iron contained in the vegetables, was first thought of by the author of the following essay, which is a strong proof of the great utility of chemical knowledge in bringing manufactures to perfection.

† Since my arrival in Dublin, Mr. Higgins, professor of chemistry, &c. accompanied me to a neighbouring bleach green: we cut a bit of calico off a piece then in the first bleaching liquor, and which had had one boil in potash; it was buff, and on trying it with marine acid and prussiate of potash it became blue: a convincing proof that iron was present. The following experiments also were made in the presence of Mr. Higgins, and his very respectable operating pupils, in the elaboratory of the Dublin Society. Six ounces of unbleached calico was burned to tinder in a crucible; digested it with marine acid, and filtered it; poured prussiate of potash on the solution, and found a copious precipitation of prussiate of iron; filtered the liquor; having previously weighed the filter (45 grains), and found when dry it had retained eight grains of the prussiate of iron.

Six ounces of linen, treated in the same manner, produced six grains of prussiate of iron.

I was then satisfied that a considerable quantity of iron was present, not only in the raw material of cotton, but in consequence of some weavers making or allowing their dressing to remain in iron pots, which they generally call sowings in Ireland, and sowlings in Lancashire.

The dressing is nearly the same in both countries, as it consists of the farina (flour) of wheat, oats, or potatoes, and, when sour, is capable of dissolving iron. This is well known from iron liquor used by calico printers being made with vegetable acid, in which they put iron hoops. This iron liquor is the mordant for black and purple, and is dyed in logwood or madder; and when, in consequence of accident in printing, it is necessary to discharge the black or purple colour before they are dyed, if recourse is had to oxygenated muriatic acid, an iron mould is produced, that is, the colour becomes buff or gold; but use sulphuric or marine acid, and the colour is discharged. I have mentioned this circumstance for the purpose of showing why some pieces assume a much deeper buff than others, when put into the oxygenated muriatic acid, as the quantity of iron put on the warp by the weavers will depend on their using or not using iron pots, the strength of the acid of their dressing, the rust or cleanness of their pot, and consequently the quantity of iron it holds in solution.

The experiment however, before mentioned, induced me to take 100 pieces which had been once boiled in potash and washed, and immerse them in sulphuric acid (bleachers sours), and after remaining about 12 hours they were washed, and again boiled in potash.

After being washed and without being put on the bleach-green, they were put into the oxygenated muriatic acid for the usual time (about 12 hours), and came out perfectly free from any buff appearance; which convinced me, as it must every one who knows its use, that the sulphuric acid had divested them of iron, and consequently had left none to be oxidated by the oxygenated muriatic acid, or the oxygen of the atmosphere. They were again boiled and immersed in the bleaching liquor alternately, until they had six boils, and five bleaching liquors; and on the seventh day, without being put on the grass, they were the whitest and strongest pieces I had ever seen: and during the process appeared uncommonly clear; for after the fourth boil they were sufficiently white for every purpose except printing. More than 5000 pieces have been bleached at my works (at White House, near Belfast) in the same manner, without any appearance whatever of buff, as before mentioned: only half the usual quantity of oxygenated muriatic acid has been used; the receiver affording sufficient for seven to eight parcels of calicoes, which before was only equal to three or four—So much less was required to bring the liquor in the kieve to its usual strength, (which was ascertained by the test of solution of indigo in sulphuric acid,) and so little had the liquor been exhausted by oxidating the iron in the cloth, &c. in lieu of acting on the colouring part.

It has been long known, and as long lamented by the calico printers, that yellow stains have appeared after the printed pieces have been dyed, and that such stains cannot be bleached out without reducing and materially injuring the colours. Prior to the commencement of the process I have described (I mean, the use of sours previous to the cloth being exposed to the oxygen of the atmosphere, by being spread on the field, or immersed in the oxygenated muriatic acid) a considerable quantity of calicoes had those stains; but I am happy to state, that on dyeing in the same parcels, and the same coppers, pieces of the old process, and pieces of the new, the latter were perfectly clear, and the first stained.

This defect continued as long as any of the old bleach remained, but with them stains entirely disappeared. The new bleach were perfectly clear, by lying two or three days on the grass, after dyeing: the old not in as many weeks.

It is unnecessary to say to men of practice and science, that the same process of bleaching is applicable to linen and to cotton, linen requiring only a stronger process and more time, or that iron oxidated and iron moulds are the same; and that the quality of cloth is in consequence injured, as they are generated by combustion in consequence of the union of oxygen gas: hence the quantity of bucks (rotten linens) and linens also resuming a yellow hue after being bleached and exposed to the air, which acts on the iron remaining in the cloth.

The eye of the scientific mind will discover at the first glance, that a solution of sulphuric acid will take up or dissolve iron in the cloth, and render it miscible, or soluble in water, if applied previous to its becoming an oxide.

Let us then look at what the bleacher has to contend with and to eradicate: and it will be found he has the resin or colouring matter of the flax or cotton, iron, and the weaver's dressing as before mentioned; to which may be added a little butter or tallow. An addition of the last article takes place by the dropping of the candles during the winter.

The first object then is to divest the cloth of the sowings. This is done by steeping in water, summer heat (76° of Fahrenheit), and washing.

The second is to dissolve the grease applied by the weaver, and some of the resinous part. This is done by a boil or buck in alkali, and washing.

The third is to divest the cloth of iron; which is most effectually done by steeping in sours for 10 or 12 hours, and washing immediately.

After another boil, the bleacher may proceed as before mentioned; or proceed in any other manner his experience or opinion may direct—as he will derive all the advantages stated to result from the use of sours previous to exposure to air or bleaching liquor: let his after-process be any one of those now in use.

But I by all means advise him to throw his cloth loose into the steeping-kieve, furnace, sours, and bleaching liquor, during the early part of the process, so that the cloth may be equally acted upon; by which means he will avoid those dark clouds which must always appear when the cloth has been kept in the band during the above processes.

Those who use the rope-net and crane (and every one ought to use them) will find no additional trouble, and will be much pleased with the evenness of the pieces.

Let the first sour be strong, and wash well: the other sours may be continued in the usual stages of bleaching, using one less in consequence of the previous sour*.

The strength of the boils should be proportioned to the quantity of resin or colouring matter of the cloth; consequently the first should be the strongest, as the resin or colouring matter of the cloth is capable of saturating a greater quantity of alkali than it is afterwards; and it should be recollected that the cloth possesses, in the first instance, the grease of the weaver's dressing, which, by uniting with the alkali, becomes saponaceous (soapy), and is in consequence easily washed off.

The alkaline hydrometer should by all means be used by the bleachers to ascertain the strength of their leys: they will in a moment see the number of ounces of alkali to a gallon, and can by a simple table† see how many gallons are necessary, so as to give them a certain weight to each piece, and which should always be proportioned to the thickness of the cloth, and, as before mentioned, to the quantity of colouring matter to be dissolved.

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* Muriatic is preferable to sulphureous acid, especially for the after-sours, as it possesses the power of dissolving oxides of iron (iron moulds).

† See the table, p. 39.

Those who use the bleaching liquor, either early or late in the process, should immerse or steep the cloth in it after it had been boiled and washed, but never to put the cloth from the field into it, as they will find, as I have done, that the quality will be destroyed.

I have likewise found that cloth, either linen or cotton, neither stiffens with the callender or beetling, if dried after being only washed out of the bleaching liquor; it must therefore be boiled in water or ashes: if the latter, it ought to be put on the grass for a few days previous to finishing; and I advise the linen bleachers to give at least one boil in ashes after the last bleaching liquor, not only to prevent the cloth being soft or slack after finishing, but as the best means of preventing mildews (so much complained of) by the attraction of moisture.

If any further proof was necessary to confirm what has been before stated relative to the utility of early souring, it will every day be found in the process of dipping or dyeing china blue, which is performed by alternate immersion in a solution of lime or ashes and sulphate of iron (copperas), by which the iron is precipitated on the cloth; it is easily removed by immediate souring: but if the cloth is exposed for some time to the air, the iron cannot be dissolved without injury to the texture or fibres, and the colour.

I therefore entreat the bleachers of linen, calico, or muslin, will try the effects of souring after the first boil, in the manner before mentioned; and after the second boil that they will pursue such process as they have most improved on (either bleaching on the field entirely, or the alternate process of bleaching on the field after a few boils, and then using the bleaching liquor with lime or ashes, in their receivers): this will most effectually enable them to compare the relative progress, &c., and which will be very obvious in favour of early souring.

In a conversation a few days ago with Mr. Whiteman, of Lisburn, on the subject of bleaching, he told me that his muslins had the buff appearance I had described, not only in the process of bleaching, but that he had then some unsaleable, even at a loss of 10 per cent., in consequence of that buff or muddy appearance, a specimen of which he gave me, and in which a considerable quantity of prussiate of iron has been found.

The muslins, calicoes, and linens, bought in the white for printing at my works, are generally stained and bad whites after printing and dyeing. Those bleached after the use of sours, as described, are always free from stains, and very clear whites.

I am very happy to have it in my power to add the following very respectable scientific authorities in favour of my practice, &c. to whom I communicated the result, viz. —Wm. Higgins, Esq. M. R. I. A. Professor of Chemistry, &c. Richard Kirwan, Esq. F. R. S. M. R. I. A. &c. and to James M'Donald, Esq. M. D. &c. Belfast, who has attended the process at my works, and whose Report is before the Linen Board.

On Soap, substituted for Bran in Bleaching. By the same.

The legislatures of Great Britain and Ireland have adopted wise and salutary measures for increasing the quantity of food, by offering bounties on importation; they have likewise lessened its consumption by stopping the distilleries. It becomes then the indispensable duty of all, especially in periods such as the present, to avoid the smallest waste or misapplication of food. Bran, if given to horses, saves corn; if to horned cattle, it produces milk, butter, or beef; but when used by calico printers, it is worse than throwing it away; for in most cases it is not only unnecessary and expensive, but injurious. There is still a stronger reason for avoiding the waste of it, on which at present it is better to be silent. Prejudice, therefore, will give place to public good, and the calico printers may rely on the following process, as it has been practised for more than a year by a house of long experience and proficiency in that business.

Let six or seven pounds of black or soft soap be well dissolved in hot water; pour about two-thirds of the above into a copper of hot water (180 to 190° of Fahrenheit): when the calicoes or muslins which have been dyed in madder are washed as for branning, give them five or six ends over the wince, taking only three pieces of calico or six of muslin at a time, that they may be even. Renew your copper with the remainder of the soap, and through one copper may be done twenty-seven or thirty calicoes, and about double the number of muslins. Let them be washed as after bran, and pinned on the grass: if the cloth has been well bleached, muslins will be white, on an average, in two days, calicoes in four. The colours will have a much finer hue than by the use of bran.

N. B. Soap has been used for taking out stains, but by branning before soaping the stain has been more fixed: therefore bran should not be used.

A Table

A TABLE ascertaining the Quantity of Ashes to be used, according to the Hydrometer, for 100 Pieces of Cloth.

Oz. marked by the hydrometer.	Gallons to be used for a 6 oz. boil.	Gallons to be used for a 5 oz. boil.	Gallons to be used for a 4 oz. boil.	Gallons to be used for a 3 oz. boil.	Gallons to be used for a 2 oz. boil.
8	75	62½	50	37½	25
8½	70¼	58¾	47	35¼	23½
9	66¼	55¾	44¾	33	22¼
9½	63	52¾	42¼	31½	21
10	60	50	40	30	20
10½	57	47½	38	28½	19
11	54¼	45	36	27	18
11½	52½	43	34½	26¼	17¼
12	50	41¼	33	25	16½
12½	48	40	32	24	16
13	46	38	30½	22¾	15¼
13½	44¼	36½	29¾	21¾	14¾
14	42¾	35	28¼	21	14
14½	41¾	34¼	27½	20½	13¾
15	40	32¾	26¼	20¼	13
15½	39	32	25¾	19½	12¾
16	37½	31¼	25	18¾	12½
16½	36⅓	30¼	24¼	18¼	12
17	35¾	29	23¾	17½	11½
17½	34⅓	28½	22¾	17	11¼
18	33⅓	27¾	22¼	16½	11
18½	32¼	26¾	21½	16	10¾
19	31½	26¼	21	15¾	10½
19½	30¾	25½	20½	15¼	10¼
20	30	25	20	15	10
20½	29¼	24¼	19½	14½	9¾
21	28¼	23¾	19	14¼	9½
21½	27¾	23	18½	13¾	9¼
22	27¼	22½	18¼	13½	9
22½	26¾	21¾	17¾	13¼	8¾
23	26	21¼	17¼	13	8½
23½	25½	21	17¼	12¾	8½
24	25	20½	16½	12½	8¼
24½	24½	20¼	16	12¼	8
25	24	20	16	12	8
25½	23¾	19¾	15¾	11¾	7¾
26	23	18¾	15½	11½	7¾
26½	22¾	18½	15¼	11¼	7½
27	22	18	15	11	7½
27½	21¾	17¾	14½	10¾	7¼
28	21½	17¾	14¼	10¼	7
28½	21	17½	14	10½	7
29	20½	17	13¾	10¼	6¾
29½	20¼	16¾	13½	10	6¾
30	20	16½	13¼	10	6½
30½	19½	16¼	13	9¾	6½
31	19¼	15¾	12¾	9½	6¼
31½	19	15½	12½	9¼	6⅓
32	18¾	15½	12½	9½	6¼

VI. *Of the Herring Fishery. Translated from an Essay in Dutch, entitled* "Beschryving van de Haringvisscherye*."

The herring inhabits the Northern seas, and probably amidst the great Ice islands, spawns in the months of August and September, and multiplies so astonishingly, that notwithstanding the great destruction of them by the fish of prey and men, the species is not sensibly diminished. The herring belongs to that class which emigrate. They make their appearance yearly in prodigious numbers. The great shoal, in its progress from the North, divides into two principal branches; the right wing goes westward, falls towards the coasts of Iceland in the month of March; the left inclines to the eastward. These two grand divisions are afterwards split into several subdivisions: some bend their course towards Newfoundland, others towards the coasts of Norway, and partly fall into the Baltic through the Sound; while another part turns the north point of Shetland, where it stretches along the coast, until it joins the division (through the Belt) which entered the Baltic. They separate again, to cover the coasts of Holstein, the Texel, the Zuiderzee, &c.

The westerly column, or right wing, which is also the greater, goes on straight forward towards the Orcades (where the Dutch fishers impatiently lie in wait for them), and from thence to Scotland, where they again separate, one wing steering by the coast of Holland, England, and France, the other taking the route of Ireland. After passing all these islands, they again meet, and form into a column, which stretches along the Atlantic ocean and disappears. But what justly challenges our admiration is, that after separating into so many different branches, they know how to rally their scattered squadrons, and find the way back to their native abode. The time of meeting, and the place of rendezvous, are settled, so that after the general retreat not one straggler is to be met with in these seas.

How regular soever the period of yearly emigration appears, it is not free from anomalies. It may appear surprising that these animals, who are secured from the persecution of their enemies, in the unfathomable depths of the Northern ocean, by an impregnable rampart of ice, should forsake their safe retreat yearly, in myriads, exposed on our coasts to great and unavoidable havoc. Is it not a striking instance of the goodness of divine providence, to draw this prodigious swarm of useful fish into our nets? This explanation, however pious, affords little light to the natural historian, who may be inclined to ask: Since divine providence is so gracious in this respect, why does it not send us a troop of whales to furnish us with train oil at less labour and cost than we can now procure it? The celebrated burgomaster of Hamburg, Mr. Anderson, is of opinion, that the emigration of herrings is owing to the overflow of young ones, who, not finding sufficient room in their old habitations, sally out in quest of new settlements, as so many colonies. This opinion does not at all account for the phænomenon. How can periodical emigrations, always taking place at stated seasons, proceed from so uncertain a cause as the mere accident of an overflowing population?

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* From the *Transactions of the Dublin Society*, vol. i. part 2.

How will this hypothesis account for the constant adherence to the same track, the separation and reunion of the main body, at stated places and at stated times? Whereas mere want of room would drive them indiscriminately towards any or all places. 2dly, So far from being the effect of excessive multiplication, it seems to be the motive of it. They are not the only species which undergoes long voyages for the sake of propagation. Many birds of passage, such as the woodcock, wild-goose, &c. do the same. In fact, the herrings multiply more on the route than they do before it; we know that many kinds of fish leave the sea, and seek the rivers to deposit their spawn: so that one very probable cause of the emigration is, the instinct of emigration; the second, the plenty of food which the quickening influence of spring prepares, by the swarms of insects and flies. The king or leader of herrings is much larger than the common, being two feet long; fishers think it criminal to destroy it; the whole column follows and observes his motions. In a streight they contract, in an open sea they expand their order of march with admirable dexterity, without slackening their pace. They live on small fishes and young crabs, as appears from their jaws being furnished with teeth. Lewenhoeck found in the stomach of a herring the indigested remains of a little fish.

To form some conception of the innumerable multitude of herrings that fill the seas, extending more than the breadth of England and Ireland together, and in order to give a clear insight into this profitable branch of trade, we shall first treat of the ships and implements used in the fishery: 2dly, of the time and manner of taking them: 3dly, of the regulations and right of carrying on the fishery, the gutting, and Ireland, being of an inferior quality, are less fit for preservation or salting; for which reason it is forbidden by an ordinance, dated 1620, to take any herrings at the forementioned places. Among the regulations that have been made for the support of the herring fishery, the following are the principal ones.

The appointment of a hearmeester, or overseer, at all the landing places where herrings are brought in, to take strict care that the herrings should get a second salting and packing: to him is also intrusted the inspection of the salt and coopers' work, or barrels in which they are packed. Their province is in a special manner to prevent foreign herrings being mixed with our own (in case any foreign cargoes arrive), and to have the barrel branded with such marks as may prevent a mistake on this head, that our trade may not suffer from the quality of such fish.

2dly.—He is to take care that all damaged herrings or of bad quality, e. g. such as take sick after having cast their fry, or on the eve of doing it, in a word, unmerchantable, be thrown aside in the packing, lest such bad fish corrupt the sound, or give them a bad flavour. That, moreover, the fish be properly salted and packed.—3dly. That the masters and crew of one buss do not put any hindrance in the way of another. In case they were unlucky, or could not succeed where they had cast nets, they must not remove to the ground of others to disturb their operations, nor damage their nets, boats, or other implements: in case they do, they must make good the damage. No buss employed in the fishery can be sold to foreigners, or hired out to them for the purposes of fishing. 4thly.—That the said overseer do inspect all the barrels before they are taken on board the busses, reject such as shall appear unfit, and mark such as he approves with the name of the cooper, and his place of residence; after having examined the quality of the timber, construction, hoops, &c. &c. 5thly.—He shall not suffer any buss out on the fishery before the 24th of June, and he shall require a declaration upon oath, before any of the herrings are landed, that there are none abroad taken before that period.

6thly.—He must take care that in salting herrings a distinction should be made. Thus, for example, the herrings taken between St. John's and St. James's day shall be salted with coarse and chosen salt. Those taken from St. James's day to the 14th of September must be packed up with the best fine salt. No herrings can be packed except such as are taken from the 14th of July to the 1st of January.

In 1774, the number of vessels sent out amounted to 165, and in 1776 to 176. From this brief exposition of the number of vessels employed in the fishery, it appears that a great decline has taken place since the year 1601, when 1500 vessels were sent out yearly from the United Provinces. The cause of this decline seems to be, the greater attention which other nations pay to this branch of fishery. It remains, however, certain, that the Dutch method of curing is superior to that of their neighbours. The diminution of trade was necessarily accompanied by a proportional diminution of national profit; formerly thousands were supported by this branch who are now out of employment. Government omitted nothing that could revive this declining branch of trade. Thus, on the 19th of May 1775 the government offered a premium of five hundred florins for any ship which should be employed two years successively in the herring fishery, and for the second two years four hundred florins: yet some abuses happened in consequence; for some, having got the premiums, discontinued to send out their vessels. As the selling prices of herrings vary with the season and plenty, it is scarce possible to ascertain with exactness the profits of one voyage. We shall, however, attempt an average account, from which it will appear that the herring fishery is worthy of every encouragement, as a branch of industry highly advantageous to the community. A buss of thirty-two lasts, fitted out for three voyages, costs, as we already observed, between sixteen and seventeen thousand florins; each last of herrings contains twelve tons, and each ton about eight hundred herrings: the whole lading then of the buss amounts to about 380 tons; and if each herring be estimated at half a stiver (i. e. twenty florins the ton), the amount will be 7600 florins, which multiplied by three voyages yields a sum total of 22,800; from which 17000 florins being deducted for prime cost, a surplus remains of 5800 florins for each buss, all net profit:

Which multiplied by 160, the number of busses yearly sent out to the fishery, yields little short of a million of florins.

Let us in the second place examine, with what instruments, and after what manner, this amazing quantity of herrings is taken as soon as the busses are all assembled. They take their course from Shetland, N. N. E. and cast the nets off Fairhill on St. John's night, the 25th of June, after midnight. The fishery is always carried on at night, as well to obviate the inconvenience of the fish discovering the nets, as also to entice them by the light of the lanterns, of which they are fond, and towards which they make. Mr. Spon, in his Travels through Italy, makes mention of a similar artifice used by the fishers on the coast of Dalmatia; they carry lanterns in order to entice the sardines, which are there in great abundance.

The nets are very long, consisting of fifty or sixty webs, fortified with narrow net meshes, in order that the herring may be entangled by his gills. The mouth of the net may be fortified with good hemp, or strong Persian silk, as being more durable than hemp, being capable of holding good three years: moreover they are tanned, or coloured brown with smoke, that they may be the less perceptible by the fish. The nets are thrown out at sun-set, betwixt two busses, which, on account of their size, require much time and labour. They are fastened, and buoyed up with casks to prevent sinking, which serve as a distinguishing mark, and also, by reason of their weight, instead of an anchor. To prevent them from getting entangled in one another, the busses should be so arranged that each may preserve its nets free. During night the fishes run into the nets spread out for them; and about five or six in the morning they are hauled up. This labour will give full employment for three hours, as the take is commonly from three to ten lasts, and sometimes even fourteen lasts. The day is taken up in dressing the fish: they begin the operation by cutting out the gills, as those parts are liable to speedy putrefaction: they are then put into barrels and salted: all that are taken under five lasts are thus prepared for the market. The overplus, which they call *stabers*, are also gilled, lightly salted, and thrown into boats to be sent ashore after the first salting. The herrings are left on deck throughout the following night. On the second morning they are properly packed, and the barrels, being filled up, are placed in the hold. In the beginning a part of the take is sent ashore in the lighters called jaagers. When the herrings are fully cured, the buss herself goes towards land, discharges her freight, and lays in provision for a second expedition. As experience has taught, that herrings caught at certain seasons keep longest and are best for use, the time for beginning the fishery is fixed by law. Before the 25th of June, all masters of fishing smacks, previous to their departure from Holland, are bound over not to trespass this ordinance, and at their return are obliged to declare upon oath, that they have not, by their own deeds or by the ministry of others, contravened the law. Testimonials of this are given to each ship, marked with the place of destination, to the end that none be deceived, or the trade suffer by improper goods. The place of the fishery changes with the seasons. From St. John's to St. James's, i. e. from the 24th of June to the 25th of July, the seat of the fishery is between Fairhill and Shetland; during this period herrings of the best quality are taken: from St. James's day to the 14th of September, the pursuit is carried on to the north of Scotland, and from thence to the 25th of November, along the coasts of Yarmouth and Norfolk. All the herrings taken the first three weeks after the 25th of June are cured and packed up together, unsorted, and sent to Holland by swift-sailing vessels called jaagers, after which all the herrings taken are carefully sorted and separated into three divisions: maatijes herrings, full, and shotten herrings, which are all separately cured and packed up in distinct barrels.

In the maatijes herrings is found neither roe nor milt. They are very fat and palatable, but do not keep well. Full herrings are those that are full of milt or roe, and in their most perfect state: this sort is fittest for market and preservation. The third sort consists of such as having cast their spawn or roe are spent, or are on the point of spending: this last sort is inferior in quality, and cannot be kept so long as the former, or full herrings.

The lading of the busses on their return to Holland consists of those three sorts, which are again inspected, packed, and salted afresh, before they are sent to any foreign market. By this fresh packing fourteen sea barrels are reduced to twelve, which make up a last. In order to bring this branch of commerce to a flourishing state, the governments of this and many other countries have made sundry regulations concerning the manner of cutting out the gills, salting, and packing.

The English have always looked on the commerce of Holland with an eye of envy, which often bursts out into open acts of violence, never omitting any opportunity of disturbing, and, if possible, of ruining our herring fisheries; the more so as Dutch herrings have always had the preference of the English, as well as of those of every other nation. In order to cut off all pretext of quarrel, our fishers are forbidden to cast their nets within ten miles of the English shore; which prohibition is the less detrimental to the fishery, as herrings of the best quality are taken at such a distance from the shore.

Those which come into the bays of Norway, Shetland,

and Ireland, being of an inferior quality, are less fit for preservation or salting; for which reason it is forbidden by an ordinance, dated 1620, to take any herrings at the fore-mentioned places. Among the regulations that have been made for the support of the herring fishery, the following are the principal ones.

The appointment of a hearmeester, or overseer, at all the landing places where herrings are brought in, to take strict care that the herrings should get a second salting and packing: to him is also intrusted the inspection of the salt and coopers' work, or barrels in which they are packed. Their province is in a special manner to prevent foreign herrings being mixed with our own (in case any foreign cargoes arrive), and to have the barrel branded with such marks as may prevent a mistake on this head, that our trade may not suffer from the quality of such fish.

2dly.—He is to take care that all damaged herrings or of bad quality, e. g. such as take sick after having cast their fry, or on the eve of doing it, in a word, unmerchantable, be thrown aside in the packing, lest such bad fish corrupt the sound, or give them a bad flavour. That, moreover, the fish be properly salted and packed.—3dly. That the masters and crew of one buss do not put any hindrance in the way of another. In case they were unlucky, or could not succeed where they had cast nets, they must not remove to the ground of others to disturb their operations; nor damage their nets, boats, or other implements: in case they do, they must make good the damage. No buss employed in the fishery can be sold to foreigners, or hired out to them for the purposes of fishing. 4thly.—That the said overseer do inspect all the barrels before they are taken on board the busses, reject such as shall appear unfit, and mark such as he approves with the name of the cooper, and his place of residence; after having examined the quality of the timber, construction, hoops, &c. &c. 5thly.—He shall not suffer any buss out on the fishery before the 24th of June, and he shall require a declaration upon oath, before any of the herrings are landed, that there are none abroad taken before that period.

6thly.—He must take care that in salting herrings a distinction should be made. Thus, for example, the herrings taken between St. John's and St. James's day shall be salted with coarse and chosen salt. Those taken from St. James's day to the 14th of September must be packed up with the best fine salt. No herrings can be packed except such as are taken from the 14th of July to the 1st of January.

ary. Each sort to be packed up separately in barrels properly filled up, stopped, hooped, &c. Lastly,—No herrings can be sold or brought to market in this country that are not picked and sorted in the following manner. By this sorting and marking, the different kinds of herrings, and the time they were taken, are discriminated and named accordingly. Thus are to be met with in the market, St. John's herrings, St James's herrings, St. Bartholomew's herrings—none but these can be packed. The take of St. John is sent ashore in lighters, in order to be sold immediately for consumption. The St. James's herrings undergo a second packing, are reduced from fourteen barrels to twelve, then marked by the overseers: these are sent off in a commercial way.

The take of St. Bartholomew, the 28th of August, are marked with the arms of the city, and commonly sent to Cologne in Germany. The take of the 14th of September are likewise marked with the arms of the city, and sent generally to Rouen in Normandy: they are not marked until they have remained in the first pickle eight or ten days. Lastly.—They must have remained in salt ten days before they can be sold. No Scotch or other foreign herrings can be worked, cured, and packed, as if they were Dutch: they may be simply packed up in barrels without any stamp. The precaution on this head is carried so far, that no empty barrels of ours, marked as above, can be exported to any other country. This extract of regulations concerning the important commerce of the herring fishery, possessed as well by the States General as by the states of the province of Holland, is drawn from the Great Placart book, from the treaty with the city of Hamburgh, from the book of Handvoesten, all which the reader must examine, if he requires a more exact knowledge of the matter.

We have said above, that the inspection of the curing, packing, &c. is intrusted to overseers, who are appointed by government, and take up their residence conveniently to the harbours, or place where the business is going forward.

I have said already, and the common suffrage of all nations confirms it, that the Dutch herrings are the best. No other cause can be assigned for this general preference, than the scrupulous adherence to the regulations and provisions just now mentioned, it being by no means true, that the art of curing, salting, and packing herrings is confined to the Dutch alone. Other nations are as expert at doing all that as we can be; but in no other country is so much attention paid to this branch of commerce as in our republic, which is of so much the greater consequence to our state, as the necessary expenses of stores and fitting for the whale fishery are almost all defrayed from the profits of it. As long as these wise ordinances and regulations are punctually observed, and no breach of them allowed, notwithstanding the high wages, which may be considered as one cause of its decline, it may still flourish.

VII. *On the Boiling Point of Mercury, and the Fixing Points of Lead and Tin. By Mr.* JAMES CRICHTON, *of Glasgow**.

To Mr. Tilloch.

SIR,

I HAVE to request that you will correct a small error in the account of my thermometer, inserted in number LVIII, (p. 147). The bar is composed not of *iron* and zinc, as printed by mistake, but of *steel* and zinc. The engraver has also made the scale to read from right to left, instead of the contrary; but this is not material.

I have it now in my power to send you the *results* of a number of interesting experiments respecting the boiling point of mercury, and the fixing points of lead and tin, which I think may answer some important purposes to the philosophical world. A detail of all the steps followed, which were similar to those stated in my last communication, would only take up time unnecessarily.

The steady uniform point at which block-tin fixes surpasses my expectations, and is far more determinate than that of water. Lead has not, like tin, the property of instantly depressing and as instantaneously raising the thermometer at the moment of congelation.

Having had occasion, in constructing some very high-ranged thermometers, to take for the purpose of graduation some point much higher than that of boiling water, as in prolonging a scale to 500 or 600° from so comparatively contracted a scale, errors must unavoidably be introduced, I had recourse to the writings of the most respected British and foreign chemists, to find from them the fixing points of lead and tin. In this search I was greatly disappointed, for they do not agree amongst themselves, varying so much as 30, 40, sometimes 70 degrees. Besides, having good reason to suspect that none of them were near the truth, I resolved to determine the fact by actual and careful experiments, the results of which were so different from what have been received, that I think myself called upon to state them.

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* Communicated by the Author.

In order to obtain the more certainty, I made on purpose three accurate thermometers, on which I could perfectly rely; and from their agreements I can with confidence say that mercury boils at 656°, lead fixes at 612°, and tin at 442°;—their specific gravities being respectively thus: mercury 13,568, lead 11,346, tin 7,278, taken in distilled water at 62°. The gravities attributed to them by authors are nearly the same; which is so far satisfying.

When the experiments were made the barometer stood at 29·3, 24 feet above the sea.

James Crichton.

VIII. *On the new Planet Pallas. By Baron* Von Zach.

The proofs on which the improvement of our astronomy, both theoretical and practical, is at present founded, cannot be more conclusive; and the triumph which this science has obtained, even in the eyes of the public in general, cannot be greater than it is at present by the re-appearance of Pallas.

This small planet, after its discovery last year by Dr. Olbers, was scarcely observed four months when it approached so near the solar rays as to become lost in them; and having now emerged from them, after being invisible for six months, it has been again found, like a small point, hardly perceptible, among myriads of worlds, and exactly in the spot where its place was announced by theory. To this new discovery of the re-appearance of Pallas, the greatest possible physical and intellectual powers of man have contributed.

On the 18th of February, about 14^h 50′, Harding, that expert observer of Lilienthal, was so fortunate as to find again Pallas, exactly in the place where it ought to be according to the calculations of Dr. Gauss, inserted in the *Monatliche Correspondenz* for December 1802. The following night he had the pleasure to ascertain its existence in the most satisfactory manner. On the 15th of February, about 15^h, he found it nearly over No. 36 of Poniatowsky's Bull, as a small star of the 12th or 13th magnitude, and knew it to be the planet, as he had observed very accurately the night before No. 36 and the surrounding stars; but now saw a star in a place where 24 hours before none was visible. The next night this luminous point had moved as theory required, and on the 20th of February, about $15^h\ 30'\ 13''$ mean time, had advanced 55 seconds from No. 36 of the Bull, against which he saw it on the 15th of February.

Mr. Harding had the goodness to communicate to us immediately his important discovery. He informed us in his letters that he was not able to distinguish this planet with a three feet achromatic telescope, through which stars of the eleventh magnitude were clearly seen. Dr. Olbers, who on the 21st of February had the pleasure of again seeing his planet, wrote to us that he could distinguish Pallas very distinctly with a five feet telescope by Dollond; but at that time he considered it equal to a star of the twelfth or thirteenth magnitude, and its light equal to that of the fourth satellite of Saturn.

Dr. Gauss therefore, in the same manner as last year in regard to Pallas, is entitled to the greatest praise, and deserves our warmest thanks for the wonderful accuracy of his calculation, by the correspondence of which alone it was possible to discover again this planet among the innumerable host of telescopic stars. "Had there been an error of only from 30 to 40 minutes," says Mr. Harding, "in the calculations of Dr. Gauss, I much doubt whether I should have been able to discover Pallas again."

In the *Monatliche Correspondenz* for the month of March, page 277, we estimated the real apparent magnitude of Pallas, on its first re-appearance, to be equal to a star of the twelfth magnitude, and this conjecture has been found to be confirmed. In that number we gave Harding's chart of the probable orbit of this planet, in order to facilitate the finding of it, and this chart is now found to be its actual orbit; for the difference between the calculations and the observations hitherto made is so small, that it is not perceptible on our chart, though constructed on a very large scale, and nearly four times as great as that employed by Professor Bode for his celestial atlas. By the help of these charts, and the calculation of the orbit of this planet given in the *Monatliche Correspondenz* for December 1802, it will be possible, even without knowing that this calculation corresponds within two minutes with observation of the heavens, to find Pallas at all times when searched for with good telescopes.

Mr.

The following account is to be found in Ulloa's Voyage to South America*:—" In the valleys of Popayan, in South America, there are found insects which are very remarkable on account of the poisonous juices contained in their bodies. Among these there is one called *coya* or *coyba*, of a fiery red colour, and not larger than a common bug. It is generally found under stones, and in the fields between the grass and other plants. When this insect is bruised on the skin of any animal its pernicious juice penetrates into the pores, becomes mixed with the blood and fluids, and occasions ulcers, which, if a remedy be not speedily applied, soon produce death. The only remedy for this evil is to set fire to the dried stems of a certain plant, and to keep the body of the patient over the flame till it begins to swell; an operation which the inhabitants of these districts can perform with great dexterity. It has been observed that no bad consequences ensue when this animal has been bruised on the palm of the hand; from which it is inferred, that the small quantity of poisonous juice is absorbed by the callous parts of the skin in the palm of the hand, and cannot penetrate into the blood. The natives, who travel through these districts, often bruise the insect in their hands in order to gratify the curiosity of travellers. But there can be no doubt that the coya, if bruised in a less callous hand, the skin of which is thinner or tenderer, would produce nearly the same effects as when bruised in other parts of the body.

" Those who travel through these valleys, when they feel a sensation of puncture by an insect in the face or neck, take great care not to rub or to touch the part, because the least pressure bruises the coya, which when not bruised is harmless. On such occasions, they cause the natives who accompany them to examine the place; and if the latter observe a coya, they blow it off with their breath without touching it; by which means the travellers are freed from danger. The cattle in these valleys are led by instinct to use the same precaution; they breathe strongly on the herbs and grass on which they are about to feed. The mules, however, often eat the coya; after which they gradually swell, and at length die."

The province of Popayan, as is well known, is a district of New Grenada, and particularly remarkable because it is the only part of America where platina is found. I have seen no account of the coya in any other work, and there-

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* Vol. i. p. 343.

Greater semi-axis - - - - - - - - -	0·4426160
Daily tropical motion - - - - - - - -	769·4161″
Eccentricity - - - - - - - - - - - - -	0·245619
Dist. from the sun 1803 - - - - - -	301° 24′ 13″
Ascending nodes 1803 stationary -	172 28 8
Inclination of the orbit - - - - - - -	34 38 19·8

A comparison of these elements with the preceding observations shows that the difference is very small.

For the observations of Dr. Olbers give:

1803.	Calculated A.R. of Pallas.	Calculated Declination.	Difference in A.R.	Difference in Declin.
Feb. 21	272° 56′ 25·0″	7° 31′ 29·8″	−20·0″	+ 15·8″
23	273 28 46·8	7 45 52·3	− 8·8	− 7·7

IX. *On the Management and Improvement by Tillage of strong wet Loams in which the Clay greatly predominates**.

Here again I can meet the wishes of the Board, and give them the information they require from actual experiment, and on a very large scale. This soil, of which there are thousands of acres in Suffolk, is in that district called clay; but I believe there is no pure clay in that county. The lands having a greater or smaller portion of calcareous matter in them, are consequently of a better quality than the clays in Surrey and Sussex, though they were in no higher repute until within these twenty years. A manor farm near Eye was offered for sale about fifteen years since, when I purchased it, much against the inclination of my friends, who joined in the general cry of the farmers, and pronounced the lands the poorest that ever crow flew over. This did not discourage me. I found a proportion of calcareous earth intermixed with the clay on the surface, and at two and three feet below the surface a stratum of fine marl in almost every field. The estate was so depreciated that I did not attempt to let it. Of the two hundred and thirty-five acres, of which the whole consisted, the major part was old sour grass lands, full of water quitch, overrun with black and white thorn, bushes, and pollard trees. I began my operations by grubbing the thorns and trees, levelling all the old fences, and filling up the ditches where they formed inconvenient angles and corners. Then the fields were squared; new ditches cut, four feet and a half wide at the top, one at the bottom, and three feet six inches deep, and planted with white thorn: expense per rod two shillings and three pence, exclusive of white thorn and bushes. Drains were then drawn out as deep as possible by the plough, dug with the common spade ten inches, then with a narrow spade ten inches*, and with a small gripping spade fourteen inches, filling the latter trench with haulm, and putting bushes on the top of it, covering the whole with mould. Heath, where it can be procured, is preferable. Expense per score yards four shillings and six pence. Marl pits were then opened, when possible, at the point where four of these new ditches met, by which the distance of carting was reduced; and with a few posts and rails the fences were made good, and a permanent pond of water secured to each field, after the marl carting was finished. Eighty loads was the quantity per acre, at two pounds twelve shillings and six pence per hundred loads.

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* From Mr. Close's paper published in the Communications to the Board of Agriculture, vol. iii. part 1.

This marl was spread on the old sour grass, and left to be pulverized by the winter's frost, and incorporated with the surface by the feet of the cattle and sheep. Early in the spring the lands, thus prepared, received one clean deep ploughing, and were drilled with oats; produce about thirteen quarters per acre. The second year drilled with peas; produce seven quarters and a half per acre: ever since which time they have been cropped regularly, and the produce has been immense. That the Board may form an accurate idea of the advantage derived from converting these old grass lands into tillage, I shall state the value of the farm per annum when I purchased it, and the money I gave for it; then the sum I received for my improvements; the rent at which I let it on a twenty-one years' lease; and the price I received for it when sold. There were two hundred and thirty-five acres, valued at one hundred and twenty pounds per annum, (the tenant paying twenty pounds a year land tax) and the price I paid for it three thousand three hundred pounds. I received for my improvements, of the tenant five hundred pounds, besides three hundred and fifty pounds which the old pollard trees sold for; let the farm for two hundred and twenty pounds, the tenant paying twenty pounds a year land tax, and covenanting to keep all the buildings, &c. in perfect repair. I then sold it for six thousand five hundred pounds. The gross produce per acre of these old sour grass lands could not exceed twenty shillings; and little or nothing was expended on productive labour. Ever since they have been converted into tillage double the former gross income must have been expended for labour, and the aggregate produce at least six pounds and six shillings on every acre, including the whole rotation of crops. You have here, gentlemen, one strong proof of the utility of converting certain portions of grass lands into tillage. This farm is now in such a high state of cultivation, that any part of it might be converted into meadow land by the mode recommended in my former chapter, and would be worth two pounds per acre, as I shall demonstrate from actual experiments on rather inferior, though somewhat similar, soils.

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* In the old grass lands the sod was reversed and placed over to the bottom drain, which formed an effectual covering.

When first I became rector of ...... in the county of ...... there were two fields of fifteen acres much impoverished by constant cropping and tillage, valued in that state at sixteen shillings per acre; soil a strong loam, and very wet. These fields I drained, to take off the surface water, as recommended in my first chapter; then fallowed and dressed them, sowing the hay seeds, &c. in August. The produce the first year was estimated by many farmers at four tons per acre; it was so stout that the mowers had double their usual wages, and it was impossible to spread it on the land. I have ever since, viz. for eight or nine years, let these lands at two pounds two shillings per acre. As the tenants have been frequently changed, the lands have been mown almost every year, and are now somewhat exhausted, but may be made as good as ever by a dressing, and by being fed with sheep one summer; or, if ploughed, would produce abundant crops for eight or ten years, and might then be converted into meadow by the process I have before recommended: the course of crops will be found at the end of this essay as best adapted to clayed loams. By the mode of cropping there recommended, a farmer may continue sowing wheat in any season. If too wet to work his pea, bean, or vetch land, his clover lands will be in order; if too dry for the latter, the former will work well, and he may be sure of putting in his seed in good time. It will be obvious, that when clover is sown with the spring corn, the three feet ridges, on which the turnips or cabbages grew, must be levelled, and five or ten feet ridges formed. The clover seed should be sown immediately before the last horse-hoeing. I could bring proofs ad infinitum to demonstrate the folly of old grass lands to remain untilled, under the foolish idea of not impoverishing the land. So far from being improved by such a state of rest, the lands, by being saturated with stagnant water, become worse and worse every year; and one hundred acres in this state, at seven shillings and six pence per acre, would starve the tenant and his cattle, bring a very poor rent to the landlord, and produce nothing to the public. Under the management I have recommended, the income of the landlord may be nearly tripled, and the produce to the tenant and the public increased in a tenfold degree. But I will, as the Board requires, substantiate these assertions by further experiments made under my direction on some lands in Sussex, belonging to his grace the duke of Richmond, and on others in Suffolk, the property of lord Rous. One will be an additional proof of the utility of breaking up old sour grass lands; the other, of the advantage of restoring lands exhausted by constant tillage into good and productive meadows. It may be necessary to say, that I applied to my two noble friends for an account of the following experiments, and received permission to transmit them to the Board. The farm of lord Rous consisted of two hundred acres of land, half of which was old sour grass, let for ninety pounds a year; but previous to my riding over it with his lordship, his steward had valued it at one hundred and fifty pounds a year, allowing a specific number of acres to be broken up every year, on condition that the tenant laid down with hay seed an equal number of acres of the old arable lands. I stated to lord Rous, that he could not have a full and fair rent for his farm under covenants, or without allowing the whole to be cleared, ditched, drained, marled, and ploughed, except about ten acres near the house. To these conditions he readily, and from conviction, acceded; and I valued the estate (which was then let at ninety pounds a year, and estimated with a partial privilege of breaking up the old pastures at one hundred and fifty,) at two hundred and twenty-five pounds; for which sum it was let to an unexceptionable tenant, so much approved of, that at the end of three years, before he had reaped the fruits of his own industry, his lordship offered him a much larger occupation, if he could find a tenant of character to repay him for his expensive improvements. Several candidates offered, out of which lord Rous selected one who paid the outgoing tenant five hundred and fifty pounds for his ditching, draining, and marling, and expended about two hundred more in similar improvements. Precisely the same system was adopted by these spirited farmers for improving these lands, as I have before given the particulars of in the account of the estate I purchased; and a recapitulation would be protracting this essay, which I wish should contain, in the most concise and plain terms, as great a mass of information as I am capable of furnishing. As his grace the duke of Richmond, with that exactness which marks his conduct in all matters of business, has furnished me with an account of his experiment, attested by his signature, I should not feel myself justified in deviating from it, even in form, but shall transmit it in the very words in which I received it: " I do certify, that some years ago above fifty acres of land that I wished to convert from tillage into pasture, which had been attempted several times by the common mode of sowing grass seeds with corn, without success, were (after two years successive turnips fed off) sown with hay seeds in a very large proportion, in August, without corn, as advised by the Rev. Mr.——; and the result was, two crops of hay the first year; one estimated at two tons per acre, and the other at one ton and a half. The land has since been dressed with old mortar and other manures occasionally, and the permanent improvement on the value of the land is estimated to amount to at least fiften shillings per acre*.

' Jan. 23, 1801. (Signed) " RICHMOND."

These experiments exactly correspond in their result with those which I have stated from my own practice; and being made in different situations, they seem to be conclusive on the most important subjects selected by the Board as objects of their premiums. The characters of the noblemen under the sanction of whose approbation I have given you the particulars of the two last experiments, stand too high in the estimation of the country for honour, veracity, and minute exactness in every transaction of the least importance, to need any encomium from me. For the particular management of these experiments I shall refer you to those similar ones made upon my own lands, from which no variation was made except in the mode of draining the duke of Richmond's lands; which, to the best of my recollection, were made with bricks. Though the query respecting the utility of draining may be supposed in effect to be answered by its being the foundation of all the advantages which I have so minutely detailed, yet I must urge the necessity of this operation as the *sine quâ non* of agricultural improvement; and add, that though the particular method of surface-draining strong clays and of clayey loams, where the substratum was less retentive of water, has proved both cheap and effectual, yet in all bogs, peats, gravels, or sands, where a large tract of land may be made firm and dry by one cut, Mr. Elkington's mode of tapping and boring is a very superior method of effecting this necessary and primary improvement. Upon rich black loams, sandy loams, good deep lands with a chalky substratum, peats, &c. (all soils capable of producingthe natural grasses of this country) the same system as has been recommended in the preceding experiments will be found effectual for converting them into meadows or pastures; only varying the rotation of crops according to the table at the end.

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* By a letter previously received from the duke of Richmond, his grace informed me this was exactly double the value of the land.

Objections to sowing Grasses with any Crop of Corn, when it is required to obtain a Meadow; and a comparative Estimate of the two Methods.

Here again I can give you, my Lord, and the Board, information from actual experiment. A friend of mine wished to procure a good meadow or pasture around his house: he fallowed the land for barley; but the spring proving wet, and the soil being a strong loam, he could only put half of it in order for that crop, which was sown, and laid with clover and rye grass. The other part was fallowed, and sown in August with the sweepings of hay chambers, as I have recommended. The barley was a good crop, and the clover and rye grass were probably equal to the first year's cut of hay. The second year the artificial grasses began to fail; worse the third, fourth, and fifth. The sixth year, after having received two dressings, the spontaneous product of the soil began to give a fleece over the surface of the land. About ten years after these lands were sown, I saw this field, when the part sown in August was worth at least fifteen shillings per acre more than the part which had been sown with artificial grasses in the barley. Thus from actual experiments, numbers of which I could adduce, I conclude that sowing the sweepings of hay chambers in August is preferable to sowing artificial grasses in the spring with any crop of corn. Suppose the corn worth five pounds per acre, the difference in the produce of hay or feed in the second, third, fourth, and fifth years, would more than counterbalance

lance this; and the proprietor would find a permanent improvement in his land of from fifteen shillings to twenty shillings per acre *.

Many object to sowing such rubbish as the sweepings of hay chambers produce; and I wish most sincerely any method could be devised for procuring clean seeds of our best and natural meadow grasses. It is a great desideratum; and premiums to encourage agriculturists to save seeds of the fescues and poas, &c. and for the largest quantity of land sown with these seeds, and kept distinct, might be of infinite service. Until this can be effected, the plan I have submitted to you, my lord, appears to me the most eligible. It certainly has been crowned with the greatest success. Sowing rubbish in August is not of so great importance as in the spring. In the former season all the annual seeds vegetate; and if the beginning of the winter be mild, they will blossom; but they cannot perfect their seed, and the first frost destroys them. If sown in the spring, they vegetate, blossom, perfect and shed their seeds, and thus stock the land with noxious weeds. The facts I have stated must do away the objections to sowing rubbish. It is immaterial what you sow, if you do but obtain an abundant crop, and leave your land clean and in good order. Should it, however, be thought proper to sow the seeds with any corn, barley must have the preference. If sown with oats, and the land be prepared as it should be, viz. in high tilth and order, the oats will be so luxuriant as to smother and destroy the young plants. Lands intended for grass or meadow cannot be in too high a state of cultivation. The permanent improvement in the intrinsic value of the land will abundantly repay almost any expense. To improve the soil with this view, and then to exhaust it by a crop of corn at the time of sowing the seeds, appears to me a sure method of counteracting the very object in view. I have always advised the landlord to provide the hay seeds, that he might be sure, not only of the quality, but that the quantity should be sown. Indeed, as the improvement is a permanent one, if the tenant make a good fallow and dress the land well previous to sowing the seeds, or directly after carting the hay, he does his proportion. On these terms I have permitted many tenants to break up grass lands, on their cove-

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* Had the barley been drilled, horse- and hand-hoed, and kept perfectly clean until July, and then sown with the common hay seeds, the only objection to the plan would arise from exhausting the soil by a crop, when your great and leading object is to make a permanent improvement.

nanting to lay them again in a specified number of years, preparing the land by a complete highly pulverized fallow, and sowing the hay and other seeds of the landlord's procuring, the first week in August; and after the first crop of hay to dress the lands with twenty loads of good dung, or compost, per acre, if the soil was of such a quality as not absolutely to require dressing previous to sowing the hay-seeds. A dressing before sowing, or after carrying the hay the first year, is absolutely necessary.

The additional rent must be regulated by the quality of the land, from ten shillings per acre to fifteen and twenty shillings. If the land be naturally very sterile, and difficult to till, as in the wilds of Sussex, and the tenant be obliged, by paring, burning, and fallowing, to purchase his first crop very dearly, no additional rent should be required, but he should be tied to such a judicious mode of cropping as would leave the lands in an improved state for the landlord. See the Table at the end. If the land, after being drained and marled, will produce a good crop of oats with one effectual ploughing, the tenant may pay an additional rent of ten shillings per acre. Where the lands will produce a good crop from one effectual ploughing, without the expense of draining and marling, the rent may be advanced fifteen shillings per acre; and on lands naturally very good, a landlord may expect an additional rent of twenty shillings per acre of grass land he may allow to be converted into arable.

The depth at which grass lands should be at first ploughed must depend on the nature of the soil. In my directions for improving sterile clays, I have, I trust, proved the utility of moving such soils to a great depth. Similar observations will apply to all clayey loams, where the surface and the substratum consist of the same component parts. On deep rich loams of all denominations deep ploughing will be advantageous; but when there are only four or five inches, or less, of good earth, and the under stratum be a rubbly chalk, gravel, or other unproductive soil, it should never be brought to the surface; and, being naturally pervious to water, it will not require being moved below the action of the plough.

In my practice I have almost invariably mown the grass the first year, and immediately dressed it well and fed it the second. My success, in every instance, has been great, and no inconvenience ever arose from this system. In some few cases I have seen much injury arise from feeding the first spring: the lands being in a high state of cultivation in August, and the surface kept very light by the frosts and fleece of grass, the cattle have pulled up the young plants by the roots, and entirely destroyed them. The duke of Richmond's bailiff was very desirous to feed the young grasses before mentioned in the spring; the flock wanted the food, and there was an abundant crop. So urgent was the man, that the duke, who had promised that my directions should be implicitly obeyed, wrote for my consent; but to this plan I positively objected, declaring that, should the stock be allowed to feed the lands the first spring, I would not answer for the success of the experiment. His grace, who is always firm on such occasions, would not therefore give his consent to his bailiff's request. The result of the experiment was such, that neither the duke nor his steward ever regretted adhering to my advice.

[To be concluded in our next.]

X. *Observations on some Insects little known.* By M. Meyer, *of Göttingen*[*].

In endeavouring to class insects which have not been accurately described by those who saw them, nothing more can be offered than conjectures. The few characters which are given must be employed in order to deduce from them something nearer the truth; and it then becomes the duty of future observers, when they have an opportunity of seeing new insects which have any similarity to those imperfectly described, to examine and establish the points in which they correspond. It is by these means only that we can attain to certainty in things which have before rested only on conjecture.

I have for some time past read, in different authors, accounts of insects, which attracted my attention the more, as noxious qualities were ascribed to them: I wished that I might be able to obtain better information than I found in the books where they are described respecting the genera to which they belong; and, having taken the trouble to determine this as far as possible, I here give the result of what appeared to me most probable. I am far from giving these observations as strictly agreeable to truth: on the contrary, I consider them as very imperfect; but it is only by making them known that I can obtain more certainty in regard to my conjectures.

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[*] From *Magazin für das Neueste aus der Physik*, vol. ix. part 2.

The following account is to be found in Ulloa's Voyage to South America[*]:—"In the valleys of Popayan, in South America, there are found insects which are very remarkable on account of the poisonous juices contained in their bodies. Among these there is one called *coya* or *coyba*, of a fiery red colour, and not larger than a common bug. It is generally found under stones, and in the fields between the grass and other plants. When this insect is bruised on the skin of any animal its pernicious juice penetrates into the pores, becomes mixed with the blood and fluids, and occasions ulcers, which, if a remedy be not speedily applied, soon produce death. The only remedy for this evil is to set fire to the dried stems of a certain plant, and to keep the body of the patient over the flame till it begins to swell; an operation which the inhabitants of these districts can perform with great dexterity. It has been observed that no bad consequences ensue when this animal has been bruised on the palm of the hand; from which it is inferred, that the small quantity of poisonous juice is absorbed by the callous parts of the skin in the palm of the hand, and cannot penetrate into the blood. The natives, who travel through these districts, often bruise the insect in their hands in order to gratify the curiosity of travellers. But there can be no doubt that the coya, if bruised in a less callous hand, the skin of which is thinner or tenderer, would produce nearly the same effects as when bruised in other parts of the body.

"Those who travel through these valleys, when they feel a sensation of puncture by an insect in the face or neck, take great care not to rub or to touch the part, because the least pressure bruises the coya, which when not bruised is harmless. On such occasions, they cause the natives who accompany them to examine the place; and if the latter observe a coya, they blow it off with their breath without touching it; by which means the travellers are freed from danger. The cattle in these valleys are led by instinct to use the same precaution; they breathe strongly on the herbs and grass on which they are about to feed. The mules, however, often eat the coya; after which they gradually swell, and at length die."

The province of Popayan, as is well known, is a district of New Grenada, and particularly remarkable because it is the only part of America where platina is found. I have seen no account of the coya in any other work, and therefore I am much inclined to suspect the accuracy of the above. At any rate, very little can be gathered from this description; but, as the insect is compared to the bug, there is reason to conjecture that it may have some affinity with that species. It is, however, difficult to determine whether it be an acanthia, a cimex, or a reduvius. Besides, we have hitherto no instance that any insect belonging to this species can produce so noxious effects; and if the coya really exists, the account of it is probably exaggerated, as Ulloa's information was no doubt derived from the natives. Stoll[*] describes an insect of Surinam, which perhaps is the same as the coya, or has an affinity with it. He calls it the poisonous bug, and gives the following description of it:—"The feelers, which consist of five joints, are yellow; the eyes are very prominent, and behind these eyes are placed two small shining ones: the neck is united to the corslet by means of a small tube. The fore-legs are covered with black hairs; which gives them a very rough aspect. The corslet is of a shining appearance, black and arched: the membranous part of the sheaths of the wings is transparent, and the wings themselves are brownish yellow: the hind part of the body is of a dark yellow colour on both sides. It is said that this bug is furnished with a sting, and that the puncture it makes occasions unsupportable pain." I have mentioned this bug, in my Natural History of poisonous Insects[†], under the name of *reduvius venenatus*. The *reduvius annulatus*, however, seems to approach nearer to the coya, because it is red; but this bug, as far as I know, is not a native of America, which is the country of the *reduvius venenatus*.

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[*] Vol. i. p. 343.

M. Schroter, superintendant of Weimar, is the first naturalist who gave an account of the *moschka*. It is to be found in the first part of his *Treatises on various Objects of Natural History*[‡]. The account he gives is in substance as follows:—"The *moschka* has some similarity to the *musca nemorum* of Linnæus: it is, however, different. It differs from Schäfer's *æstrus* by its four eyes, as the latter has only three. It is very dangerous to cattle, and particularly to oxen. It makes a loud humming noise when it approaches them, falls suddenly upon them, and then drops on the grass. The cattle as soon as they hear this enemy at a distance betake themselves to flight, and cannot be restrained. This insect seldom appears, and flies with great rapidity: it does not long remain on the cattle, but throws itself on the grass, and therefore can with difficulty be caught. M. Schroter was able only once to obtain one of these insects. It is fond of frequenting those fields which are interspersed with clumps of trees. The head is oval, and tapers towards the mouth; the mouth consists of two moveable jaws, and the pincers are like those of the beetle. The whole head is covered with hair, but more so around the mouth than on the forehead. The hair on the upper part of the head stands erect like bristles, but that around the mouth hangs down. Its colour is whitish. Two large eyes stand on each side of the head, and two small ones in the centre. The large eyes are cut into facets, the small ones are plane. It has a white hairy band, which passes round the whole neck and ends at the extremities of the first two legs. The corslet is round, very black, and at the extremity towards the body covered with single, crooked hairs, white at the points. The two wings are white, a little brownish, and not much longer than the body. The hind part of the body is entirely covered with hair; the upper part towards the corslet is entirely black, interspersed with single white hairs; but the lower part and the rump are yellow. The whole body terminates in a point. On the lower part the insect is completely hairy and black, except on the rump, where it is yellow. The six legs are black: the hind thighs are covered with hair, and the tarsus has six joints.

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[*] Tab. xiii. f. 66.

[†] Part i. p. 141.

[‡] Abhandlungen über verschiedene Gegenstande der Naturgeschichte, Halle 1776, 8vo.

M. Schroter reckons this kind of gnat among the Linnæan flies with silk-like naked feelers; and it certainly deserves to form a peculiar variety. He has given a representation of it, tab. i. fig. 6.

After him few seem to have paid any attention to this insect till it was again brought to notice by M. Bechstein. He introduces it under the name of the cattle fly, *musca nemorum* Linn., as an enemy to cattle, and says it has four eyes, and that the body is marked with yellow rings and three white bands.

From various grounds it appears that this insect, till its natural history has been more accurately examined, may be placed in the same class as the *rhingia* of Fabricius. Experienced entomolgists will certainly do a great service to natural history by undertaking the examination of this subject. Should it be found that it really belongs to this class, it might be introduced into the system under the following characters:

Rhingia tinniens. R. hirsuta, atra, thorace glabro, abdominis segmento primo albo, ano flavo.

Lepechin, in the first part of the Journal of his Travels through different Parts of the Russian Empire*, describes a spider, which, on account of its remarkable characters, deserves a place in our systems, of which it has hitherto been deprived. It has, perhaps, been overlooked, because Lepechin calls it a tarantula; a name very improper, since the spider described by him differs very much from the tarantula (*aranea tarantula*). At any rate, on account of this difference it ought to be distinguished by the name of the Tartarian tarantula. His description is as follows:

On account of the saline nature of the districts around Usolia, a very dreadful kind of spider is fond of frequenting them. These animals reside in holes under the earth, and the passage which conducts to them does not seem to be above a quarter of an arschine in depth. They extend their webs on the ground before their hole, and devour without mercy the insects which fall into them. The exterior part of the body is entirely covered with soft hair, which gives them a horrid appearance. The head has the figure of a half pyramid, the sharp side of which represents the middle of it, and the bottom the forehead. The breast is covered by a roundish shield, whitish on the edges and furnished in the middle with black hair. Small white and almost imperceptible veins, which proceed from the white edge of the shield, meet in the centre. On the top of the head there are eight eyes, the smallest of which, four in number, occupy the lowest place before the mandibles: above these there are two large eyes, and above these two others of a moderate size. Besides these there are large, bright red eyes on the sides above the mandibles. The two mandibles are covered with soft hair, blackish at the ends, armed with small pointed and stiff hooks. The feelers consist of four joints; three of them are reddish brown; the other, which is the last, is black, and has a blunt point. On the back there are two rows of whitish points between the black ones.

The lower part of this spider is black and rough. The first joints of the legs are reddish brown on the lower side; the next is short, and black all around; the third is reddish brown, with a black band at the end; the fourth and fifth are grayish black, and the last is split in the form of a claw. The rump is furnished with six papillæ, two of which are on the sides, two below, and two between the upper and lower ones, which are shorter than the others. Though I made inquiries among the inhabitants with the greatest care, I could not find that there ever had occurred any instance of people being hurt by this formidable animal.

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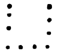
* German edition, Altenbourg 1774, p. 200.

In p. 257 of the same work Lepechin gives a further account of this spider in the following words:—" In this place we learned some further particulars respecting the tarantula. Having dug up some of their nests, we observed what this animal uses as weapons against its enemies. When it finds itself deprived of every means of flight, it remains quiet, puffs itself up, and ejects from its back, as if through a syringe, a white juice to the distance of two arschines. I cannot, with certainty, say whether this juice be poisonous or not, because none of us were inclined to make the dangerous experiment. But we were assured by one of the cossacs who were on guard at Woguti, that a woman of Little Russia had the misfortune to feel the effects of this juice. She dug up a spider of this kind, and on turning it with a stick it squirted some of the juice on her hand, which in a short time became inflamed: the part swelled with unsupportable pain, and the most serious consequences might have ensued had not a remedy been speedily applied. The best antidote to this poison is the animal itself. It is put alive into oil, and kept in that manner for use. If the part exposed to the action of the juice be rubbed over with this oil, a cure takes place without any need of employing music; which, indeed, could not be obtained in these districts, as the whole music of the country people consists of a screeking noise produced by reeds, or the stem of the wild angelica (*angelica sylvestris*). These spiders were so quarrelsome, and greedy of devouring each other, that of twenty of them confined in a glass only one remained alive as conqueror. The black sheep, it is said, are great enemies of the tarantulas. These animals dig them up from the earth, and readily eat them; on which account these sheep are highly esteemed by the Calmucs, who are much afraid of the tarantulas, so that they never encamp in places frequented by them, but proceed to some other district, however tired their cattle may be.

Lepechin calls this kind of spider a tarantula, and consequently it must have some similarity to the tarantula of the system. According to Fabricius the tarantula has ten eyes, in the following form, ; whereas that of Lepechin has only eight, as the large red eyes above the mandibles will hardly be considered by entomologists as real eyes, but as prominences which have the form of eyes without possessing their properties. If this conjecture, which appears to me very probable, be well founded, and as the eight real eyes of Lepechin's spider, according to his own account, are arranged in this manner, $\overset{:\ :}{....}$, it belongs to Fabricius's sixth family of spiders, where the *aranea clavipes, saccata,* and *fumigata,* are placed. In regard to form, it seems to have the greatest similarity to the *aranea clavipes*; from which, however, it is so different in many respects, even without taking into account the diversity of country, that few entomologists will be inclined to consider both as varieties of the same kind. The parts of the mouth are not different from those of spiders in general: what Lepechin calls feelers are, in all probability, nothing else than what entomologists have long and much better distinguished by the name of *palpi*; for it may with certainty be affirmed, that no *antennæ* have ever yet been found in spiders. It is seen, from what has been said, that Lepechin's tarantula certainly deserves to form a new kind in the system, which may be placed after the *aranea clavipes* as follows:

ARANEA tartarica oculis $\overset{:\ :}{....}$.

A. hirta atra, thorace albo marginato, rotundato, abdominis dorso seriebus duabus punctorum albicantium, pedibus e rufescente-nigris.

Habitat in Tartaria valde frequens.

Subterranea, retiaria. Hirta, atra. Thorax rotundatus, albo-marginatus, medio alter, pilosus, verulis albis. Mandibularum pars lateralis, maculis magnis, ocularibus, rubris. Palpi rufescentes, articulo extremo obtuso, nigro. Abdominis dorsum seriebus duabus punctorum albicantium. Femora priora subtus rufescentia; tibiæ tarsique annulis atris, rufescentibus et nigris. Anus papillis sex.

Succo viroso albo prædita.

It is not improbable that this spider is of the same kind as that described by Laxman* under the name of *aranea Singoriensis*: at any rate there is great reason to make this conjecture, from what Gmelin says of this spider, in describing the *aranea tarantula*†: should this be the case,

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* Nov. Comment. Petropol. vol. xiv. p. 602.

† See Systema Nat. Linn. ed. 13, tom. i. pars v. p. 2956. n. 34.

Laxman's *aranea Singoriensis* ought not to be considered as a variety of the *aranea tarantula*, but rather as a variety of Lepechin's spider, or as a particular species. Whether the large red pair of eyes be really eyes, still remains a problem which deserves to be an object of future research to naturalists; and the question, whether the proper tarantula (*aranea tarantula auctorum*) be found in the southern parts of Russia, requires further proof before it can be answered in the affirmative.

In *A Voyage to Madeira and the Leeward Caribean Islands*, by a lady, London 1792, 8vo. mention is made also of a tarantula, which, according to every probability, is nothing else than the *aranea clavipes*, or a large American kind of earth spider. It does not appear to be the *aranea avicularia*, at least according to the description of it, which is very defective, like all the descriptions of natural objects found in books of travels written by ladies. There is reason, however, to conjecture that it is not the real *aranea tarantula*, because this spider has never been found in the districts which the authoress visited. Of her spider she gives the following account:—" A kind of tarantula is found in Antigua in stony places and under old ruins. Its bite produces convulsions with strangury, and sometimes proves mortal. Music, however, produces no sensible effect on those bitten by it." It certainly is worth while to make some further researches respecting this spider.

In large collections there are generally found two varieties of the American bird spider (*aranea avicularia*); a larger than the common, which is dark brown, and a smaller, of a bright cinnamon colour, a very beautiful specimen of which I saw in the collection of Mr. Ortman, apothecary of Hamburgh. The owner of it observed on the inside of the first joint of the fore-legs of some specimens a small, black, corneous, sharp-pointed spur. Is it not probable that this spur is a distinguishing mark of sex, and may not this mark be peculiar to the male? These questions can be best answered by comparative observations.

John Angelo Brunelli, in his description of the river of the Amazons, in the Transactions of the Institute of Bologna*, gives an account of an insect, which is so badly described that no entomologist can with certainty determine to what genus it belongs, though it is probable that it belongs to the *cymothœa* of Fabricius: at any rate I am of opinion that we shall not be far from the truth if we consider Brunelli's insect as a little known species of this genus till the natural history of it has been better explained. The following is his account of it:—"The Brasilians give the name of *condiru* to an insect shaped like a small worm, which, when stroked with the finger in one direction, is smooth, but when stroked in the other feels so rough that it wounds the finger. It is greedy of blood from wounds; it is dangerous to the crocodile when wounded in the water, and is found in many places in multitudes. It makes its way into the limbs of the natives, and cannot be taken out without laying open the part: the men, therefore, must be very cautious when they go into the rivers."

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* Comm. de Bononiens. Scient. et Art. Institut. atque Acad. tom. vii. 1791, 4to.

XI. *Miscellanies in Natural History: viz. An Improvement in the System of the Mammalia; Observations on a living Opossum; and an Account of the third Generation of the Porcupine Man. By Professor* BLUMENBACH *.

THE system of the mammalia, which I have made the foundation of my *Manual of Natural History*, and which has been followed by various naturalists in their works, will, I hope, be rendered more agreeable to nature, and more perfect, by the following alteration, which has been occasioned, in particular, by the discovery of the *ornithorhyncus paradoxus*. The organs of motion are made the chief ground of these orders, because they soonest strike the attention, and are in the most intimate relation with the whole habits of the animals. I have, however, subdivided two of them, which comprehend a great variety, into two families, according to the diversity of their incisor teeth, and distinguished them by the known names of some Linnæan orders, that those whole classes are arranged as follows:

I. ORDER. BIMANUS.
  1. Homo.

II. QUADRUMANA.
  2. Simia.
  3. Papio.
  4. Cercopithecus.
  5. Lemur.

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* From *Magazin für den Neuesten Zustand der Naturkunde*, &c., by J. H. Voigt, vol. iii. 1802.

III. CHIROPTERA.

6. Vespertilio.

IV. DIGITATA.

Mammalia with detached toes on all the four feet. This order is divided, according to the diversity of the incisors, into the following families:

A. GLIRES with mouse-like incisors.

7. Sciurus.
8. Glis.
9. Mus.
10. Marmota.
11. Scavia.
12. Lepus.
13. Jaculus.
14. Hystrix.

B. FERÆ. The *rodentia*, properly so called, and some other genera with similar teeth.

15. Erinaceus.
16. Sorex.
17. Talpa.
18. Didelphis.
19. Viverra.
20. Mustela.
21. Ursus.
22. Canis.
23. Felis.

C. BRUTA. Without incisors, or at least without fore-teeth.

24. Bradypus.
25. Myrmecophaga.
26. Manis.
27. Tatu.

V. SOLIDUNGULA.

28. Equus.

VI. BISULCA. The ruminating animals with divided hoofs.

29. Camelus.
30. Capra.
31. Antilope.
32. Bos.
33. Giraffa.
34. Cervus.
35. Moschus.

VII. MULTUNGULA. Mostly large, or shapeless, bristly, thinly haired mammalia, with more than two claws on each foot; comprehending swine, for these properly have four claws.

36. Sus.
37. Tapir.
38. Elephas.
39. Rhinoceros.
40. Hippopotamus.

VIII. PALMATA. Web-footed mammalia, again divided, according to the diversity of their incisors, into the above three families.

A. Glires.
41. Castor.

B. FERÆ.
42. Phoca.
43. Lutra.

C. BRUTA.
44. Ornithorhyncus.
45. Trichechus.

IX. CETACEA.
46. Monodon.
47. Balæna.
48. Physeter.
49. Delphinus.

Observations on a living Opossum, Didelphis marsupialis.

Some months ago I obtained that wonder of all the land animals, as Mr. Lawson calls it, for which I was indebted to the kindness of an American friend, Dr. Tidyman, of Charlestown, in South Carolina.

It is about as large as a middle-sized cat. Its head is shaped like that of the fox: but its long snout, and the bare flesh-coloured nose turned somewhat upwards almost in the form of a snout, are nearly like those of a pig. The aperture of the mouth is exceedingly wide: the lower jaw is perceptibly shorter than the upper; and the upper angular teeth, even when the mouth is shut, are visible. The head is white, with a faint blackish stripe along the forehead, and the part between the fore corners of the eyes and the snout is of the same colour. Both sides of the mouth, and in particular the chin, are furnished with a great many long stiff hairs. The pupil of the eye is small, but the cornea is proportionally large and exceedingly convex, so that very little of the white of the eye can be seen; and this, with the dark brown colour of the iris, gives to the animal a lively appearance. Of a *membrana nictitans*, as among the quadrumana, scarcely any rudiment is to be seen. The ears are large, black, naked, and, according to appearance, merely membranous, without any cartilaginous folds, and therefore nearly like those of the bat; in my animal also, without the white border which is ascribed to others of this genus.

The neck is short and thick, and the same is the case with the rump, which is well covered with hair. Sometimes the hair on the back is long and erect, almost as in the badger; its colour is white mixed with black, and darkest on the shoulders.

The bag on the belly is very apparent by its prominence, especially when the singular *ossa marsupialia* or *cornua pelvis abdominalia* lie under it. The place of its aperture is marked only by a longitudinal fissure.

The tail is about the length of the body; it is almost naked, and as scaly as that of the rat, but a real *cauda prehensilis*.

The shoulders and fore legs are black, and covered with soft hair. The toes are naked, and of a flesh colour. The hind feet are furnished with detached toes with a small flat nail, but on all the other toes there are hooked claws of a white colour.

A figure of the animal, drawn from the life, may be seen in my *Abbildungen naturhistorischer Gegenstande*, tab. 54.

It is a real *animal omnivorum*, and can feed upon any kind of fruit; it is fondest of plums, and of other food, next to flesh, of fowl, game, and in particular of soup and bouilli. It chews its food with great deliberation, and catches the large pieces very dexterously with its fore feet; and it uses these feet with great address for dressing its snout, on which occasions it sits on its hind legs like a squirrel.

Its cry, which it seldom emits except when irritated, is a weak kind of grunting. It drinks very little, and sometimes not for several days. It seldom makes water, and even when in good health voids its excrements only once in four or five days. It however does neither in the place where it lies, but always retires to a corner of its kennel.

In general it preserves itself very clean; and on the whole is a quiet, good-natured animal; slow, and as it were cautious in all its motions; and of so strong a constitution that the people in America are accustomed to say, "If a cat, according to the proverb, has nine lives, the opossum has nineteen."

I shall now say a few words respecting the oldest accounts and figures of this animal, which were published in Europe after the discovery of the New World.

The first person who made mention of it, as far as I know, was V. Pinzon, who accompanied Columbus on his first voyage of discovery. This notice is to be found in Herwag's Collection (Novus Orbis, the first edition of 1532, p. 121*.)

About the end of the fifteenth century one of these animals was brought alive to Seville, and presented to the king of Granada.

Peter Martyr, who saw a dead specimen of this animal, gave a more accurate account of the opossum, which he thus describes: "Monstrosum animal, vulpino rostro, cercopitheca cauda; vespertilioneis auribus, manibus humanis, pedibus simiam æmulans, &c."

The name of *simivulpa* was first given to it by Gylli, in his edition of Ælian, 1553, 4to. p. 209; and this denomination was afterwards adopted by Gessner.

The oldest figure of it with which I am acquainted, but which is indeed very defective, is in the unfortunate Servetus's edition of Ptolemy, 1535, fol. tab. 28. It is there given as brought from the eastern coast of *Terra Firma*, with this inscription: "Reperitur hic animal habens reservaculum quo suos pullos secum portat, et eos non nisi lactandi tempore emittit. Tale regi Hispanie Granate oblatum est."

The first tolerable figure was given by Nierenberg, p. 156, if we except the woolly hair and the hind feet, which are entirely misrepresented.

The third Generation of the Porcupine Man.

The well-known astronomer J. Machin gave in the Philosophical Transactions for 1732† the first account of a boy of 14 years of age, afterwards called the *porcupine man*, whose whole skin, the head, the palms of the hand, and the soles of the feet excepted, was covered with corneous pegs, which gave the body an appearance as if covered with a coat of mail. He was not born with this cuticular deformity, which first made its appearance seven or eight weeks after birth, at which period the skin became yellow, and gradually continued to grow darker, till at length it became black, and soon after thicker and more corneous.

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* This very scarce *editio princeps* seems to be unknown to modern bibliographers best acquainted with the literature of voyages and travels. I obtained my copy through the kindness of Sir Joseph Banks.

† Vol. xxxvii. p. 299.

In his fiftieth year this man, who was now married and a father, exhibited himself in London, together with his son, who had the same deformity of skin. The celebrated Baker, who wrote on the microscope, gave at that time in the Philosophical Transactions* an appendix to M. Machin's paper; and as the latter had given a representation of the hand of the father, the former gave a figure of that of the son from a drawing, an engraving of which may be seen also in Edwards's Gleanings of Natural History, p. 1, tab. 212.

This son afterwards married; and in the month of September 1801 I saw two of his sons perfectly like their father and grandfather, and consequently the third generation of this family so singular on account of this cuticular deformity.

The oldest was twenty-two years of age and married, the younger was fourteen. Both were stout, well made, and of an athletic constitution. The older was a good pugilist like his grandfather, who is said to have excelled in this gymnastic art. His face, the palms of the hands, and the soles of the feet were of the usual appearance, but seemed to me to be uncommonly red. The skin of the remaining parts of the body was covered with corneous excrescences, or pegs of greater or less size, and of a more or less horny nature. The longest, strongest, and hardest, were on the fore arm and thighs; the finest were on some parts of the lower belly. They were in general smaller on the younger brother, and in many places, such as the breast, soft. The largest excrescences were from four to five lines in length, and of an irregular prismatic form, with blunt edges, almost as if pressed flat. The thickest were about three lines in diameter; at the extremities in general split, and many of them diverging like a fork. On the other hand, I scarcely observed one of them of that cylindric form ascribed to them by Baker, who besides supposed them to be hollow; at least such was the opinion of Haller, who considered this as a confirmation of Boerhaave's opinion in regard to the construction of the epidermis, as he says: "In hoc puero tota superficies corporis abiit in congeriem tubulorum exstantium, callosorum, subinde renascentium, quod certe exemplum quasi de industria ad conformandam præceptoris sententiam factum est." Boerhaave says expressly of the epidermis: "Constat vasorum exhalantium et inhalantium innumerabilium extremis annulis, inter se connatis."

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* Vol. xlix. part i. p. 21.

Where the excrescences were longest and thickest, they appeared to me to be like those which I have seen in the elephant under the forehead and above the trunk.

The colour of them in general appeared to be a chesnut or coffee brown. This however was the case at the surface, for in other parts the larger ones were rather yellowish gray.

The hair of the skin appeared sometimes as if grown into the horny substance of these excrescences.

Both the brothers, as well as the father and grandfather, had had the smallpox, in the last stage of which they lost the greater part of their excrescences; but they were soon gradually reproduced. In general they drop off singly from time to time, especially in winter; but new ones gradually grow up. When they are in any manner torn off, the skin which lies under them readily begins to bleed.

The skin on the top of the head before, and especially in the oldest, forms a kind of broad callosity, which has some resemblance to the *tofis* of the camel.

The perspiration of these two brothers exhibits nothing uncommon, no perceptible smell, &c. and during great heats or violent exercise they sweat like other men.

I am acquainted with only two cases which have a real analogy to that of the porcupine men from Suffolk. The one is the boy from Biseglia, of whom Stalp van der Wiel has given a figure and some account, in his Observations*: the other is a female child, three years of age, at Vienna, whose history and an account of the cure have been published by J. A. von Brambillas†. In both the face was free from these excrescences, but the palms of the hands and the soles of the feet were the most covered with them. An observation made in regard to the boy corresponds exactly with a circumstance related of the porcupine man: "Delapsis veteribus, novæ illico succedebant squamæ, quibus avulsis mox effluebat sanguis:" and the case is the same with what Brambilla says of the girl: "she was born with a smooth and somewhat yellow skin, but in six weeks it became brown, and in the course of a year black and bristly." The last-mentioned child was freed from its bristly warts by the continued use of bathing and mercurials; and we are told by Baker that the first porcupine man twice employed salivation to cleanse his skin; that by these means the excrescences dropped off, and that the skin continued for some time as white and smooth as that of other people; but that soon after the cure it became covered with these horny excrescences as before.

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* Observat. part ii. p. 374.

† Abhandlungen der Josephinischen medicinisch-chirurgischen Akad. vol. i. p. 371.

Other instances of singular deformities in the skin are mentioned by Fabricius, Hildanus, Fourcroy, &c., but these are so different from that here alluded to, that they cannot be placed in the same class.

XII. *A Survey and Report of the Coasts and Central Highlands of Scotland; made by the Command of the Right Honourable the Lords Commissioners of His Majesty's Treasury in the Autumn of* 1802. *By* THOMAS TELFORD, *Civil Engineer, Edinburgh, F. R. S.*

[Continued from vol. xv, p. 311.]

The Fisheries.

IN what regards the fisheries, I beg leave to quote a passage from my last year's Report. "I believe it is generally admitted, that in the improvement of a country, the interference of government should extend only to the removing obstacles, and affording conveniences which are of a nature not easily to be surmounted by individuals, or any body of men who can be brought to act together; and where it is evident that by removing those obstacles and affording these conveniences, the exertions of individuals will be greatly facilitated, so as to promote the general good of the empire."

The objects connected with the Fisheries, which seem to come under the foregoing description, are, 1st, the want of a ready communication by water between the east and west coasts; 2dly, the want of communications by land from the low countries and the east coast, with the shores and fishing lochs of the west coast; 3dly, the inconveniences arising from the operation of the salt laws; and 4thly, the want of a harbour in Caithness.

The first and second of those objects have already been fully discussed under the heads of the Caledonian Canal, and the proposed Bridges and Roads. The third has been so often and thoroughly investigated, that I shall only in this place take the liberty of mentioning that all the information I have certe exemplum quasi de industria ad conformandam præceptoris sententiam factum est." Boerhaave says expressly of the epidermis: "Constat vasorum exhalantium et inhalantium innumerabilium extremis annulis, inter se connatis."

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I have received tends to confirm the justice of the complaints against the laws now in force which regard salt.

As to the fourth object, the harbour of Wick in Caithness, which I examined, estimated, and reported upon last year, will remove the just complaints of want of protection on the N. E. coast.

The Emigrations.

That emigrations have already taken place from various parts of the Highlands, is a fact upon which there does not remain room to doubt: from the best information I have been able to procure, about three thousand persons went away in the course of last year; and, if I am rightly informed, three times that number are preparing to leave the country in the present year.

I shall not encroach upon your lordships' time by investigating all the remote or unimportant collateral causes of emigration, but shall proceed to that which I consider to be the most powerful in its present operation; and that is, converting large districts of the country into extensive sheepwalks. This not only requires much fewer people to manage the same tract of country, but in general an entirely new people, who have been accustomed to this mode of life, are brought from the southern parts of Scotland.

The difference of rents to the landlords between sheep and black cattle is, I understand, at least three to one, and yet on account of the extraordinary rise in the prices of sheep and wool, the sheep farmers have of late years been acquiring wealth. As the introducing sheep farms over countries heretofore stocked with black cattle creates an extensive demand for the young sheep from the established farms, it is possible that the high prices may continue until a considerable portion of the country is fully stocked: after this takes place, the quantities of sheep produced will bear a very great proportion to the demand, and then it is possible the prices may fall below the average value: in this case it is probable the farms will be subdivided, and a proportion of black cattle and cultivation be introduced in the lower grounds in the valleys, while the upper parts of the hills continue to be pastured with sheep. This I consider as the most improved state of Highland farming, and is consistent with a very considerable population: a beautiful instance of this is to be seen along the north side of Loch Tay. But improved communications, by means of roads and bridges, are necessary for this state of society; and for this reason I have said, that if these conveniences had been sooner introduced into the Highlands, it is possible this emigration might not have taken place, at least to the present extent.

The very high price of black cattle has also facilitated the means of emigration, as it has furnished the old farmers with a portion of capital which enables them to transport their families beyond the Atlantic.

In some few cases a greater population than the land can support in any shape, has been the cause of emigrations; such was the island of Tiree.

Some have, no doubt, been deluded by accounts sent back from others gone before them; and many deceived by artful persons, who hesitate not to sacrifice these poor ignorant people to selfish ends.

A very principal reason must also be, that the people, when turned out of their black cattle farms to make way for the sheep farmers, see no mode of employment whereby they can earn a subsistence in their own country; and sooner than seek it in the Low Lands of Scotland, or in England, they will believe what is told them may be done in the farming line in America.

What I have here mentioned appear to me to be the immediate causes of the present emigrations from the north-western parts of Scotland. To point out the means of preventing emigrations in future, is a part of my duty, upon which I enter with no small degree of hesitation. As the evil at present seems to arise chiefly from the conduct of land-owners, in changing the œconomy of their estates, it may be questioned whether government can with justice interfere, or whether any essential benefits are likely to arise from this interference.

In one point of view it may be stated, that, taking the mountainous parts of Scotland as a district of the British empire, it is the interest of the empire that this district be made to produce as much human food as it is capable of doing at the least possible expense; that this may be done by stocking it chiefly with sheep; that it is the interest of the empire the food so produced should not be consumed by persons residing amongst the mountains totally unemployed, but rather in some other parts of the country, where their labour can be made productive either in the business of agriculture, fisheries or manufactures; and that by suffering every person to pursue what appears to them to be their own interest, that although some temporary inconveniences may arise, yet, upon the whole, that matters will in the

Brought over	£. 15,400
8. To a wooden pier or wharf 160 yards long, and 15 feet wide at the Cavin, in Loch Ryan, for embarking and disembarking troops, and for promoting the commerce of this part of the country	1,500
Total -	£. 16,900

London,
March 15, 1803.

THOMAS TELFORD.

General Statement of Funds necessary for the several Works mentioned in the foregoing Report.

Roads and Bridges.	For 1st three years public aid, annually -	£. 20,000	- 60,000	£. 192,000
	Do. land-owners, &c.	20,000	- 60,000	
	For 2d three years public aid, annually -	12,000	- 36,000	
	Do. land-owners -	12,000	- 36,000	
Naval Stations.	Aberdeen, five years public aid, annually	5,500	- 27,500	55,000
	Aberdeen, city funds	5,500	- 27,500	
	Cromarty, two years	2,500	- - -	5,000
Caledonian Canal.	Seven years public aid, annually -	50,000	- - -	350,000
Fisheries.	Wick, three years public aid, ann.	2,000	- - -	6,000
Carlisle to Port Patrick.	Three years public aid, annually	5,650	- - -	16,950
				£. 624,950

London,
March 15, 1803.

THOMAS TELFORD.

Statement of the Annual and Total Sums to be advanced, beyond the Proportion to be contributed by the Landowners.

1st Year.	Roads and bridges - -	£. 20,000	
	Aberdeen - - - - - -	5,500	
	Cromarty - - - - - -	2,500	
	Caledonian canal - - -	50,000	
	Wick - - - - - - -	2,000	
	Carlisle to Port Patrick -	5,650	
			85,650

		Brought over £.	85,650
2d Year.	Roads and bridges - - -	20,000	
	Aberdeen - - - - - -	5,500	
	Cromarty - - - - - -	2,500	
	Caledonian canal - - -	50,000	
	Wick - - - - - - -	2,000	
	Carlisle to Port Patrick -	5,650	
			85,650
3d Year.	Roads and bridges - - -	20,000	
	Aberdeen - - - - - -	5,500	
	Caledonian canal - - -	50,000	
	Wick - - - - - - -	2,000	
	Carlisle to Port Patrick -	5,650	
			83,150
4th Year.	Roads and bridges - - -	12,000	
	Aberdeen - - - - - -	5,500	
	Caledonian canal - - -	50,000	
			67,500
5th Year.	Roads and bridges - - -	12,000	
	Aberdeen - - - - - -	5,500	
	Caledonian canal - - -	50,000	
			67,500
6th Year.	Roads and bridges - - -	12,000	
	Caledonian canal - - -	50,000	
			62,000
7th Year.	Caledonian canal - - -	50,000	50,000
			£.501,450

London,
March 15, 1803.

THOMAS TELFORD.

[To be concluded in our next.]

XIII. *On the Theory of Combustion.*

To the Editor of the Philosophical Magazine.

SIR, Edinburgh, May 20, 1803.

As you have admitted Mr. Henry's claim in favour of his father's discovery, I trust you will do equal justice to an author, in whose cause I presume, though unauthorized, to appear. In your last number is a communication from Mr. Portal on the subject of combustion: he there mentions the opinion, that heat and light are different bodies, simultaneously separated in this process, without their having any peculiar connection. This system he attributes to Dr. Thomson, author of a very excellent article on chemistry in the Supplement to the Encyclopædia Britannica, and of a separate [illegible] work on the same subject. [illegible] that the [illegible] was before published [illegible] Chemistry. [illegible] the latter work in England, [illegible] that shorter and [illegible] has been translated, and [illegible] was published in a work [illegible] Gentleman at Exeter;" [illegible] philosophical subjects, [illegible] by what authority I am [illegible] the peculiar and di- [illegible] heat are pointed out. [illegible] they are distinct bodies, [illegible] of being related, they [illegible] repelling each other. [illegible] forms an ingredient in [illegible] chemical combination, [illegible] separation takes place with- [illegible]

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T. R. O.

[illegible] Observations on the Processes of Tanning.

By Mr. Davy*.

1. On the Preparation of Skins for Tanning.

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When lime is employed, it has been generally supposed that it acts by destroying the epidermis, so as to render it soluble in water. This, however, does not appear to be the case: I exposed to two ounces of lime water four grains of epidermis, separated from cow skin, and which had been freed from loose moisture by blotting paper; but, after five days, it appeared rather of larger volume than before; and instead of having lost any weight, I found that it had gained very nearly half a grain.

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In examining the circumstance of the action of lime water, and of milk of lime, upon skin, I have always observed that the cuticle is rendered extremely loose and friable after this action: from which it is probably that it combines with the lime, so as to form an insoluble compound. This may be observed indeed in washing the hands with lime water: the cuticle becomes extremely rough and dry; whereas, after the action of weak alkaline solutions, which form soluble compounds with it, it is found smooth.

Not only the epidermis, but likewise the soft matter at the extremity of the hair, is acted upon by lime; and this effect must tend considerably to facilitate the process of depilation. Likewise the fat and oily matter adhering to the skin form saponaceous compounds with the earth, and these compounds are removed with other extraneous matter, before the skins are submitted to any new chemical agents.

It has been proposed to use the residuum of the tanning lixivium, or the exhausted ooze, for the purposes of depilation; but this liquor seems to contain no substances capable of acting upon the epidermis, or of loosening the hair; and when skin is depilated by being exposed to it, the effect must really be owing to incipient putrefaction.

Skins, after being depilated and cleansed, are in this country generally subjected to other processes of preparation before they are impregnated with the tanning principle.

The large and thick hides which have undergone incipient putrefaction are introduced for a short time into a strong infusion of bark, when they are said by manufacturers to be *coloured;* and after this they are acted upon by water impregnated with a little sulphuric acid, or acetous acid formed by the fermentation of barley or rye. In this case they become harder and denser than before, and fitted, after being tanned, for the purpose of forming the stouter kinds of sole leather. The acids are capable of combining both with skin and with tannin: and it would appear that, in this process, a triple combination must be effected on the surface of the skin, though from theory one should be disposed to conclude, that the interior part could be little modified in consequence of the colouring, and the action of the acids.

The light skins of cows, the skins of calves, and all smaller skins, are treated in a very different way, being submitted for some days to the action of a lixivium, called the grainer, made by the infusion of pigeons dung in water. After this operation they are found thinner and softer than before, and more proper for producing flexible leather. When the infusion of pigeons dung is examined, after being freshly made, it is found to contain a little carbonate of ammonia; but in a short time it undergoes fermentation, when carbonic acid and hydrocarbonate are given out by it, and a small quantity of acetous acid formed. The alkali in the grainer may probably have some action upon the skin; it may be supposed to free it from any oils or calcareous soap that remained adhering to it: but the great effect probably depends upon the complicated process of fermentation, during which the skin loses its elasticity, and becomes soft; and it is found by tanners, that dung which has undergone fermentation is wholly unfit for their use.

I have tried several experiments on different substances, as substitutes for the pigeons dung used in the grainer, but without gaining successful results. Very weak solutions of carbonate of potash and carbonate of ammonia seemed to soften considerably small pieces of skin that had been depilated by lime; but when they were tried by Mr. Purkis, in the processes of manufacture, the effects were less distinct. In the western counties of England the excrement of dogs is employed instead of pigeons dung, and culver or the dung of fowls is in common use. The dung of graminivorous quadrupeds enters only slowly into fermentation, and it is not found efficacious in the process.

XV. *Notices respecting New Books.*

Connoissance des Tems, à l'Usage des Astronomes et des Navigateurs pour l'An 13 *de l'Ère de la République Françoise* (1805) *; publiée par le Bureau des Longitudes à Paris. De l'Imprimerie de la République.*

THIS work was published for the first time in 1679. Picard, one of the greatest astronomers of the 17th century, and Lefebvre, edited the earlier volumes; Lieutaud began in 1702; Godin in 1730; Miraldi in 1735; Lalande in 1760; Jeaurat in 1776; Mechain in 1788: in consequence of the absence of Mechain, Lalande resumed the editorship in 1795, and has continued ever since.

This work is divided into two parts: the first comprehends the calendar, that is to say, every thing relating to observations both at sea and at land; a catalogue of 600 principal stars for the 1st of January 1805, corrected by Michel Lefrançais de Lalande. A geographical table of longitudes and latitudes, corrected and enlarged by Buache, Mechain, and Lalande, according to the last voyages and observations which the French Board of Longitude receives from different countries. The six eclipses which will take place in the above year are carefully calculated, as well as eclipses of the stars.

The second part of the volume contains observations which render this work of more general utility to astronomers. Among the articles are a history and observations of the new planets and the last comets; a new catalogue of the stars hitherto known, and amounting to 13000, extracted from the 50,000 stars observed by Lalande the uncle and nephew; memoirs or observations by Von Zach and Ciccolini, Delambre, Messier, Mechain, Vidal, Flauguergues, Cousin, Lalande uncle and nephew, Burckhardt, Nouet, Chabrol de Murol, Thulis, &c. Also the history of Astronomy for the years 8 and 9, being a continuation of that published by Lalande for the preceding years, since 1782. The most curious articles in this volume are the observations of the new planet discovered by Dr. Olbers, to whom the National Institute has decreed the prize medal founded by Lalande for the best annual work on astronomy.

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been sooner introduced into the Highlands, it is possible this emigration might not have taken place, at least to the present extent.

The very high price of black cattle has also facilitated the means of emigration, as it has furnished the old farmers with a portion of capital which enables them to transport their families beyond the Atlantic.

In some few cases a greater population than the land can support in any shape, has been the cause of emigrations; such was the island of Tiree.

Some have, no doubt, been deluded by accounts sent back from others gone before them; and many deceived by artful persons, who hesitate not to sacrifice these poor ignorant people to selfish ends.

A very principal reason must also be, that the people, when turned out of their black cattle farms to make way for the sheep farmers, see no mode of employment whereby they can earn a subsistence in their own country; and sooner than seek it in the Low Lands of Scotland, or in England, they will believe what is told them may be done in the farming line in America.

What I have here mentioned appear to me to be the immediate causes of the present emigrations from the north-western parts of Scotland. To point out the means of preventing emigrations in future, is a part of my duty, upon which I enter with no small degree of hesitation. As the evil at present seems to arise chiefly from the conduct of land-owners, in changing the œconomy of their estates, it may be questioned whether government can with justice interfere, or whether any essential benefits are likely to arise from this interference.

In one point of view it may be stated, that, taking the mountainous parts of Scotland as a district of the British empire, it is the interest of the empire that this district be made to produce as much human food as it is capable of doing at the least possible expense; that this may be done by stocking it chiefly with sheep; that it is the interest of the empire the food so produced should not be consumed by persons residing amongst the mountains totally unemployed, but rather in some other parts of the country, where their labour can be made productive either in the business of agriculture, fisheries or manufactures; and that by suffering every person to pursue what appears to them to be their own interest, that although some temporary inconveniences may arise, yet, upon the whole, that matters will in the

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called *coodum nagoo*) was made to bite the *coultiah* on the belly: both fangs visibly acted; blood appeared on the wound, but no other consequence followed. A *tar'tutta* bitten immediately after in the same manner, died within two hours.

Chickens and pigeons bitten by a cobra de capello, whose fangs had been eradicated, suffered no symptoms of poison; but when poison taken from the same snake was inserted into their bodies, either by incision or puncture, they suffered the usual symptoms, and very often died.

Mr. Jameson, author of the Mineralogy of the Scottish Isles, is about to put to the press a work entitled "A Natural History of Fossils, according to the System of the celebrated Professor Werner, of Freyberg." As Mr. Jameson studied two years under this illustrious naturalist, he hopes to be able to present to the public a work free from the errors and imperfections of Wiëdemann, Emmerling, and Brochaut.

XVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

The following is a short account of experiments on the absorption of different gases by water, at different temperatures, and under different pressures, made by Mr. William Henry, and communicated to the Society. The processes were carried on by means of instruments invented by the author; plates and descriptions of which are annexed to the account of the experiments.

Mr. Cavendish, in pursuing his first experiments in pneumatic chemistry, ascertained many of the circumstances of the absorption of carbonic acid gas by water; and Dr. Priestley had noticed the relations of this absorption to pressure: more lately, the manufacture of artificial gaseous waters has called the attention of many chemists to this part of the subject; but the power of combination of other aëriform fluids with water, and the manner in which this power is modified by different causes, have been but little studied.

Mr. Henry, in the first part of the detail of his experiments, states, that the quantity of carbonic acid gas absorbed by water, is influenced materially by the quantity of common air, which may be either combined with the water, or mixed with the gas; and the effect is in some measure proportional to the quantity of the residue, the absorption being always greatest when a large quantity of gas was agitated with a comparatively small quantity of water.

From various trials made with great care, Mr. Henry concludes, that, in judging of the influence of temperature, the experiments should be made on equal proportions of gas, and of water, and that in this case, with regard to carbonic acid gas, 1-fourteenth of the whole bulk, absorbable at 55°, is the diminution of the quantity of absorption produced by each elevation of 10° of Fahrenheit.

Of sulphuretted hydrogen gas, 100 parts of water, at 55°, absorb 86 parts, and at 85° 78 parts.

Of nitrous oxide gas, 100 cubic inches of water, at 55°, take up 50, and at 70° only 44.

The experiments on those gases which are absorbed only in small proportions by water the author could not conveniently make at more than one temperature.

He found that at 60°, 100 cubic inches of water absorb, of nitrous gas, 5 cubic inches; of oxygen, 2·63; of phosphuretted hydrogen, 2·14; of gaseous oxide of carbon, 2·01; of carburetted hydrogen gas, 1·40; of nitrogen gas, 1·20; of hydrogen gas, 1·08.

Mr. Henry mentions that during the absorption of large quantities of carbonic acid, sulphuretted hydrogen and nitrous oxide by water, an increase of temperature of about 3-fourths of a degree is perceived.

From the results of a great variety of experiments made in the more absorbable gases, and on oxygen and nitrogen gases, the author draws the following general conclusion with regard to pressure:

That under the same circumstance of temperature, water takes up the same volume of gas, whether it be condensed, or under ordinary pressure; but, as the spaces occupied by gases are inversely as the weights compressing them, it follows, that water takes up of gas condensed, by one, two, or three additional atmospheres, a quantity which is equal to twice, thrice, or four times the quantity taken up under the ordinary pressure. D.

GALVANIC SOCIETY, PARIS.

On the 28th of May the Galvanic Society made, for the second time, some experiments on a large scale at the veterinary school of Alfort.

Animals of all sizes, from the insect to the horse, were subjected to an apparatus composed of more than 2000 disks, formed into piles which communicated with each other. Besides other results, the following were observed:

1st, That a spark cannot be obtained, as in common electricity, at an explosive distance, but at the point of contact.

2d, That Coulomb's electrometric balance does not indicate electric tension in the ratio of the number of pairs of disks, or of the strength of the pile.

3d, That the power of a very formidable apparatus is required to kill a small animal.

4th, That Galvanism, applied after death, can determine movements of inspiration and expiration. If a lighted taper be placed near a small aperture made in the trachea, the flame at the moment when the Galvanism is applied is drawn into the breast by the impulse of the air passing into the lungs, and extinguished when the air issues.

5th, That contractions can be produced in the head and forehead of a large animal—a horse, for example—by placing it at a very great distance, and making it communicate with the pile by one conductor, while the other is formed by the common reservoir*.

XVII. *Intelligence and Miscellaneous Articles.*

A DEFENCE AGAINST FIRE.

PROFESSOR PALMER, of Hamburgh, has lately discovered a means by which all inflammable matters, such as wood, paper, linen, &c., can not only be secured from burning, but also be speedily extinguished when on fire. These means consist in a powder composed of one ounce of sulphur, one ounce red ochre, and six ounces of copperas water. To render wood incombustible, it is first daubed over with cabinet-maker's glue, after which the powder is strewed over it: and this operation, when the wood becomes dry, is three or four times repeated. When the powder is to be applied to linen or paper, plain water is employed in the room of glue: in other respects the process is the same, with this difference alone, that the operation is performed only once or twice. When the powder is used for articles already on fire, two ounces are sufficient to extinguish a square foot of surface. Professor Palmer intends to publish ing any peculiar connection. This system he attributes to Dr. Thomson, author of a very excellent article on chemistry, in the Supplement to the Encyclopædia Britannica, and of a separate elementary work on the same subject. You remark in a note that this opinion was before published in Gren's Chemistry.

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* This sentence is ambiguous. We have translated it literally, however, from the French Journal in which it appeared.—EDIT.

Long before the appearance of the latter work in England, and I believe before the publication of that shorter and more comprehensive system, which has been translated, and which you quote, the same opinion was published in a work entitled "Essays by a Society of Gentlemen at Exeter;" and those essays, which relate to philosophical subjects, were reprinted in this part of Britain, by what authority I am not informed. In one of these essays the peculiar and distinguishing properties of light and heat are pointed out. The author attempts to show that they are distinct bodies, by a variety of facts, and that, instead of being related, they are apparently antagonizing principles, repelling each other. He traces the principle of light, as it forms an ingredient in many different bodies, not only in chemical combination, but in a looser union, where its separation takes place without decomposition.

If this opinion be then established, that author must have the prior claim; and it is remarkable that, in the Encyclopædia, it is attributed to Dr. Parr of Exeter, though the name is omitted in Dr. Thomson's separate work. But with this conjecture I have no business: my wish is only to state the circumstances.

As I have paid some attention to this subject, I may trouble you with some further remarks on it, should you approve of the communication. I will now only intrude further, by requesting a place for this letter in your next; and am your sincere friend and well wisher,

T. R. O.

XIV. *Observations on the Processes of Tanning.*

By Mr. Davy[*].

I. *On the Preparation of Skin for Tanning.*

In all the processes for forming leather, the skins are depilated, and freed from flesh and extraneous matter before they are submitted to the action of the tanning lixivium. In some cases, when large skins are employed, a slight degree of putrefaction is induced, for the purpose of enabling the hair to be readily separated; but in general this effect is produced by a mixture of lime and water.

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Not only the epidermis, but likewise the soft matter at the extremity of the hair, is acted upon by lime; and this effect must tend considerably to facilitate the process of depilation. Likewise the fat and oily matter adhering to the skin form saponaceous compounds with the earth, and these compounds are removed with other extraneous matter, before the skins are submitted to any new chemical agents.

It has been proposed to use the residuum of the tanning lixivium, or the exhausted ooze, for the purposes of depilation; but this liquor seems to contain no substances capable of acting upon the epidermis, or of loosening the hair; and when skin is depilated by being exposed to it, the effect must really be owing to incipient putrefaction.

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I have tried several experiments on different substances, as substitutes for the pigeons dung used in the grainer, but without gaining successful results. Very weak solutions of carbonate of potash and carbonate of ammonia seemed to soften considerably small pieces of skin that had been depilated by lime; but when they were tried by Mr. Purkis, in the processes of manufacture, the effects were less distinct. In the western counties of England the excrement of dogs

This formidable reptile has obtained its Portuguese title of *cobra de capello*, or *hooded snake*, from the appearance which it presents when viewed in front in an irritated state, or when preparing to bite; at which time it bends the head rather downwards, and seems hooded, as it were, in some degree, by the expanded skin of the neck. In India it is every where exhibited publicly as a show, and is, of course, more universally known in that country than almost any other of the race of reptiles. It is carried about in a covered basket, and so managed by its proprietors as to assume, when exhibited, a kind of dancing motion; raising itself up on its lower part, and alternately moving its head and body from side to side for some minutes, to the sound of some musical instrument which is played during the time. The Indian jugglers, who thus exhibit the animal, first deprive it of its fangs; by which means they are secured from the danger of its bite.

Dr. Russel, in his account of experiments made in India with this serpent, observes, that, as a general standard for a comparison of the effect of its bite with that of other poisonous serpents, he never knew it prove mortal to a dog in less than twenty-seven minutes, and to a chicken in less than half a minute. Thus, fatal as it is, its poison seems not so speedy in operation as that of the rattle-snake, which has been known to kill a dog in the space of two minutes.

In the month of June 1787, a dog bitten by a cobra de capello on the inside of the thigh howled at first as if in severe pain: after two or three minutes he lay down, continuing to howl and moan: after twenty minutes he rose, but with much difficulty, being unable to walk, and his whole frame appeared greatly disordered. He soon lay down again, and in a few minutes was seized with convulsions, in which he expired twenty-seven minutes after the bite.

A large and very stout dog was bitten by another cobra de capello on the inside of the thigh, which in a minute or two was drawn up; which is, in general, the first symptom of the poison having taken effect. He continued, however, nearly an hour longer, walking on the three remaining legs, seeming not otherwise disordered; but after this time he laid himself along in great inquietude, his head and throat being convulsed in an uncommon degree; he made several vain efforts to rise; his legs became both paralytic, and after continuing in this state near an hour he expired.

Nov. 11, a large dog was bitten by a cobra de capello which had been captive only two days. He complained a good deal at the instant of the bite, and the leg was drawn

General Zoology, or Systematic Natural History, by George Shaw, *M.D. F.R.S. &c. with Plates from the first Authorities, and most select Specimens engraved principally by Mr. Heath.* Vol. III. Parts 1 and 2. Kearsley, 1802.

Having already noticed the two preceding volumes of this useful work, we shall only observe, that the one now announced seems to be executed with the same care and attention, and that the plates are engraved in the same style of excellence. The two parts of this volume contain amphibia; comprehending tortoises, frogs, toads, lizards, and serpents. As a specimen of the work, we subjoin the following account of the Coluber Naja:

The coluber naja, or cobra de capello, is a native of India, where it appears to be one of the most common, as well as most noxious, of the serpent tribe; very frequently proving fatal, in the space of a few minutes, to those who unfortunately experience its bite. Its remarkable form and colours are such as to distinguish it with great ease from almost every other snake. Its general length seems to be three or four feet, and the diameter of the body about an inch and quarter: the head is rather small than large, and is covered on the fore part with large smooth scales, resembling in this respect the majority of innoxious serpents; the back part, sides, and neck, with smaller ovate scales; and the remainder of the animal, on the upper parts, with small, distinct, oblong-oval scales, not ill resembling the general form of a grain of rice. At a small distance beyond the head is a lateral swelling or dilatation of the skin, which is continued to the distance of about four inches downwards, where the outline gradually sinks into the cylindric form of the rest of the body. This part is extensile, at the pleasure of the animal; and when viewed from above, while in its most extended state, is of a somewhat cordated form, or wider at the upper than the lower part: it is marked above by a very large and conspicuous patch or spot, greatly resembling the figure of a pair of spectacles; the mark itself being white with black edges, and the middle of each of the rounded parts black. This mark is more or less distinct in different individuals, and also varies occasionally in size and form, and in some is even altogether wanting. The usual colour of the animal is a pale ferruginous brown above; the under parts being of a blueish white, sometimes slightly tinged with pale brown, or yellow: the tail, which is of moderate length, tapers gradually, and terminates in a slender, sharp-pointed extremity.

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Aug. 9, a cobra de capello which had lost his two longest fangs, but retained two of the second order, was made to bite a very large stout dog. At first the dog complained loudly, though without drawing up the thigh, or showing any other symptom of poison: but, happening at this time to break loose, he was pursued, and, after a chase of an hour and a half, was brought back, much fatigued and heated. After resting a quarter of an hour, water was offered to him, which was refused, though he ate some morsels of bread thrown into it. About a quarter of an hour afterwards he became much disturbed, grew entirely outrageous, howling violently, snapping at and gnawing the stake to which he was tied with incredible ferocity. This continued about three hours, when, growing faint, his howlings grew weaker, his convulsions increased, and he expired in about four hours after the bite.

A pig bitten by a snake of this kind, which had been kept for more than six weeks, and fed only once in seven days with milk, became greatly disordered in twenty minutes, and expired in less than an hour.

A chicken bitten by a cobra de capello has been sometimes known to survive two hours.

Aug. 17, 1788, an attempt was made to make a cobra de capello bite another (of the variety called *nooni paragoodo*) in the tail; but that part being found too small, the belly was bitten, a little above the vent. The bitten snake soon lost its former activity, and, when put under a glass, coiled itself up. In this state it was left, and after an hour and a quarter was found dead. On opening the belly, the parts immediately beneath the bite appeared much inflamed, though it could not be discovered whether the fangs had penetrated into the cavity.

A cobra de capello received by Dr. Russel from Ganjam, under the name of *saltanag*, was made to bite another remarkably large cobra brought from the same place under the name of *coultiah*. The poison was shed on the place, but no marks of fangs could be perceived, and the *coultiah* remained as well as before: this experiment was repeated with the same result, though a little blood as well as poison was found on the part bitten.

Some days after this, a cobra de capello (of the variety called *coodum nagoo*) was made to bite the *coultiah* on the belly: both fangs visibly acted; blood appeared on the wound, but no other consequence followed. A *tar'tutta* bitten immediately after in the same manner, died within two hours.

Chickens and pigeons bitten by a cobra de capello, whose fangs had been eradicated, suffered no symptoms of poison; but when poison taken from the same snake was inserted into their bodies, either by incision or puncture, they suffered the usual symptoms, and very often died.

Mr. Jameson, author of the Mineralogy of the Scottish Isles, is about to put to the press a work entitled "A Natural History of Fossils, according to the System of the celebrated Professor Werner, of Freyberg." As Mr. Jameson studied two years under this illustrious naturalist, he hopes to be able to present to the public a work free from the errors and imperfections of Wiëdemann, Emmerling, and Brochaut.

XVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

The following is a short account of experiments on the absorption of different gases by water, at different temperatures, and under different pressures, made by Mr. William Henry, and communicated to the Society. The processes were carried on by means of instruments invented by the author; plates and descriptions of which are annexed to the account of the experiments.

Mr. Cavendish, in pursuing his first experiments in pneumatic chemistry, ascertained many of the circumstances of the absorption of carbonic acid gas by water; and Dr. Priestley had noticed the relations of this absorption to pressure: more lately, the manufacture of artificial gaseous waters has called the attention of many chemists to this part of the subject; but the power of combination of other aëriform fluids with water, and the manner in which this power is modified by different causes, have been but little studied.

Mr. Henry, in the first part of the detail of his experiments, states, that the quantity of carbonic acid gas absorbed by water, is influenced materially by the quantity of common air, which may be either combined with the water, or mixed with the gas; and the effect is in some measure proportional to the quantity of the residue, the absorption being always greatest when a large quantity of gas was agitated with a comparatively small quantity of water.

From various trials made with great care, Mr. Henry concludes, that, in judging of the influence of temperature, the experiments should be made on equal proportions of gas, and of water, and that in this case, with regard to carbonic acid gas, 1-fourteenth of the whole bulk, absorbable at 55°, is the diminution of the quantity of absorption produced by each elevation of 10° of Fahrenheit.

Of sulphuretted hydrogen gas, 100 parts of water, at 55°, absorb 86 parts, and at 85° 78 parts.

Of nitrous oxide gas, 100 cubic inches of water, at 55°, take up 50, and at 70° only 44.

The experiments on those gases which are absorbed only in small proportions by water the author could not conveniently make at more than one temperature.

He found that at 60°, 100 cubic inches of water absorb, of nitrous gas, 5 cubic inches; of oxygen, 2·63; of phosphuretted hydrogen, 2·14; of gaseous oxide of carbon, 2·01; of carburetted hydrogen gas, 1·40; of nitrogen gas, 1·20; of hydrogen gas, 1·08.

Mr. Henry mentions that during the absorption of large quantities of carbonic acid, sulphuretted hydrogen and nitrous oxide by water, an increase of temperature of about 3-fourths of a degree is perceived.

From the results of a great variety of experiments made in the more absorbable gases, and on oxygen and nitrogen gases, the author draws the following general conclusion with regard to pressure:

That under the same circumstance of temperature, water takes up the same volume of gas, whether it be condensed, or under ordinary pressure; but, as the spaces occupied by gases are inversely as the weights compressing them, it follows, that water takes up of gas condensed, by one, two, or three additional atmospheres, a quantity which is equal to twice, thrice, or four times the quantity taken up under the ordinary pressure. D.

GALVANIC SOCIETY, PARIS.

On the 28th of May the Galvanic Society made, for the second time, some experiments on a large scale at the veterinary school of Alfort.

Animals of all sizes, from the insect to the horse, were subjected to an apparatus composed of more than 2000 disks, formed into piles which communicated with each other. Besides other results, the following were observed:

1st, That a spark cannot be obtained, as in common electricity, at an explosive distance, but at the point of contact.

2d, That Coulomb's electrometric balance does not indicate electric tension in the ratio of the number of pairs of disks, or of the strength of the pile.

3d, That the power of a very formidable apparatus is required to kill a small animal.

4th, That Galvanism, applied after death, can determine movements of inspiration and expiration. If a lighted taper be placed near a small aperture made in the trachea, the flame at the moment when the Galvanism is applied is drawn into the breast by the impulse of the air passing into the lungs, and extinguished when the air issues.

5th, That contractions can be produced in the head and forehead of a large animal—a horse, for example—by placing it at a very great distance, and making it communicate with the pile by one conductor, while the other is formed by the common reservoir*.

XVII. *Intelligence and Miscellaneous Articles.*

A DEFENCE AGAINST FIRE.

Professor Palmer, of Hamburgh, has lately discovered a means by which all inflammable matters, such as wood, paper, linen, &c., can not only be secured from burning, but also be speedily extinguished when on fire. These means consist in a powder composed of one ounce of sulphur, one ounce red ochre, and six ounces of copperas water. To render wood incombustible, it is first daubed over with cabinet-maker's glue, after which the powder is strewed over it: and this operation, when the wood becomes dry, is three or four times repeated. When the powder is to be applied to linen or paper, plain water is employed in the room of glue: in other respects the process is the same, with this difference alone, that the operation is performed only once or twice. When the powder is used for articles already on fire, two ounces are sufficient to extinguish a square foot of surface. Professor Palmer intends to publish

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* This sentence is ambiguous. We have translated it literally, however, from the French Journal in which it appeared.—Edit.

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Of the Stratus.

This modification has a mean degree of density.

It is the lowest of clouds, since its inferior surface commonly rests on the earth or water.

Contrary to the last, which may be considered as belonging to the day, this is properly the cloud of night; the time of its first appearance being about sun-set. It comprehends all those creeping mists which in calm evening ascend in spreading sheets (like an inundation of water) from the bottom of valleys and the surface of lakes, rivers, &c.

Its duration is frequently through the night.

On the return of the sun the level surface of this cloud begins to put on the appearance of cumulus, the whole at the same time separating from the ground. The continuity is next destroyed, and the cloud ascends and evaporates, or passes off with the appearance of the nascent cumulus.

This has been long experienced as a prognostic of fair weather*, and indeed there is none more serene than that which is ushered in by it. The relation of the stratus to the state of the atmosphere as indicated by the barometer, &c. appears notwithstanding to have passed hitherto without due attention.

Of the Cirro-cumulus.

The cirrus having continued for some time increasing or stationary, usually passes either to the cirro-cumulus or the cirro-stratus, at the same time descending to a lower station in the atmosphere.

The cirro-cumulus is formed from a cirrus, or from a number of small separate cirri, by the fibres collapsing as it were, and passing into small roundish masses, in which the texture of the cirrus is no longer discernible, although they still retain somewhat of the same relative arrangement. This change takes place either throughout the whole mass at once, or progressively from one extremity to the other. In either case, the same effect is produced on a number of adjacent cirri at the same time and in the same order. It appears in some instances to be accelerated by the approach of other clouds.

This modification forms a very beautiful sky, sometimes exhibiting numerous distinct beds of these small connected clouds, floating at different altitudes.

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* At nebulæ magis ima petunt, campoque recumbunt.
Virgil. Georg. lib. i.

The cirro-cumulus is frequent in summer, and is attendant on warm and dry weather. It is also occasionally and more sparingly seen in the intervals of showers, and in winter*. It may either evaporate, or pass to the cirrus or cirro-stratus.

Of the Cirro-stratus.

This cloud appears to result from the subsidence of the fibres of the cirrus to a horizontal position, at the same time that they approach towards each other laterally. The form and relative position, when seen in the distance, frequently give the idea of shoals of fish. Yet in this, as in other instances, the structure must be attended to rather than the form, which varies much, presenting at other times the appearance of parallel bars, interwoven streaks like the grain of polished wood, &c. It is always thickest in the middle, or at one extremity, and extenuated towards the edge. The distinct appearance of a cirrus does not always precede the production of this and the last modification.

The cirro-stratus precedes wind and rain, the near or distant approach of which may sometimes be estimated from its greater or less abundance and permanence. It is almost always to be seen in the intervals of storms. Sometimes this and the cirro-cumulus appear together in the sky, and even alternate with each other in the same cloud, when the different evolutions which ensue are a curious spectacle, and a judgment may be formed of the weather likely to ensue by observing which modification prevails at last. The cirro-stratus is the modification which most frequently and completely exhibits the phænomena of the solar and lunar halo, and (as supposed from a few observations) the parhelion and paraselene also. Hence the reason of the prognostic for foul weather, commonly drawn from the appearance of halo†.

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* The following passage is beautifully descriptive of the appearance of this modification by moonlight:

For yet above these wafted clouds are seen
(In a remoter sky, still more serene)
Others, detached in ranges through the air,
Spotless as snow, and countless as they're fair;
Scatter'd immensely wide from east to west,
The beauteous semblance of a flock at rest.
These to the raptur'd mind aloud proclaim
Their mighty shepherd's everlasting name.

Bloomfield's Farmer's Boy, Winter.

† The frequent appearance of halo in this cloud may be attributed to its possessing great extent, at such times, with little perpendicular depth, and

ANTIQUITIES.

A number of curious remains of antiquity arrived at Portsmouth in one of the transports from Egypt; they are the property of the earl of Cavan, and were put on board a vessel to be conveyed to his lordship's seat at Fawley: among them are the following:—A case containing mummies of an antient Egyptian family, viz. a male, female, and two children: the male measures five feet nine inches in height; and as the upper half of the body had been stript of the linen swathes, the flesh, the nails of the fingers, and even the features, can be seen very distinctly: the arms are bent upwards, crossing each other on the breast, the fingers of the right hand touching the left shoulder, and the left hand clenched as if holding something. The female measures five feet six inches in height, and the infant children about twenty-two inches. Mummies of an ichneumon, a dog, two hawks, two owls, and six ibises, some of them in covered urns of red earthen ware; another complete mummy, with the external case beautifully painted with hieroglyphics; a bust of Isis; a large frog in gray granite; a large slab of whitish granite, with hieroglyphics cut in bas relief; a broken sarcophagus in black granite, and many antique fragments of marble porphyries, jaspers, agates, and masses of the various rocks of Upper Egypt, which will be highly interesting to the mineralogist, as well as amusing to the antiquarian; a perfect sarcophagus of red granite; its inside dimensions are six feet six inches long, two feet four inches wide, and one foot six inches deep: a large column of red porphyry; also, a bowl of red granite, its outside dimensions near six feet; it is cut out of the base of a Corinthian column; the mouldings are very perfect, and the whole height of the column must have been about 54 feet.

MEASUREMENT OF A DEGREE OF THE MERIDIAN.

Astronomers long suspected that there was an error in the measurement of a degree of the earth, made in Lapland in 1736 by Maupertuis, Lemonnier, Outhier, and Celsius, because that degree was greater than it ought to be according to all the other measurements. M. Melanderhiolm has found means to repeat this measurement. He informs me in a letter that M. Svanberg and three other astronomers have found the degree to be 57209 toises in lat. 66° 20′, which makes 196 toises less than by the measurement of the French astronomers, and gives for the flattening of the earth a 213th part. This agrees better with the other comparisons, and shows us that the figure of the earth is not so irregular as was supposed from the northern degree.

M. Mechain set out on the 26th of April for Spain, where he intends to measure a triangle of 93,000 toises, terminating at the Balearian islands, and which will complete the grand and important measurement of the meridian which Mechain and Delambre made a few years ago. He is accompanied by M. Chevalier, Dezauche, and Mechain jun. This measurement will be very difficult: but no one is more capable than M. Mechain to overcome difficulties; and we shall have the 45th degree in the middle of the whole arc measured by the French. DE LALANDE.

HUMBOLDT'S TRAVELS.

The report some time ago circulated of the death of this celebrated traveller has been contradicted by some of the Berlin journals, which state, that letters have been lately received from him by some of his friends at Paris. These letters, which are dated at Lima, announce that he proposed to return to Europe about the month of September next.

NEW METAL.

Proust has discovered that the substance which he considered as a new metal, and to which he had given the name of *selene*, is nothing else than uranium.

ASTRONOMY.

Table of the geocentric motions of the two new planets for the month of July.

	A. R. of Pallas.			Declin. North.		A. R. Ceres Ferd.			Declin. South.	
July 2	18^h	21^m	37^s	23°	1′	18^h	42^m	33^s	28°	22′
5	18	19	7	22	50	18	39	38	28	34
8	18	16	43	22	36	18	36	46	28	45
11	18	14	20	22	21	18	32	54	28	55
14	18	12	5	22	2	18	31	8	29	4
17	18	9	54	21	43	18	28	33	29	12
20	18	7	52	21	21	18	24	55	29	20
23	18	5	57	20	56	18	23	32	29	27
26	18	4	11	20	30	18	21	18	29	34
29	18	2	36	20	4	18	19	17	29	39

PHILOSOPHICAL LECTURES.

The rev. D. F. PRYCE, A. M. curate of Bathwick, has been for some time engaged in preparing a philosophical apparatus, on a very extensive scale, under the inspection of Messrs. W. and S. Jones, Holborn, on which he designs to read two or more courses of popular lectures annually in the city of Bath. The apparatus is in such a state of forwardness,

ness, as will enable Mr. Pryce to commence early in the ensuing winter. He is also in treaty for the erection of a scientific theatre, capable of holding about five hundred persons, on a plan similar to that at the Royal Institution.

Dr. PEARSON proposes, in addition to his summer course of lectures on physic and chemistry, to give the following on the cow-pock inoculation, at the Institution, (founded Dec. 1799, late No. 5, Golden-square,) No. 44, Broad-street, Golden-square. A lecture to be given at the Institution once or twice a week, according to the subject of it, for about twelve weeks.

The principal objects will be: 1. The history of what is known of the vaccina in cows. 2. The history of the discovery, introduction, and propagation of vaccine inoculation. 3. To show, in patients at the Institution, the progress of the inoculated cow-pock, through its stages of growing into a vesicle, constitutional disorder, scabbing process, deciduary carbuncle-like scab, with a view especially to make known the distinguishing characters of the vaccina. 4. To explain the unusual or accidental symptoms and effects of the vaccina: viz. eruptions, phlegmonous inflammation, erythema, axillary swellings, essera vaccina, pustule, ulcerations of inoculated parts, &c. 5. To explain the anomalous eruption of inoculated parts. 6. To explain the anomalous course of the inoculated vaccina. 7. Intervening disorders, especially the small-pox, measles, chicken-pox, contagious angina, hooping cough, tooth rash, red gum, &c. 8. Instances, by inoculation, of the small-pox and cow-pock at the same time in the same person. 9. The various modes of preserving vaccine matter. 10. The effects of various modes of inoculation. 11. The effects of matter at different ages of the vaccine pock. 12. The effects of inoculation of persons who have undergone the small-pox or cow-pock. 13. The effects of inoculation when it fails to destroy the susceptibility of the small-pox. 14. The medical treatment and regimen during the cow-pock. 15. The effects on health subsequently to inoculation. 16. The vaccine inoculation instead of the small-pox, as vicarious of a disease in sheep.

PROPOSALS.—1. Subscribers for life, viz. of ten guineas, to the Vaccine Institution to be admitted gratuitously; as well as 2. Perpetual pupils to Dr. Pearson's lectures in general; and his 3. Other pupils on becoming perpetual, in addition to their present lectures, to be admitted on the same terms. 4. Those who are neither subscribers to the Institution, nor are pupils, as just mentioned, are to pay three guineas for a single course, or six guineas as perpetual.

XVIII. *On the Modifications of Clouds, and on the Principles of their Production, Suspension, and Destruction; being the Substance of an Essay read before the Askesian Society in the Session* 1802-3. *By* LUKE HOWARD, *Esq.*[*]

SINCE the increased attention which has been given to meteorology, the study of the various appearances of water suspended in the atmosphere is become an interesting and even necessary branch of that pursuit.

If clouds were the mere result of the condensation of vapour in the masses of atmosphere which they occupy, if their variations were produced by the movements of the atmosphere alone, then indeed might the study of them be deemed an useless pursuit of shadows, an attempt to describe forms which, being the sport of winds, must be ever varying, and therefore not to be defined.

But however the erroneous admission of this opinion may have operated to prevent attention to them, the case is not so with clouds. They are subject to certain distinct modifications, produced by the general causes which effect all the variations of the atmosphere: they are commonly as good visible indications of the operation of these causes as is the countenance of the state of a person's mind or body.

It is the frequent observation of the countenance of the sky, and of its connection with the present and ensuing phænomena, that constitutes the antient and popular meteorology. The want of this branch of knowledge renders the predictions of the philosopher (who in attending only to his instruments may be said only to examine the pulse of the atmosphere) less generally successful than those of the weather-wise mariner or husbandman.

With the latter, the dependence of their labours on the state of the atmosphere, and the direction of its currents, creates a necessity of frequent observation, which in its turn produces experience.

But as this experience is usually consigned only to the memory of the possessor, in a confused mass of simple aphorisms, the skill resulting from it is in a manner incommunicable; for, however valuable these links when in connection with the rest of the chain, they often serve, when taken singly, only to mislead; and the power of connecting them, in order to form a judgment upon occasion, resides only in the mind before which their relations have passed, though perhaps imperceptibly, in review. In order to enable the meteorologist to apply the key of analysis to the experience of others, as well as to record his own with brevity and precision, it may perhaps be allowable to introduce a methodical nomenclature, applicable to the various forms of suspended water, or, in other words, to the modifications of cloud.

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[*] Communicated by the Author.

By modification is to be understood simply the structure or manner of aggregation, not the precise form or magnitude, which indeed varies every moment in most clouds. The principal modifications are commonly as distinguishable from each other as a tree from a hill, or the latter from a lake; although clouds in the same modification, considered with respect to each other, have often only the common resemblances which exist among trees, hills, or lakes, taken generally.

The nomenclature is drawn from the Latin. The reasons for having recourse to a dead language for terms to be adopted by the learned of different nations are obvious. If it should be asked why the Greek was not preferred, after the example of chemistry, the author answers, that the objects being to be defined by visible characters, as in natural history, it was desirable that the terms adopted should at once convey the idea of these, and render a recourse to definitions needless to such as understand the literal sense, which many more would, it is concluded, in Latin than in Greek words.

There are three simple and distinct modifications, in any one of which the aggregate of minute drops called a cloud may be formed, increase to its greatest extent, and finally decrease and disappear.

But the same aggregate which has been formed in one modification, upon a change in the attendant circumstances, may pass into another.

Or it may continue a considerable time in an intermediate state, partaking of the characters of two modifications; and it may also disappear in this stage, or return to the first modification.

Lastly, aggregates separately formed in different modifications may unite and pass into one exhibiting different characters in different parts, or a portion of a simple aggregate may pass into another modification without separating from the remainder of the mass.

Hence, together with the simple, it becomes necessary to admit intermediate and compound modifications, and to impose names on such of them as are worthy of notice.

The simple modifications are thus named and defined:

1. CIRRUS. *Def.* Nubes cirrata, tenuissima, quæ undique crescat.

Parallel, flexuous, or diverging fibres, extensible in any or in all directions.

2. CUMULUS. *Def.* Nubes cumulata, densa, sursum crescens.

Convex or conical heaps, increasing upward from a horizontal base.

3. STRATUS. *Def.* Nubes strata, aquæ modo expansa, deorsum crescens.

A widely extended, continuous, horizontal sheet, increasing from below.

This application of the Latin word *stratus* is a little forced. But the substantive, *stratum*, did not agree in its termination with the other two, and is besides already used in a different sense even on this subject, e. g. *a stratum of clouds;* yet it was desirable to keep the derivation from the verb *sterno*, as its significations agree so well with the circumstances of this cloud.

The intermediate modifications which require to be noticed are:

4. CIRRO-CUMULUS. *Def.* Nubeculæ densiores subro tundæ et quasi in agmine appositæ.

Small, well defined roundish masses, in close horizontal arrangement.

5. CIRRO-STRATUS. *Def.* Nubes extenuata sub-concava vel undulata. Nubeculæ hujus modi appositæ.

Horizontal or slightly inclined masses, attenuated towards a part or the whole of their circumference, bent downward, or undulated, separate, or in groups consisting of small clouds having these characters.

The compound modifications are:

6. CUMULO-STRATUS. *Def.* Nubes densa, basim planam undique supercrescens, vel cujus moles longinqua videtur partim plana partim cumulata.

The cirro-stratus blended with the cumulus, and either appearing intermixed with the heaps of the latter or superadding a wide-spread structure to its base.

7. CUMULO-CIRRO-STRATUS vel NIMBUS. *Def.* Nubes vel nubium congeries pluviam effundens.

The rain cloud. A cloud or system of clouds from which rain is falling. It is a horizontal sheet, above which the

cirrus

cirrus spreads, while the cumulus enters it laterally and from beneath.

Of the Cirrus.

Clouds in this modification appear to have the least density, the greatest elevation, and the greatest variety of extent and direction. They are the earliest appearance after serene weather. They are first indicated by a few threads pencilled, as it were, on the sky. These increase in length, and new ones are in the mean time added laterally. Often the first-formed threads serve as stems to support numerous branches, which in their turn give rise to others.

The increase is sometimes perfectly indeterminate, at others it has a very decided direction. Thus the first few threads being once formed, the remainder shall be propagated either in one, two, or more directions laterally, or obliquely upward or downward, the direction being often the same in a great number of clouds visible at the same time: for the oblique descending tufts shall appear to converge towards a point in the horizon, and the long straight streaks to meet in opposite points therein; which is the optical effect of parallel extension.

Their duration is uncertain, varying from a few minutes after the first appearance to an extent of many hours. It is long when they appear alone and at great heights, and shorter when they are formed lower and in the vicinity of other clouds.

This modification, although in appearance almost motionless, is intimately connected with the variable motions of the atmosphere. Considering that clouds of this kind have long been deemed a prognostic of wind, it is extraordinary that the nature of this connection should not have been more studied, as the knowledge of it might have been productive of useful results.

In fair weather, with light variable breezes, the sky is seldom quite clear of small groups of the oblique cirrus, which frequently come on from the leeward, and the direction of their increase is to windward. Continued wet weather is attended with horizontal sheets of this cloud, which subside quickly and pass to the cirro-stratus.

Before storms they appear lower and denser, and usually in the quarter opposite to that from which the storm arises. Steady high winds are also preceded and attended by streaks running quite across the sky in the direction they blow in.

The relations of this modification with the state of the barometer, thermometer, hygrometer, and electrometer, have not yet been attended to.

Of the Cumulus.

Clouds in this modification are commonly of the most dense structure: they are formed in the lower atmosphere, and move along with the current which is next the earth.

A small irregular spot first appears, and is, as it were, the nucleus on which they increase. The lower surface continues irregularly plane, while the upper rises into conical or hemispherical heaps; which may afterwards continue long nearly of the same bulk, or rapidly rise to mountains.

In the former case they are usually numerous and near together, in the latter few and distant; but whether there are few or many, their bases always lie nearly in one horizontal plane, and their increase upward is somewhat proportionate to the extent of base, and nearly alike in many that appear at once.

Their appearance, increase, and disappearance, in fair weather, are often periodical, and keep pace with the temperature of the day. Thus they will begin to form some hours after sun-rise, arrive at their maximum in the hottest part of the afternoon, then go on diminishing and totally disperse about sun-set.

But in changeable weather they partake of the vicissitudes of the atmosphere; sometimes evaporating almost as soon as formed, at others suddenly forming and as quickly passing to the compound modifications.

The cumulus of fair weather has a moderate elevation and extent, and a well defined rounded surface. Previous to rain it increases more rapidly, appears lower in the atmosphere, and with its surface full of loose fleeces or protuberances.

The formation of large cumuli to leeward in a strong wind, indicates the approach of a calm with rain. When they do not disappear or subside about sun-set, but continue to rise, thunder is to be expected in the night.

Independently of the beauty and magnificence it adds to the face of nature, the cumulus serves to skreen the earth from the direct rays of the sun, by its multiplied reflections to diffuse, and, as it were, economise the light, and also to convey the product of evaporation to a distance from the place of its origin. The relations of the cumulus with the state of the barometer, &c. have not yet been enough attended to.

Of the Stratus.

This modification has a mean degree of density.

It is the lowest of clouds, since its inferior surface commonly rests on the earth or water.

Contrary to the last, which may be considered as belonging to the day, this is properly the cloud of night; the time of its first appearance being about sun-set. It comprehends all those creeping mists which in calm evening ascend in spreading sheets (like an inundation of water) from the bottom of valleys and the surface of lakes, rivers, &c.

Its duration is frequently through the night.

On the return of the sun the level surface of this cloud begins to put on the appearance of cumulus, the whole at the same time separating from the ground. The continuity is next destroyed, and the cloud ascends and evaporates, or passes off with the appearance of the nascent cumulus.

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Of the Cirro-stratus.

This cloud appears to result from the subsidence of the fibres of the cirrus to a horizontal position, at the same time that they approach towards each other laterally. The form and relative position, when seen in the distance, frequently give the idea of shoals of fish. Yet in this, as in other instances, the structure must be attended to rather than the form, which varies much, presenting at other times the appearance of parallel bars, interwoven streaks like the grain of polished wood, &c. It is always thickest in the middle, or at one extremity, and extenuated towards the edge. The distinct appearance of a cirrus does not always precede the production of this and the last modification.

The cirro-stratus precedes wind and rain, the near or distant approach of which may sometimes be estimated from its greater or less abundance and permanence. It is almost always to be seen in the intervals of storms. Sometimes this and the cirro-cumulus appear together in the sky, and even alternate with each other in the same cloud, when the different evolutions which ensue are a curious spectacle, and a judgment may be formed of the weather likely to ensue by observing which modification prevails at last. The cirro-stratus is the modification which most frequently and completely exhibits the phænomena of the solar and lunar halo, and (as supposed from a few observations) the parhelion and paraselene also. Hence the reason of the prognostic for foul weather, commonly drawn from the appearance of halo†.

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* The following passage is beautifully descriptive of the appearance of this modification by moonlight:

> For yet above these wafted clouds are seen
> (In a remoter sky, still more serene)
> Others, detached in ranges through the air,
> Spotless as snow, and countless as they're fair;
> Scatter'd immensely wide from east to west,
> The beauteous semblance of a flock at rest.
> These to the raptur'd mind aloud proclaim
> Their mighty shepherd's everlasting name.
>
> *Bloomfield's Farmer's Boy, Winter.*

† The frequent appearance of halo in this cloud may be attributed to its possessing great extent, at such times, with little perpendicular depth, and

This modification is on this account more peculiarly worthy of investigation. Little is yet ascertained of the relations of this and the last modification with the barometer, &c. although, as may be readily supposed, they have been found to accord with opposite indications of those instruments.

Of the Cumulo-stratus.

The different modifications which have been just treated of sometimes give place to each other, at other times two or more appear in the same sky; but in this case the clouds in the same modification lie mostly in the same plane of elevation, those which are more elevated appearing through the intervals of the lower, or the latter showing dark against the lighter ones above them. When the cumulus increases rapidly, a cirro-stratus is frequently seen to form around its summit, reposing thereon as on a mountain, while the former cloud continues discernible in some degree through it. This state continues but a short time. The cirro-stratus speedily becomes denser and spreads, while the superior part of the cumulus extends itself and passes into it, the base continuing as before, and the convex protuberances changing their position till they present themselves laterally and downward. More rarely the cumulus alone performs this evolution, and its superior part constitutes the incumbent cirro-stratus.

In either case a large lofty dense cloud is formed, which may be compared to a mushroom with a very thick short stem. But when a whole sky is crowded with this modification, the appearances are more indistinct. The cumulus rises through the interstices of the superior clouds, and the whole, seen as it passes off in the distant horizon, presents to the fancy mountains covered with snow, intersected with darker ridges and lakes of water, rocks and towers, &c. The distinct cumulo-stratus is formed in the interval between the first appearance of the fleecy cumulus and the

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and that degree of continuity of substance which seems requisite to the phænomenon. There is also probably some additional peculiarity of structure in it not yet attended to.

The following lines of Virgil seem to relate to an effect of the cirro-stratus, which in this country is more often to be observed on the *setting* sun:

> Ille ubi nascentem *maculis* variaverit ortum
> Conditus in nubem, *medioque refugerit orbe*,
> Suspecti tibi sunt *imbres*: namque urget ab alto
> Arboribusque satisque *Notus*, pecorique sinister.
>
> *Georgic.* lib. i.

commencement of rain, while the lower atmosphere is yet too dry; also during the approach of thunder storms: the indistinct appearance of it is chiefly in the longer or shorter intervals of showers of rain, snow, or hail.

The cumulo-stratus chiefly affects a mean state of the atmosphere as to pressure and temperature; but in this respect, like the other modifications, it affords much room for future observation.

Of the Nimbus, or Cumulo-cirro-stratus.

Clouds in any one of the preceding modifications, at the same degree of elevation, or in two or more of them, at different elevations, may increase so as completely to obscure the sky, and at times put on an appearance of density which to the inexperienced observer indicates the speedy commencement of rain. It is nevertheless extremely probable, as well from attentive observation as from a consideration of the several modes of their production, that the clouds while in any one of these states do not at any time let fall rain.

Before this effect takes place they have been uniformly found to undergo a change, attended with appearances sufficiently remarkable to constitute a distinct modification. These appearances, when the rain happens over our heads, are but imperfectly seen. We can then only observe, before the arrival of the denser and lower clouds, or through their interstices, that there exists *at a greater altitude* a thin light veil, or at least a hazy turbidness. When this has considerably increased we see the lower clouds spread themselves till they unite in all points and form one uniform sheet. The rain then commences, and the lower clouds, arriving from the windward, move under this sheet and are successively lost in it. When the latter cease to arrive, or when the sheet breaks, every one's experience teaches him to expect an abatement or cessation of rain.

But there often follows, what seems hitherto to have been unnoticed, an immediate and great addition to the quantity of cloud. At the same time the actual *obscurity* is lessened, because the arrangement, which now returns, gives freer passage to the rays of light: for on the cessation of rain the lower broken clouds which remain rise into cumuli, and the superior sheet puts on the various forms of the cirro-stratus, sometimes passing to the cirro-cumulus.

If the interval be long before the next shower, the cumulo-stratus usually makes its appearance, which it also does sometimes very suddenly after the first cessation.

But we see the nature of this process more perfectly in viewing a distant shower in profile.

If the cumulus be the only cloud present at such a time, we may observe its superior part to become tufted with nascent cirri. Several adjacent clouds also approach and unite laterally by subsidence.

The cirri increase, extending themselves upward and laterally, after which the shower is seen to commence. At other times the converse takes place of what has been described relative to the cessation of rain. The cirro-stratus is previously formed above the cumulus, and their sudden union is attended with the production of cirri and rain.

In either case the cirri *vegetate,* as it were, in proportion to the quantity of rain falling, and give the cloud a character by which it is easily known at great distances, and to which, in the language of meteorology, we may appropriate the nimbus of the Latins *.

When one of these arrives hastily *with the wind* it brings but little rain, and frequently some hail or driven snow.

In heavy showers, the central sheet once formed, is, as it were, warped to windward, the cirri being propagated above and against the lower current, while the cumuli arriving with the latter are successively *brought to* and contribute to reinforce it.

Such are the phænomena of showers. In continued gentle rains it does not appear necessary for the resolution of the clouds that the different modifications should come into actual contact.

It is sufficient that there exist two strata of clouds, one passing beneath the other, and each continually tending to horizontal uniform diffusion. It will rain during this state of the two strata, although they should be separated by an interval of many hundred feet in elevation. See an instance in De Luc, *Idées sur la Météorologie,* tom. ii. p. 52, &c.

As the masses of cloud are always blended and their arrangement destroyed before rain comes on, so the reappearance of these is the signal for its cessation. The thin sheets of cloud which pass over during a wet day, certainly receive from the humid atmosphere a supply proportionate to their consumption, while the latter prevents their increase in bulk. Hence a seeming paradox, which yet accords strictly with observation, that for any given hour of a wet day, or any given day of a wet season, *the more cloud the less rain.*

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* Qualis ubi ad terras abrupto sydere *nimbus*
It mare per medium, miseris heu *prescia longe*
Horrescunt corda agricolis.

Hence also arise some further reflections on the purpose answered by clouds in the œconomy of nature. Since rain may be produced by, and continue to fall from, the slightest obscuration of the sky by the nimbus (that is, by *two sheets* in different states), while the cumulus or cumulo-stratus, with the most dark and threatening aspect, shall pass over without letting fall a drop, until their change of state commences; it should seem that the latter are reservoirs in which the water is collected from a large space of atmosphere for occasional and local irrigation in dry seasons, and by means of which it is also arrested at times in its descent in the midst of wet ones *. In which so evident provision for the sustenance of all animal and vegetable life, as well as for the success of mankind in that pursuit so essential to their welfare, in temperate climates, of cultivating the earth, we may discover the wisdom and goodness of the creator and preserver of all things †.

The nimbus, although in itself one of the least beautiful clouds, is yet now and then superbly decorated with its attendant the rainbow; which can only be seen in perfection when backed by the widely extended uniform gloom of this modification ‡.

The relations of rain, and of periodical showers more especially, with the varying temperature, density, and electricity of the atmosphere, will probably now obtain a fuller investigation, and with a better prospect of success, than heretofore.

[To be continued; when Plates of the different Modifications will be given.]

XIX. *Researches respecting the Organization of Leaves. By* A. Jurine, *Member of the Society of Physics and Natural History of Geneva.*

[Continued from p. 15.]

Having described the organization of the surface of leaves such as I observed it, I shall proceed to their interior organization, omitting at present the different vessels found there, and which will be described in the second part of this memoir.

The authors who speak of the parenchyme describe it in so many different ways that it seems difficult to form a just idea of it. Saussure expresses himself as follows:—"In observing the cortical reticulation I have often had occasion to study another reticulation which is placed immediately below the former; it is the parenchymatous reticulation. This reticulation has larger and straighter vessels, and its meshes are generally greater than those of the cortical reticulation. The vessels of the parenchyme are besides almost always coloured, and for the most part green. They are very rarely cylindric; in general they grow narrower and larger in succession, so that they resemble vessels contiguous to each other."

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* Nulla dies adeo est Australibus humida nimbis
Non intermissis ut fluat imber aquis.

† See on this subject Job, chap. xxxvii and xxxviii.

‡ *Bibit* INGENS *arcus*, says Virgil, in enumerating the prognostics of continued rain.

Hedwig says* that he has seen in the leaves of moss small ducts, disposed according to the length of the leaf, which anastomose laterally with other transverse or lateral ducts in such a manner as to form areolæ sometimes square and sometimes oblong, pentagonal, or hexagonal, which almost all contain a parenchyme, the form of which is globular, and which gives to leaves their colour.

Senebier gives his opinion in the following terms†:— The appellations of cellular tissue, cellular covering, and parenchyme, are given indifferently to that reticulation formed by transparent fibres or vessels filled with a green juice, which are anastomosed at the places where they meet, and swelled up in the intervals. Utriculi will in all probability be found there, though nothing is seen with the best glasses but the meshes of a reticulation. If we conceive some parts of vegetables composed of fibres, forming meshes with a granulated substance, we shall have some idea of the matter which constitutes the greater part of leaves and fruits. The parenchyme I am about to describe will be found in that in particular which fills up the meshes of the greater part of reticulations.

C. Mirbel considers the parenchyme of leaves as a cellular tissue formed of cells which are filled with a juice almost always coloured green. It does not consist, he says, of small bags or utriculi; it is a membrane which unlines itself in some measure to form vacuities contiguous to each other.

In all the leaves which I have examined I have always found that their parenchyme was composed only of an aggregation of utriculi closely united to each other, filled with a green juice by which they are coloured, and of which the form varies according to the different plants. For example,

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* Musci Frondosi, pars i. p. 24.

† Physiologie Végétale, tom. i. p. 161.

ple, they are nearly spherical in the *fritillaria*, elongated or cylindrical in the *lavatera triloba* and the *erythronium dens leonis*, prismatic in the *aloe*, and irregular in the *sylphium perfoliatum*.

The utriculi of the parenchyme of the same leaf are seen to vary also in regard to their form and size. Thus the leaves of the *sylphium* and the *impatiens balsamina* have the utriculi irregular on the inferior side and cylindric on the superior. Those of the nenuphar have them small and elongated on the upper side, and large and prismatic on the lower. Those of the narcissus and others of the lily kind have them round and small on both sides, but large and prismatic in the interior part of the leaf.

From what has been said, if we suppose the parenchyme of a leaf to be composed of spherical utriculi adhering to each other, it will readily be conceived that, as these utriculi cannot touch each other throughout their whole surface, the necessary result will be vacuities or intervals between them, which will have a communication with each other, as seen fig. 15. If the utriculi are irregular, as those of fig. 18, the vacuities in this case will be larger; and if the utriculi approach the cylindric or prismatic form, the vacuities will be the less sensible as the utriculi touch each other in a greater extent of their surface.

It is of importance to comprehend properly the aggregation of the utriculi; the vacuities resulting from it, which in future I shall call *utricular interstices*; and the communication of these utriculi with the pores, in order to account for the circulation of the air in the leaves.

That air exists in leaves is a truth fully confirmed; for, by compressing them in water, the air is seen to issue from them. By exposing them under water to the action of an air-pump they emit air; and if they be left in the water for some time they soon lose their opacity, become transparent by the introduction of water, which assumes the place of the air, and are precipitated to the bottom of the vessel. In a word, these leaves when again exposed to the air gradually resume their opacity and their natural colour; which can be ascribed only to the air which re-enters as the water is evaporated. But where is this air lodged? I shall reply from my observations, that it is contained in the utricular interstices of the parenchyme.

If a leaf of the *fritillaria* or the *portulaca* be examined with the naked eye, or, still better, with a magnifying glass, there will be observed in its interior part small luminous points, which are produced by the air contained in the utricular aloe, which is composed of hexagonal utriculi, between which appear several square apertures. These apertures, similar to those of the fritillaria, fig. 2, correspond to as many conjugate utriculi which have remained on the parenchyme. The square form which these apertures constantly affect, seems to depend on that of the exterior utriculi.

Fig. 14. This pellicle belongs to the leaf of the digitalis purpurea: it consists of utriculi very much festooned, between which are seen the conjugate utriculi, the pores of which seem to be obliterated by a black matter, which is nothing else than air.

The surface of this pellicle is furnished with some conical hairs, very strong, and composed of several rings: they proceed from the middle of the utriculus, as if they were a prolongation of it.

Fig. 15. This figure represents the utriculi of the parenchyme of the leaf of the fritillaria: their form is nearly spherical; they are united to each other by a sort of prolongation in the manner of a neck, and separated in the rest of their extent by pretty considerable vacuities, which have a communication with each other. To these vacuities I have given the name of *utricular interstices.* Each of these utriculi is filled with a viscous juice, in which is found a great number of small green globules applied to the membrane of the utriculus: the upper ones appear more distinct, and the lower ones fainter.

The utriculus A is remarkable on account of the bundle of small prisms contained in its inside.

In fig. 16 these prisms are seen insulated and larger.

In fig. 17 is represented a different kind of these organs found in the nenuphar. Its exterior side is covered with points, and two ramifications proceed from its middle.

Fig. 18. On account of the singular form affected by the utriculi which compose the parenchyme of a great number of leaves, they have been called *irregular utriculi.* From their bodies proceed several arms in different directions, which unite with similar prolongations of the other utriculi; so that the parenchyme which they form becomes very lax, in consequence of the large interstices which they constitute.

These utriculi were taken from the parenchyme of the nectary of the narcissus.

Fig. 19. This figure exhibits the pellicle of a petal the utriculi of which rise above the surface in the form of conical papillæ, which produces a kind of dull velvety appearance, remarked in most petals.

Fig. 20. This figure represents a longitudinal section of the stem of the white poppy, the utriculus of which exhibited in a very striking manner the points considered by C. Mirbel as the pores of his cellular tissue: their form is elongated, and I saw them placed constantly in the direction of the breadth of the utriculus or of the stem.

XX. *Communication from Dr.* THORNTON *relative to the Pneumatic Practice.*

July 20, 1803.

MR. JOHN GREY, merchant, living at Newport in the Isle of Wight, was attacked by spasmodic asthma fourteen years ago, and has had paroxysms of this dreadful disorder, returning at first at uncertain intervals, until the year 1797, when these fits of difficult respiration recurred usually once, sometimes twice a week, leaving him in the intervals very languid and dispirited. The smallest exertion was a pain to him, and, to use his own expressions, life a burthen. Various physicians had been applied to without any essential benefit, and he despaired of ever getting free from this disease, when he was advised to come to London to be under my care. Upon his arrival in town, tonic medicines, such as he had taken before, were ordered, and he commenced with daily inspiration of four quarts of oxygen or vital air mixed with thirteen of atmospheric air. The immediate effect was a diminution of the violence of the paroxysms, which before lasted from two to three days; the expectoration was earlier, and the fits subsided sooner. Continuing this plan for a few weeks, the paroxysms of asthma wholly disappeared, and Mr. Grey was recommended to return home, and take down with him an eighteen gallon cask, with a tin pneumatic apparatus for inhaling the same; which arrived safe by the Newport waggon, the air being found to have lost none of its peculiar properties; as rekindling a match when blown out, and burning iron like wood. Another cask was sent down, medicines discontinued, and for six months Mr. Grey continued perfectly free from asthma, at the expiration of which time he had a slight return; but resuming again the oxygen air sent for from town, he was again re-established in health. For five years Mr. Grey has continued free from asthma, except in the month of August, when he is slightly indisposed; but having always had recourse to the inhalation of the oxygen air, sent to him in an eighteen gallon cask, the disorder has been did not belong to the epidermis. The result of his observations showed that these brilliant points were neither holes nor the orifices of vessels, for they were not altered by desiccation and maceration. "As they are not," says he, "resinous or gummy moleculæ, since they are unalterable in water and spirit of wine, what can they be?"

It appears to me that the formation of these globules is not owing to an inspissation of the utricular juice, and that the green colour which they assume by exposure to the light is only a modification depending on the effects of that fluid.

Besides the globules here mentioned, there are found in the utriculi of some plants singular organs with the uses of which I am unacquainted. These organs, represented fig. 15, A, and fig. 16, are small, prismatic, smooth, and transparent filaments of equal length, terminating in a point on each side, and united in a bundle to the number of forty or fifty, and even more.

In a leaf of the fritillaria which had been macerated for some time, these bundles are distinguished by the naked eye through the utriculi, like small, elongated, whitish, argenteous bodies, disposed in the direction of the length of the leaf.

I found them in the bulb of the *leucojum vernum*, of the *scilla bifolia* and *maritima*, in the stem of the *phytolaca decandra*, and in its leaves, where they are very apparent, especially when the pellicle has been removed from the inferior surface.

In the aloe, besides these bundles there are seen a great number of other prismatic filaments similar to the preceding, but insulated, larger, and lodged, as appeared to me, between the utricular interstices.

I saw also in the stem of the nenuphar other insulated filaments lodged also between the utriculæ, but cylindric, shagreened on the outside, and from the middle of which there proceed in general two or three ramifications, fig. 17.

Having accidentally touched my face with my hands while I was examining the aloe, I soon experienced a violent itching, which I ascribed to the entrance of these prisms into my skin. To ascertain this fact, I rubbed the back of my hand with a piece of a leaf of that plant; which occasioned a strong smarting pain followed by a cutaneous eruption. I repeated this experiment with the parenchyme of the *scilla maritima*, and found the same result.

If the leaves of the *narcissus*, *hyacinth*, *amaryllis formosissima*, and *scilla bifolia*, be cut transversely, there will be seen the course of my observations on the *System of Chemical Knowledge.*

To save the reader from the trouble of recurring to that work, I shall copy the passage where the author collects the facts and arguments, in consequence of which he thinks himself authorized to entertain this opinion. This passage is very remarkable, by the opposition discovered in it between the manner in which he and the modern chemists characterize other vegetable products.

"Rouelle junior, who examined and carefully compared it with other animal matters, asserts, that he found it in coloured fecula, and particularly in that called the green fecula of plants. But the expression of fecula, given indifferently to the fibrous matter contained in the juice of plants and to starch, having induced chemists to consider the latter as a part of the remains of solid vegetable substances, there is reason to think that it was merely by analogy, and in consequence of some ambiguous properties, that Rouelle was of opinion that the green matter contained gluten. At least the experiments made since that period, and those which I repeated several times on these coloured feculæ, did not confirm this assertion; and nothing has really proved that gluten is one of the principles of the latter fecula."

"The expression fecula," says Fourcroy, "given *indifferently* to the fibrous matter contained in the juice of plants and to starch, having induced an opinion that the latter is a part of the remains of solid vegetable substances, there is reason to believe," &c.

I shall first observe that this opinion is not correct. For example, chemists at present will never agree with Fourcroy, that the confusion which has been so justly ascribed to the improper use of words has induced those who preceded us to adopt these ideas. Our masters, it must be allowed, gave to things bad denominations; but they did not confound them more than we do.

At times even when every vegetable deposit was considered by them as fecula, the resemblance of names never deceived them so far as to make them compare starch to the remains of the solid parts of plants. In the first place, we are acquainted with no remains of that kind to which chemists could with reason compare it; and in the second, if any of them took the green fecula for a residuum, there was not one of them who was not perfectly acquainted with the whole difference between these feculæ or remains and starch: and since no confusion of this kind is found in their works, it is not just to reproach them with it;

for we need only cast our eyes on those of Rouelle, Macquer, Baumé, Sage, Parmentier, &c., to be convinced that the word fecula has not led these authors into comparisons so unworthy of their judgment as to arrange in the same class green fecula, the remains of solid parts, and starch.

We shall now proceed to green feculæ, and shall observe that in laboratories, in apothecaries' shops, and still less in the hands of a chemist so celebrated for exactness as Rouelle, the chopped straw of green plants has never been confounded with that beautiful liquid velvet expressed from their leaves, or with the emulsive product which passes in complete freshness through the cloth, and which by its excessive fineness, and the splendour of its colour, is so superior to their herbaceous filaments.

If it were true that the fecula is a body homogeneous with the rest of the plant, if it were possible to consider it only as a part which differs in no other respect than that of having been better pounded, would not complete trituration of the remainder be sufficient to convert it also into fecula? When a fresh herb is pounded, the pestle breaks and bruises its tissue, but does not pulverize it.

And this contusion of a few moments is too far from resembling dry pulverization to admit of any comparison between its fecula and moistened powder. If an aqueous plant, such as sedum for example, be bruised on a piece of marble by means of a roller, its expressed juice will give fecula. It is not to trituration that fecula is indebted for its velvety appearance, its fineness, and impalpability, which distinguish it from powder: it is molecular by its nature, and perhaps even crystallized in the fibrous meshes where vegetation deposits it.

"Rouelle asserts," says Fourcroy, "that feculæ contain a principle which may be compared to animal matters," &c. Rouelle has done more: very little disposed to be satisfied with simple assertions, he proved it not by *analogies* and *ambiguous properties*, but by a series of convincing facts, by comparisons which obtained general assent only because they united the greatest characters then known, and which are yet known, in animal substances. Otherwise whence could Rouelle deduce analogies to compare, as he does, green fecula to the gluten of wheat? What is there common in the appearance of these two products to serve as a basis for comparison? To find points of comparison, it would be necessary to examine in an intimate manner their composition and chemical properties; and this is what this laborious chemist does. Comparisons of this kind deduced from analysis served as a basis for the memoir which he gave on green feculæ, and of which no mention is made in The System of Chemical Knowledge; no doubt because, according to the ideas of this illustrious author, Rouelle had confounded albumen with gluten, and the detail of his mistake must have appeared to him a matter of indifference to the history of chemistry.

Rouelle, however, found in the fecula of sorrel a product so strongly possessed of the chemical properties of albumen, that he particularly insisted on it, to call the attention of chemists to a substance so animalized; and as he afterwards extracted it from a plant which, according to Fourcroy, does not furnish the slightest vestige of albumen, it is now certain, as it was then, that Rouelle was the first who found in green juices and feculæ a *product* which, if it ought not to be distinguished by the name of *albumen*, has, however, all its properties in such a degree that it must appear no less proper to make a figure in the history of their discoveries than albumen itself.

It is to the same penetration that we are indebted for those astonishing relations exhibited by caseum and gluten, when they have both experienced a kind of fermentation which transforms them into that cellular, odorous, and savoury combination called cheese: and in this singular result gluten approaches nearer to its model the more carefully it has been washed. Macquer, by asserting, as is now every where repeated, that it is indebted for a part of these changes to a residuum of starch, had not correct ideas. Starch, a substance always inert in fermentation, in that of bread, and of beer, and even in germination, would only serve to retard that experienced by gluten itself, and consequently could obliterate only in part the traces by which Rouelle discovered the resemblance of these two products.

And even their analysis passes beyond the limits which had been assigned to them; for, when gluten has changed its insipid and viscous mucosity for the caseous state, when it has passed through all the periods of that fermentation peculiar to it to arrive at this state, it is found seasoned with those acrid and burning salts which form the principal merit of the cheese of Roquefort; salts which have no affinity with that which is added, and which are found equally strong in the curd that has been washed and left to its own fermentation.

In the cheese of gluten, indeed, as in that of animals, potash and sulphuric acid will detect that ammonia and vinegar which Vauquelin discovered. Is the ammoniacal acetate, then, one of the ingredients which season cheese?

I know only that alcohol applied to pungent cheese deprives it of all its savour. An analysis directed to this point might give us curious results: but let us return to the green feculæ; let us try them by the test of modern chemistry; and, in particular, let us endeavour to discover whether albumen really exists where Beccari and Rouelle found gluten.

Green Fecula.

I. Fecula exposed to heat experiences a change capable of furnishing it alone with a decisive character in regard to its nature. I allude to that concrescibility of which there are few examples among vegetable products; that agglutination which attaches its moleculæ to each other, and gives them the appearance of caseous curd. If fecula before this change passes easily through the cloth, it can no longer do so when boiled; a peculiar crispation has then deprived it of its tenuity; but heat does not coagulate the fibrous tissue. The fecula, therefore, in this point of view, cannot be compared to the torn straw of green plants.

II. Fecula separated from juices by filtration assumes in drying a corneous and elastic consistence. It becomes soft with difficulty in boiling water, but it does not acquire softness even at the end of a month: notwithstanding its humectation it always retains its corneous nature. It recovers its former state if bent, but absolutely refuses to crumble: all these are qualities which are not observed in dried ligneous pulp.

The feculæ of green and white cabbages, cresses, hemlock, &c. do not by these means lose their property of coagulating by heat. In warm water between 145 and 165 degrees, if two equal matrasses be immersed, one with diluted fecula and the other with the water of the white of an egg, the fecula becomes crisp and is collected in flakes, such as those seen in any juice exposed to heat in order to be clarified; but at that temperature albumen does not even lose its transparency.

III. Green fecula is nearly of the same weight as water; for that of plants which are not acid employs more than eight days to deposit itself.

If fecula washed and diluted be poured into three jars, and if a little alcohol be added to the first, a few drops of acid to the second; and if the third be placed between the other two for the sake of comparison, the fecula in the two former will be completely deposited in less than half an hour, while that in the third scarcely begins to fall. Alcohol and acids then have the power of coagulating fecula, but they do not exercise the same action on woody remains.

IV. A hundred parts of the dry fecula of hemlock transmit to alcohol from 15 to 16 of green resin. When taken from repeated infusions to which it has been subjected, it remains of an earthen gray colour, and alcohol is never able to bleach it. Sage, who was well acquainted with the nature of feculæ, found some which gave even a third of their weight of resin: to exhaust them with ease, it is necessary to throw them still moist into spirit of wine; the spirit then penetrates and attacks them in every point: but this is much more difficult when they have been rendered corneous by desiccation.

Parmentier, I think, was the first who doubted, and with reason for his time, that alcoholic tincture of feculæ is resinous, because it is not precipitated by water. However, if it be considered that water can never detach it from gluten, that alcohol, oils, and fat, have exclusively this property; and that this substance, when separated from alcohol and concentrated in itself, is a fat tenacious body insoluble in water, it will be found that there is no product in vegetables which it approaches so much as the resinous: but we shall here show, that to determine in it more perfectly this character, nothing is necessary but to furnish it with a little oxygen.

The oxygenated muriatic acid in a few days renders green resin white and firm; it then suffers itself to be drawn out in threads like boiled turpentine, and its dye mixes readily with water: but if the green part of feculæ belonged to those coloured juices which are found in the ingredients proper for dyeing, oxygen would not convert it into a resin. But at present, since observation has taught us that we ought no longer to establish between vegetable products such rigorous limits as formerly; since we see them so often confounded by intermediate qualities, we are not astonished to find that a resin carried to its maximum of divisibility can associate with water. Do we not see camphor, essential, animal, and vegetable oils, sarcocolla, &c. dissolve completely in water? We shall not, however, for this reason, take such products from that classification which has been assigned to them by analysis.

Green feculæ assume in oxygenated muriatic acid that colour of dead leaves which forms the mourning of vegetation during the winter, and their dye becomes very turbid in water. Let us deduce then from all this, that if the colouring part of feculæ cannot resist water when transmitted by alcohol, it is no less in regard to its other qualities a substance absolutely resinous; and though this product, one of the most curious in the vegetable kingdom, since it embellishes it with its different shades, has not been introduced into the *System of Chemical Knowledge*, various chemists, such as Rouelle, Danel, Sage, Parmentier, &c. thought it worthy of their researches.

This resin, dissolved in potash, abandons it to attach itself to silk, and to give it a bright green dye; but it fades too much to become useful: its shade, however, resists verjus: but the preference which it at length gives to gluten over the vegetable fibre is agreeable to known principles; for it is to animalized substances in general, rather than to the fibres of flax, hemp, or cotton, that colouring bodies attach themselves. Fecula then has something in its nature analogous to wool, silk, &c.: it is gluten.

V. Let us now examine fecula in points of view more proper for unveiling new characters of animalization.

If fecula, either boiled or raw, be kept under water, it begins in less than twenty-four hours to emit a bad odour. It soon exhales an excrementitious putridity, which always goes on increasing, and to which one perhaps could not long be exposed without danger. The infectious miasm which it diffuses around it instantly obscures metallic writing, and its liquor speedily blackens plates of silver.

It is to the corruption of this principle, no doubt, rather than to any other, that are owing the pernicious exhalations of hemp and flax when watered. As running water, which is equally proper as stagnant for separating the filaments, speedily carries off their extractive juices, nothing but the green fecula which unites the fibres is susceptible of being destroyed by watering.

The liquor which at the end of a year floats above putrid fecula contains sulphurated hydrogen, carbonate of ammonia, and gluten dissolved by means of the latter.

It has this peculiarity also, that it retains its stercoraceous odour after long ebullition. The product of its distillation contains carbonate of ammonia joined to a principle of infection which does not blacken metallic solutions, and with the nature of which I am not acquainted. Acids are not weakened by precipitating fecula and becoming saturated with ammonia; which induces me to think, that if the effluvia of a mass of animal putridity can serve as a vehicle to the phosphorus and sulphur it contains, it is not indebted for its infectious quality to these combustibles alone; that there is a great difference, for example, between the odour of rotten fish or flesh, corrupted fecula or putrid cheese, and that of phosphorated and sulphurated hydrogen.

[To be continued.]

XXII. A

XXII. *A Survey and Report of the Coasts and Central Highlands of Scotland; made by the Command of the Right Honourable the Lords Commissioners of His Majesty's Treasury in the Autumn of* 1802. *By* THOMAS TELFORD, *Civil Engineer, Edinburgh, F.R.S.*

[Concluded from p. 81.]

APPENDIX.

Report from the Highland Society of Scotland.

Queries referred to in the annexed Report.

1st, DOES it consist with your knowledge, that the progress of improvement in the northern parts of Scotland is much retarded by the want of roads and bridges? If so, what lines would tend most effectually to open the country and promote the public good?

2d, Does the valley, which passes through the north of Scotland from the Murray Frith on the east to Loch Eil, and the Linnhe Loch on the west, appear to you to be well calculated for an inland navigation, if formed of a size sufficient to admit of large trading vessels and frigates? (I have, for the sake of distinction, named this navigation the Caledonian Canal.)

3d, Would this navigation, by opening a ready and safe communication from one side of the island to the other, prove the means of promoting the extension of the fisheries, and of throwing the industry and intelligence of the fishers who reside on the east coast upon the extensive fishing grounds along the west coast?

4th, Would the undertaking these public works at the present time, by affording employment to the people, giving them habits of industry, and furnishing them with capital, tend to check the spirit of emigration which now prevails, and, connected with the powers which would be furnished by using the water which flows down each extremity of the valley from the extensive lochs, prove the means of laying the certain foundation of future employment?

5th, If the executing these roads and bridges would prove the means of employing the people, improving the agricultural state of the country, and of extending the fisheries, the nation would evidently derive an increase of revenue and power; and the land-owners through whose estates the lines of road passed, and indeed the whole of the adjoining districts of country, would enjoy improved cultivation and pasturage, increased incomes, and all the blessings which are derived from a facility of intercourse: is it not therefore the interest of the land-owners to unite with government in executing these plans? and should not the memorials and propositions to this purpose originate with the land-owners, and be transmitted by them to the lords of the treasury, who will, by comparing the memorials with the information contained in the surveys made by their directions, judge how far the public aid can be with propriety extended?

6th, If the opening the Caledonian canal upon the scale I have proposed would prove the means of facilitating the intercourse from the west of England and Scotland, and the whole of Ireland, with the northern parts of Europe, and likewise from the east side of Great Britain to America and the West Indies; is it not just and reasonable that the commercial interests should be united with the efforts of government in carrying the same into effect?

7th, In my last I neglected to state, in order to enable the Highland proprietors to contribute, without inconvenience to themselves, a moiety of the expense of making the roads and bridges necessary for the improvement of that part of the country, that they might be empowered by an act of parliament to sell land to that amount. This is reasonable, because the price would be applied to improve the remainder of the entailed estates, which would by this means be much improved in value, though somewhat diminished in extent.

Report of a Sub-Committee of the Directors of the Highland Society of Scotland, on Consideration of a Letter from Mr. Telford, Engineer, to Henry Mackenzie, Esq. one of the Directors of the Society; made to, and approved of by, the General Committee of Directors of the said Society, 10th December 1802: the Right Honourable Lord Macdonald, one of the Vice-Presidents in Office, in the Chair.

The committee have fully considered Mr. Telford's questions, addressed to Mr. Mackenzie, one of the society's directors.

In answer to the first, they are persuaded that even the lines of communication by means of military roads in some parts of the Highlands, have been productive of benefit to the country, though, the motives which gave rise to their formation having no relation to objects of commerce and industry, the advantages derived from them are very imperfect.

perfect. The committee accordingly have no hesitation in declaring it to be their fixed opinion, that the want of further roads and communications in the Highlands has hitherto proved the greatest obstacle to the introduction of useful industry there, and that every attempt for that purpose must fail, until regular and easy communication is afforded from one part of the country to another, and more especially from the remote points where there is the best field for useful exertion to the present seats of capital and industry. With regard to what lines would tend most effectually to open the country, and promote the public good, the committee humbly report their opinion as follows:

The Highlands, as to this question, may be divided into three districts: the first, comprehending the west coasts of Argyle and Inverness-shire, as connected with each other; the second, including the county of Ross and a part of the county of Inverness; and the third, or northern district, comprising the shires of Sutherland and Caithness.

In the first of these districts, the utmost benefit would arise from drawing a direct line of communication from the west side of the Frith of Clyde nearly opposite to Greenock to the Bay of Strachur upon Loch Fyne, from whence there is already an excellent and well conducted road to Fort William. From this point the road may be easily continued by Loch Eil Side to Loch na Gaul, through Arisaig into Morer. Such a communication would tend very greatly to the success of the fisheries in the islands of Egg, Rum, Cana, Muck, Barra, and South Uist, all of which possess numerous lochs and fishing banks in and around them. The greatest advantages would arise from approximating these various fisheries and extensive coasts to the Frith of Clyde, where the fishing capital is at present almost exclusively resident. It is evident that nothing can more discourage the employment of that capital in those parts than that difficulty of approach, amounting almost to inaccessibility, which renders the communication of intelligence always slow and even often precarious.

In the northern district the lines of communication would, from the nature of the thing, be drawn to a different point. A central point at the south of that district is found at or near Invershin, to which place the Frith of Dornoch is navigable, and where a bridge can easily be thrown over, and from whence a direct and short communication could be made to Dingwall and Inverness. From this point several advantageous lines of road might be made, one stretching by the banks of Loch Shin through part of Assint to been prevented forming, and he has continued free from asthma during the rest of the year.

Observation.—It is surprising that a remedy which is so safe, pleasant, and efficacious, and which recommends itself so strongly by innumerable facts, and the analogy of ordering sick people into the country, should not have made more progress in the medical world, especially in cases of asthma, which is so materially affected by changes in the atmosphere. At this time I shall only add another case.

Miss ——— was recommended to my care by the late Mr. Cruikshank for a humour she had been afflicted with upwards of twenty years. It was not an ordinary case, for the tumefaction of the legs was such as to make the leg the thickness of one's thigh; and so great was the discharge from this part, and the whole body, that the servant has been known to come in the morning with the mop, to mop under the bed, the whole of the under part of the bed being wet through. For more than two years this amiable lady daily inhaled the vital air, and the benefit was progressive until a complete cure was accomplished. This was more than five years ago, and the patient has since continued in perfect health.

At another opportunity I will trouble the readers of your Magazine with other facts of the same sort.

XXI. *An Essay on the Fecula of Green Plants. By Professor* Proust [*].

H. Rouelle was the first who discovered in fecula a substance analogous to the gluten of farina. This substance since that time appeared problematical, only because few chemists tried to ascertain its real characters. The fecula of which it is the basis is, according to Fourcroy, either a supposed substance, or a substance too little examined to be placed among the number of the immediate products of vegetables. He even goes so far as to suppose that albumen, an animal substance which no one before him had supposed to exist in plants, is that which ought to be substituted for the glutinous part of green fecula.

Are albumen and fecula, then, found together or separate in the juice of plants? Such is the question I have proposed to myself, and which I shall endeavour to resolve in the in the northern lochs, some considerable shoals having appeared, intelligence was dispatched to Greenock; but owing to the indirect course of the post, and the difficulties of some parts of the circuitous journey, several weeks elapsed before any advantage could be taken of the information *.

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[*] From the *Journal de Physique*, Pluviose, an 11.

The lines that have been suggested, or nearly such lines, are, in the opinion of the committee, the radical lines of road, as they may be termed, from which in process of time various ramifications will be formed, when the benefits of these begin to be perceptibly diffused.

From consideration of the connection of the fifth question with what precedes, the committee in so far depart from Mr. Telford's arrangement as to put next in order the answer to it. They are fully persuaded of the reality of those views, both of public and individual benefit, which the statement of the question includes; and they think it highly reasonable that the land-owners should, according to their respective abilities, unite with government in executing these plans by contributing a certain proportional part of the expense, varying with the different circumstances of their several situations. But the committee humbly report their opinion, that it would be advisable for the lords of the treasury, after weighing such suggestions as have been made, and consulting their surveyor, to select the lines of road which more immediately, and in a national view, invite the public aid; and then, after the selection is made known, it will be the duty as well as the interest of land-owners, to come forward with their proposals, stating, with regard to each separately, those local considerations which seem to fix the proportion of public aid that may fairly be solicited.

In answer to query second, the committee have no doubt that the Caledonian canal, formed on the scale suggested (sufficient for the passage of large trading ships and frigates), will be attended with the greatest national advantage. In respect to these objects, indeed, the benefit must be so incalculably great, that this truly useful undertaking assuredly merits the attention and exertions of government. The committee have equally little doubt in concurring with the opinion inferred in the third question, that, by opening a free communication from the eastern to the western sea, it would be highly beneficial to the fisheries, particularly by transferring the skill in the cod and ling fishery, possessed by the people on the eastern coast, by whom it is certainly

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* The ordinary course of the post is one week from Loxford to Tongue, and another from Tongue to Tain, being on the line by Inverness to Edinburgh.

better understood than by the natives of the western, from the former to the latter of these shores, where the field for its action is inexhaustible.

With regard to query fourth, the committee are well convinced that the undertaking these public works must produce the united good consequences of checking the spirit of emigration, by affording useful employment to a great number of people, of improving the habits of the country by teaching lessons of systematic industry, and of affording at once the excitement to undertake, and the intelligence as well as (to a certain moderate extent) the means required for instituting those fishing and manufacturing establishments, on which the future prosperity of the Highlands must be founded.

On the sixth question the committee have to observe, that they are fully aware of the commercial as well as other national advantages derivable from the Caledonian canal: but with regard to the question to what extent commercial men would be ready to contribute individually towards carrying the same into effect, the Highland Society can have no means of forming an opinion, other than by reference to that general spirit of liberal enterprise which distinguishes the commercial body.

Adverting to a supplementary suggestion from Mr. Telford, the committee apprehend that it would be highly expedient to introduce a clause into any act of parliament on the subject, authorizing and empowering proprietors of entailed estates either to sell lands for defraying the expense of contributing along with government to the making of roads and bridges in the Highlands, or in their option to make the same a debt, affecting the subsequent heirs of entail.

It has been stated to the committee, that tutors and curators of minor proprietors, and trustees holding possession of estates concerned in these improvements, might feel some hesitation in venturing on the necessary outlays, as entertaining a doubt of such acts of administration falling within their powers. The committee are humbly of opinion, that it would be proper to add to the clause already suggested, an enactment, that tutors and curators of a minor heir of entail, or trustees in possession of an estate already entailed, or which is directed to be entailed, should have the same power of selling lands or charging the estate that is by the act conferred upon heirs of entail themselves. As also that tutors and curators of minors possessed of unentailed estates, and trustees holding possession of such, should be entitled by their acts in the premises to bind the minor or trustee, and all successors to the estate.

A true copy from the record.

(Signed) *Lewis Gordon*, dep. sec.

Highland Society Hall,
Edinburgh, Dec. 10, 1802.

Highland Society Hall, Edinburgh, Dec. 23, 1802.

Minute of the Committee of the Directors of the Highland Society of Scotland, which formerly drew up Answers, in the Shape of a Report, to the Queries of Mr. Telford, Engineer, respecting the opening of Communications by Roads and Bridges in the Highlands, and by a Canal from Inverness to Fort William, upon considering a Letter from Mr. Telford, of date 14th December 1802, to Henry Mackenzie, Esq. one of the Society's Directors, owning Receipt of said Report, which he states to be able, full, and satisfactory, and that the "only Instance in which it is rather less explicit than he could wish, is with respect to the Road in the Middle Division, which should connect the Inverness and Fort William Road with Skye, &c. &c." as to which Mr. Telford wishes the Committee could say something more specific, and recommends their taking any Information which can be furnished by Mr. Donaldson, Surveyor of Military Roads, upon that Point: the Right Honourable Lord Macdonald, one of the Vice-Presidents in Office, in the Chair.

The committee, in their former report, have pointed out the great objects to which roads through the district in question should in their opinion apply; but there being a difference of opinion as to the precise lines of road by which those objects would best be attained, the committee do not feel themselves at liberty to specify those precise lines. They would take the liberty of suggesting the expediency of government employing some able surveyor or engineer of respectable character and abilities to report on the subject; and if, relative to the present point of inquiry, they are to say any thing more particular, they may mention that the objects of this line of intersection seem chiefly twofold, viz. to afford a communication to the head of Loch Hourn, a very valuable fishing loch, and also to Bernera, the nearest point to Skye; both which they apprehend may be attained by two ramifications of a road from the military road leading from Fort William to Inverness.

The committee, adverting to Mr. Telford's suggestion of an examination of Mr. Donaldson, called him before them; but found that he had never travelled any part of the country from Fort Augustus, westward, to Bernera or the lochs, and that his information was solely, as to that part of the country, derived from others.

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XXIII. *Anatomical Observations on the Crocodile of the Nile.* *By* E. Geoffroy.

The following observations were read in the last sitting of the Institute of Egypt:—Two unfortunate combats, and the loss of the battle of the 30th of Ventose, year 9, gave us reason to apprehend that the enemy, favoured by the misunderstanding which prevailed between our chiefs, would at length tear from us the most valuable of our colonies, which had cost us so many efforts and sacrifices; in a word, that celebrated country Egypt, which we had explored in every direction, which we had seen covered with monuments coeval with the heroic ages, and the fertility of which had appeared to us superior to its reputation. At the moment when we were informed of our disasters, and when the report circulated of them immediately excited against us the whole population of Egypt, a crocodile was brought to me which had been carried alive to Cairo, and which had died three days before. At a more fortunate period I had ardently desired to dissect an animal so much celebrated by antient authors; but being at that time abandoned to those painful sensations which all the French experienced, I hesitated a moment whether I should undertake this labour. Foreseeing, however, that if I let slip this opportunity I might never have another, and being persuaded, as I always have been, that the courage proper for travellers placed in the same circumstances as those in which I then found myself, is that of resignation, I paid no attention to any thing but the crocodile then before me. But I was not able to proceed to a regular dissection, nor to extend my researches to all these organs which appeared to be worthy of notice, being prevented by a commencement of putrefaction which the crocodile had already experienced, and by the obligation I was under to save and to preserve the skin. Besides, as it had already been observed by several distinguished anatomists, I thought it sufficient to confine myself to the consideration of the organs which might have escaped their examination; so that what I now publish contains only some additions to the history of an animal known since the earliest ages.

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* From *Annales du Museum d'Histoire Naturelle*, No. 7.

I. *Of the Manner in which the Jaws are moved.*

Who could believe, considering the present state of science, that this question is still problematical? It has been combated by a great number of travellers and naturalists, but none of them, as will be seen, have completely solved it.

Herodotus is the first who asserts that the crocodile is the only known animal whose upper jaw is moveable on the lower, which remains fixed: his opinion has been followed by all the antients. Aristotle, Pliny, &c., and some of the moderns, such as Margrave, Oligerus, Jacobæus, Marmol, the illustrious Vesalius, and some Jesuit missionaries to Siam, who had an opportunity of seeing living crocodiles, or of examining them soon after death, all speak of them in the same terms; but little attention was paid to these testimonies. The first anatomists of the Academy of Sciences undertook to demonstrate the impossibility of the fact advanced by Herodotus, and the names of Perrault and Duverney tended to establish this opinion, which has been adopted by the naturalists who have since written on the crocodile.

It is no doubt very surprising that Perrault, known for his accuracy, and who carefully dissected a crocodile from the menagerie of Versailles, did not pay sufficient attention to the singular conformation of the head of this animal; and that he should have opposed with so much violence the opinion of the antients. He gives a minute description of the articulation of the jaws, without observing that it furnishes proofs against the fact which he proposed to establish; and he besides supposed that he had done it in a satisfactory manner by rectifying the errors of Marmol, which he falsely ascribed to Vesalius, and by proving, with reason, that the upper jaw is not, as in perroquets, separated from the cranium, but that it forms with the rest of the head one osseous piece.

Since men of such merit as Perrault, Duverney, and the other naturalists who have since examined crocodiles in collections, could doubt of a fact attested by so great a number of observers, this question must certainly be embarrassed by a difficulty which can be cleared up only by an exact description of the head, and of the organs by which it is moved.

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XXII. *A*

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In the first of these districts, the utmost benefit would arise from drawing a direct line of communication from the west side of the Frith of Clyde nearly opposite to Greenock to the Bay of Strachur upon Loch Fyne, from whence there is already an excellent and well conducted road to Fort William. From this point the road may be easily continued by Loch Eil Side to Loch na Gaul, through Arisaig into Morer. Such a communication would tend very greatly to the success of the fisheries in the islands of Egg, Rum, Cana, Muck, Barra, and South Uist, all of which possess numerous lochs and fishing banks in and around them. The greatest advantages would arise from approximating these various fisheries and extensive coasts to the Frith of Clyde, where the fishing capital is at present almost exclusively resident. It is evident that nothing can more discourage the employment of that capital in those parts than that difficulty of approach, amounting almost to inaccessibility, which renders the communication of intelligence always slow and even often precarious.

In the northern district the lines of communication would, from the nature of the thing, be drawn to a different point. A central point at the south of that district is found at or near Invershin, to which place the Frith of Dornoch is navigable, and where a bridge can easily be thrown over, and from whence a direct and short communication could be made to Dingwall and Inverness. From this point several advantageous lines of road might be made, one stretching by the banks of Loch Shin through part of Assint to

Kylescow, another by the kirk of Lairg to the head of Loch Loxford, and a third from the kirk of Lairg by the west of Lochnaver to Tongue. Another road again would connect together the western and eastern extremities of this the northern coast of Scotland, proceeding from Loch Eriboll (at which place there is one of the finest harbours in the kingdom) by Tongue, Farr, and Thurso, to Honna on the east. From this point, where there is a ferry to Orkney, the road would return to Wick, and from thence along the east coasts of Sutherland and Caithness, crossing the river Fleet by a bridge, to avoid the little Ferry, till it terminated at Invershin. Such lines as the above would open the whole of these countries to all the trading capital of Inverness and the east coast of Scotland, as well as by the way of Fort William to that of the Clyde; and it is well known that all the way from the vicinity of Kylescow round to Wick, the fishing grounds are abundant and excellent.

As to the middle division, the committee would humbly suggest the utility of certain lines of intersection from east to west. One of these ought to be from the great military road between Fort William and Inverness in a western direction, such as may best afford an easy intercourse between both these places and the islands of Skye, Harries, and North Uist, as well as Loch Hourn, Loch Duich, and the other valuable fishing lochs in that vicinity. A second will lead from Contin (which has already a good road to Dingwall) by the south side of Loch Garve and the head of Loch Lickart to Achnashine, and from thence in one branch to Loch Carron and in another to Pollew. From one or other of these branches a road of important benefit might be drawn to Loch Torridon, a third road will extend from the port of Ullapool in Loch Broom to Invershin at the head of the Frith of Dornock.

When the lines of road now mentioned are completed, the course of post will become rapid and regular. From the neighbourhood of Skye to Greenock the mail would be conveyed in three days, while from Invershin to Edinburgh by Aberdeen, or to Greenock by Inverness and Fort William, it would be conveyed at furthest in four, and thus the most remote points of the Highlands would be brought within five days course of post, at the utmost, of Edinburgh and the Frith of Clyde. It may suffice for contrasting such a situation of the Highlands with that in which they are now placed, in respect to communication of intelligence, to relate what happened this very year. When, after the return of the Clyde vessels from a vain search for herrings in the northern lochs, some considerable shoals having appeared, intelligence was dispatched to Greenock; but owing to the indirect course of the post, and the difficulties of some parts of the circuitous journey, several weeks elapsed before any advantage could be taken of the information *.

The lines that have been suggested, or nearly such lines, are, in the opinion of the committee, the radical lines of road, as they may be termed, from which in process of time various ramifications will be formed, when the benefits of these begin to be perceptibly diffused.

From consideration of the connection of the fifth question with what precedes, the committee in so far depart from Mr. Telford's arrangement as to put next in order the answer to it. They are fully persuaded of the reality of those views, both of public and individual benefit, which the statement of the question includes; and they think it highly reasonable that the land-owners should, according to their respective abilities, unite with government in executing these plans by contributing a certain proportional part of the expense, varying with the different circumstances of their several situations. But the committee humbly report their opinion, that it would be advisable for the lords of the treasury, after weighing such suggestions as have been made, and consulting their surveyor, to select the lines of road which more immediately, and in a national view, invite the public aid; and then, after the selection is made known, it will be the duty as well as the interest of land-owners, to come forward with their proposals, stating, with regard to each separately, those local considerations which seem to fix the proportion of public aid that may fairly be solicited.

In answer to query second, the committee have no doubt that the Caledonian canal, formed on the scale suggested (sufficient for the passage of large trading ships and frigates), will be attended with the greatest national advantage. In respect to these objects, indeed, the benefit must be so incalculably great, that this truly useful undertaking assuredly merits the attention and exertions of government. The committee have equally little doubt in concurring with the opinion inferred in the third question, that, by opening a free communication from the eastern to the western sea, it would be highly beneficial to the fisheries, particularly by transferring the skill in the cod and ling fishery, possessed by the people on the eastern coast, by whom it is certainly

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* The ordinary course of the post is one week from Loxford to Tongue, and another from Tongue to Tain, being on the line by Inverness to Edinburgh.

better understood than by the natives of the western, from the former to the latter of these shores, where the field for its action is inexhaustible.

With regard to query fourth, the committee are well convinced that the undertaking these public works must produce the united good consequences of checking the spirit of emigration, by affording useful employment to a great number of people, of improving the habits of the country by teaching lessons of systematic industry, and of affording at once the excitement to undertake, and the intelligence as well as (to a certain moderate extent) the means required for instituting those fishing and manufacturing establishments, on which the future prosperity of the Highlands must be founded.

On the sixth question the committee have to observe, that they are fully aware of the commercial as well as other national advantages derivable from the Caledonian canal: but with regard to the question to what extent commercial men would be ready to contribute individually towards carrying the same into effect, the Highland Society can have no means of forming an opinion, other than by reference to that general spirit of liberal enterprise which distinguishes the commercial body.

Adverting to a supplementary suggestion from Mr. Telford, the committee apprehend that it would be highly expedient to introduce a clause into any act of parliament on the subject, authorizing and empowering proprietors of entailed estates either to sell lands for defraying the expense of contributing along with government to the making of roads and bridges in the Highlands, or in their option to make the same a debt, affecting the subsequent heirs of entail.

It has been stated to the committee, that tutors and curators of minor proprietors, and trustees holding possession of estates concerned in these improvements, might feel some hesitation in venturing on the necessary outlays, as entertaining a doubt of such acts of administration falling within their powers. The committee are humbly of opinion, that it would be proper to add to the clause already suggested, an enactment, that tutors and curators of a minor heir of entail, or trustees in possession of an estate already entailed, or which is directed to be entailed, should have the same power of selling lands or charging the estate that is by the act conferred upon heirs of entail themselves. As also that tutors and curators of minors possessed of unentailed estates, and trustees holding possession of such, should be entitled by their acts in the premises to bind the minor or trustee; and all successors to the estate.

A true copy from the record.

(Signed) *Lewis Gordon,* dep. sec.

Highland Society Hall,
Edinburgh, Dec. 10, 1802.

Highland Society Hall, Edinburgh, Dec. 23, 1802.

Minute of the Committee of the Directors of the Highland Society of Scotland, which formerly drew up Answers, in the Shape of a Report, to the Queries of Mr. Telford, Engineer, respecting the opening of Communications by Roads and Bridges in the Highlands, and by a Canal from Inverness to Fort William, upon considering a Letter from Mr. Telford, of date 14th December 1802, to Henry Mackenzie, Esq. one of the Society's Directors, owning Receipt of said Report, which he states to be able, full, and satisfactory, and that the "only Instance in which it is rather less explicit than he could wish, is with respect to the Road in the Middle Division, which should connect the Inverness and Fort William Road with Skye, &c. &c." as to which Mr. Telford wishes the Committee could say something more specific, and recommends their taking any Information which can be furnished by Mr. Donaldson, Surveyor of Military Roads, upon that Point: the Right Honourable Lord Macdonald, one of the Vice-Presidents in Office, in the Chair.

The committee, in their former report, have pointed out the great objects to which roads through the district in question should in their opinion apply; but there being a difference of opinion as to the precise lines of road by which those objects would best be attained, the committee do not feel themselves at liberty to specify those precise lines. They would take the liberty of suggesting the expediency of government employing some able surveyor or engineer of respectable character and abilities to report on the subject; and if, relative to the present point of inquiry, they are to say any thing more particular, they may mention that the objects of this line of intersection seem chiefly twofold, viz. to afford a communication to the head of Loch Hourn, a very valuable fishing loch, and also to Bernera, the nearest point to Skye; both which they apprehend may be attained by two ramifications of a road from the military road leading from Fort William to Inverness.

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The committee, adverting to Mr. Telford's suggestion of an examination of Mr. Donaldson, called him before them; but found that he had never travelled any part of the country from Fort Augustus, westward, to Bernera or the lochs, and that his information was solely, as to that part of the country, derived from others.

A true copy from the record.

(Signed) *Lewis Gordon*, dep. sec.

XXIII. *Anatomical Observations on the Crocodile of the Nile.* *By* E. GEOFFROY.

THE following observations were read in the last sitting of the Institute of Egypt:—Two unfortunate combats, and the loss of the battle of the 30th of Ventose, year 9, gave us reason to apprehend that the enemy, favoured by the misunderstanding which prevailed between our chiefs, would at length tear from us the most valuable of our colonies, which had cost us so many efforts and sacrifices; in a word, that celebrated country Egypt, which we had explored in every direction, which we had seen covered with monuments coeval with the heroic ages, and the fertility of which had appeared to us superior to its reputation. At the moment when we were informed of our disasters, and when the report circulated of them immediately excited against us the whole population of Egypt, a crocodile was brought to me which had been carried alive to Cairo, and which had died three days before. At a more fortunate period I had ardently desired to dissect an animal so much celebrated by antient authors; but being at that time abandoned to those painful sensations which all the French experienced, I hesitated a moment whether I should undertake this labour. Foreseeing, however, that if I let slip this opportunity I might never have another, and being persuaded, as I always have been, that the courage proper for travellers placed in the same circumstances as those in which I then found myself, is that of resignation, I paid no attention to any thing but the crocodile then before me. But I was not able to proceed to a regular dissection, nor to extend my researches to all those organs which appeared to be worthy of notice, being prevented by a commencement of putrefaction which the crocodile had already experienced, and by the obligation I was under to save and to preserve the skin. Besides, as it had already been observed

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[*] From *Annales du Museum d'Histoire Naturelle*, No. 7.

by several distinguished anatomists, I thought it sufficient to confine myself to the consideration of the organs which might have escaped their examination; so that what I now publish contains only some additions to the history of an animal known since the earliest ages.

I. *Of the Manner in which the Jaws are moved.*

Who could believe, considering the present state of science, that this question is still problematical? It has been combated by a great number of travellers and naturalists, but none of them, as will be seen, have completely solved it.

Herodotus is the first who asserts that the crocodile is the only known animal whose upper jaw is moveable on the lower, which remains fixed: his opinion has been followed by all the antients. Aristotle, Pliny, &c., and some of the moderns, such as Margrave, Oligerus, Jacobæus, Marmol, the illustrious Vesalius, and some Jesuit missionaries to Siam, who had an opportunity of seeing living crocodiles, or of examining them soon after death, all speak of them in the same terms; but little attention was paid to these testimonies. The first anatomists of the Academy of Sciences undertook to demonstrate the impossibility of the fact advanced by Herodotus, and the names of Perrault and Duverney tended to establish this opinion, which has been adopted by the naturalists who have since written on the crocodile.

It is no doubt very surprising that Perrault, known for his accuracy, and who carefully dissected a crocodile from the menagerie of Versailles, did not pay sufficient attention to the singular conformation of the head of this animal; and that he should have opposed with so much violence the opinion of the antients. He gives a minute description of the articulation of the jaws, without observing that it furnishes proofs against the fact which he proposed to establish; and he besides supposed that he had done it in a satisfactory manner by rectifying the errors of Marmol, which he falsely ascribed to Vesalius, and by proving, with reason, that the upper jaw is not, as in perroquets, separated from the cranium, but that it forms with the rest of the head one osseous piece.

Since men of such merit as Perrault, Duverney, and the other naturalists who have since examined crocodiles in collections, could doubt of a fact attested by so great a number of observers, this question must certainly be embarrassed by a difficulty which can be cleared up only by an exact description of the head, and of the organs by which it is moved.

The question here is not merely to rectify an accredited error, and to defend the antients from the injustice done them by some of the moderns. I must also call the attention of naturalists to a singular fact in regard to organization. Nothing, indeed, can be more paradoxical than the head of the crocodile; all those parts which in other animals are on the sides, are in the crocodile thrown backwards. The temporal bone itself projects backwards a good deal beyond the cranium: it is elongated, and transformed into a double condyle, the functions of which it performs. Every thing has in some measure been said of the head of the crocodile, by considering it as composed merely of two jaws, for the cranium is so small and so displaced that it escapes the first examination. It is found below and a little before the occipital plate; the brain, or rather the ganglion, contained in its cavity, which is exceedingly narrow, is continued pretty far forwards, so that the organs of sight and of hearing are situated below and a little behind it.

Another anomaly equally worthy of remark is, 1st, That the lower jaw is a sixth longer than the upper and the cranium. 2d, That the lower jaw exhibits a cavity with two facets, where the horns of the temporal bone are articulated by a ginglymus. 3d, That the occipital condyle is on the same line as the four condyles of the temporal bones, so that the head is really retained towards its points of articulation as the lid of a box is by its hinge. 4th, That as the two jaws have only a simple motion from the top downwards, they cannot be moved separately to the right or to the left, to subject the aliments to a sort of trituration.

On examining a living crocodile, or one prepared, as is customary in collections, it is hardly possible to believe that the head terminates at the extremity of the jaws; one looks for the osseous box which contains the brain, and which in all other animals manifests itself externally under the form of a frontal protuberance. The observer thinks he sees it towards the anterior part of the neck, which is symmetrically swelled up, and which is generally taken for the complement of the head: but this swelling arises from the presence of the crotaphite muscles, which are pretty voluminous, and which in a great measure are lodged between the straight and oblique muscles.

Differences so great in the form of the head necessarily occasion others in the organs which correspond directly with it; and it is indeed found that the cervical column is composed of seven vertebræ, which are distinct, but combined in their articulation in such a manner that they are not moveable on each other. The apophyses of these vertebræ are so multiplied, so long, and so close to each other, that the animal cannot bend its neck, and that the cervical column, in regárd to its uses, must be considered as one bone. The straight and oblique muscles attached to it, and which have their second point of insertion towards the occipital ridge, raise up when they contract the head on the neck, by making it describe an arch of 45°. The skin is thin behind the occipital plate, and readily yields to all the movements given to the head: on the other hand, the lower jaw is, as it were, sheathed in a rugous and very little flexible skin. If we suppose a muscular force sufficiently strong to draw it downwards, it would be retained by its coverings: it is besides confined towards its posterior extremity; for the long apophysis situated beyond the articular facets approaches the skin by describing a curve exactly towards the point, where it is armed with a long scale. The latter opposes an almost invincible resistance to the elevation of the condyle, and consequently to the depression of the jaw: it is not however entirely fixed, especially in the manner understood by Marmol, who thought that it formed with the sternum one bone. Two small elongated muscles, by contracting, can give it a slight motion. The assertion of Herodotus, then, is almost strictly true: "The crocodile is the only animal known, whose upper jaw, between the branches of which is comprehended the cranium, is moveable on the under, which has a motion almost insensible."

II. *Of the Organs of Digestion.*

The antients, and almost all the moderns, have stated that the crocodile has no tongue: it is indeed true that it does not appear outwardly, but, speaking in a physiological point of view, this animal is not destitute of a tongue. The whole skin comprehended between the branches of the lower jaw is clothed interiorly with spongy, thick and flabby flesh, which is inseparably attached to it throughout its whole extent: but this muscle or tongue is in some measure masked in the inside by a continuation of the general coverings. It is a yellowish shagreened skin, perfectly similar to that of the palate: it is pierced with a great number of small holes, which are the orifices of the glands with which the upper part is furnished. This tongue has the form of the head of a lance: its dimensions in the subject I examined, which was 2·10 metres long, was 0·15 in length by 0·05 at the root. Though it does not project forwards, I have no doubt that it serves to retain and convey the aliments into the œsophagus;

phagus; for it is fixed by its base to the broad piece of the os hyoides. When the latter then is drawn downwards, while the muscular fibres of the tongue at the same time contract, it forms itself into a ball, and, being drawn backwards by the muscles of the os hyoides, it necessarily carries with it in the contrary movement the aliments comprehended between it and the palate.

The os hyoides is composed of three pieces. The largest 0·10 metre by 0·07 is cartilaginous, and resembles the broad part of a wooden shovel: the posterior bottom is round, and the interior straight. The latter is inflected on the convex surface of the large piece, and it is in the groove formed by this inflection that the root of the tongue is inserted. It results from this disposition that the large piece projects beyond the root of the tongue by about a centimetre. This projecting edge or kind of ridge becomes a velum, which, when the os hyoides is carried backwards, closes the whole of the back part of the mouth, and sometimes also the posterior apertures of the nostrils.

Such is the mechanism which allows the crocodile when pursued and frightened, as I have had occasion to observe in Upper Egypt, to shelter itself, and to lie concealed in the river, and to be able to respire in it. It only thrusts out of the water the extremity of its muzzle where the nasal apertures are situated: the jaws are then open, without the water being able to penetrate into the œsophagus and trachea.

The horns of the os hyoides are two small arched and elongated bones about 0·07 metre in length.

The os hyoides is drawn backwards by four muscles, the exterior ones of which are round and the interior flat: it is drawn forwards by the contraction of the tongue.

Perrault gives to the œsophagus of the young crocodile which he examined a greater diameter than to the stomach. He compares the œsophagus of this reptile to the gizzard of a bird which feeds on grain; and he consequently supposes, what would be an anomaly too monstrous to be found in the animal œconomy, that digestion is effected in a great measure in the œsophagus. My observations are directly contrary to those of that celebrated anatomist. I found the diameter of the œsophagus to be 0·06 metre, and that of the stomach 0·17 by 0·15; for the form of this bag is that of an ellipsoid slightly compressed on the sides: in a word, it did not appear to me to have any resemblance to a gizzard. The velvety tunic was exceedingly thick; the muscular part was less so: the inside was filled with a quantity of small pebbles, the polish of which announced that they had served for the trituration of the alimentary matters. The stomach had over it a bag, which was terminated by the pylorus. In the intestines, which were 3·67 metres in length, nothing could be distinctly observed but the rectum, in consequence of its great thickness. The duodenum, a little below the pylorus, was remarkable by a double contour, which it made from the top upwards for the extent of 0·14: its folds, which touched each other, were united by an adipose membrane split in three different places. The rest of the intestines, in which no trace of a cœcum is observed, was strongly attached to the loins by means of the mesentery.

III. *Organs of Respiration.*

The flatness of the tail of the crocodile, and the membranes extended between the toes of its hind feet, sufficiently account for the decided taste which this animal has for rivers; but as the ears and back part of the mouth are each provided with a cartilage, which, when necessary, prevents the introduction of the surrounding liquid, I had reason to expect a similar relation between the pulmonary organs and those of natation. I therefore always wished that I might be able to describe these pulmonary organs as compared with those of other lizards, in order to trace out the most essential anatomical characters by which the genus of the crocodile differs from various other families of reptiles. If I therefore give this description, it is not because most of the preceding anatomists have omitted to do it: on the contrary, we are acquainted with those of Vesalius, sir Hans Sloane, Perrault, Hasselquist, and that more minute by the Jesuit missionaries to Siam, to which Duverney has added, and which might be considered as complete.

The trachea opens in the centre of the broad piece of the os hyoides, and accompanies it backwards nearly (that I may employ the comparison already used) as the handle of a wooden shovel accompanies the lower part. A little before it divides itself into two branches, it is folded back, and turns to the right side, as is observed in several birds. Its length in a straight line, as far as the point of its bifurcation, is 0·38 metre. It is composed of complete, broad, cartilaginous rings, separated from each other by a very narrow membranous ring. I found only the first ten rings complete. Duverney, in the crocodile of the Academy, counted sixteen, the portions of which were united by a membrane. The Jesuits above mentioned found a greater number in the crocodiles of Siam. It is this membrane strongly distended, and made to vibrate in the manner of the parchment of a drum, by the interior air of the lungs, which causes the crocodile to emit that cry, or rather that bellowing noise, mentioned by Catesby, La Coudreniere, and Bartram. The fissure of the glottis is then shut by the muscular roll which borders it on each side.

The lungs are two conical bags, the summits of which are turned towards the head. Their interior surfaces, which rest on the œsophagus, retain the impression of it by a longitudinal furrow. Their length is 0·33 metre, and their breadth at the base 0·22. The figure given by Perrault represents them of an elongated ovoid form.

The lungs of lizards are only two elongated bags, 0·40 in length, and 0·11 in their greatest thickness; the interior sides of which are lined with small reticular carneous fibres and sanguiferous vessels. Those of crocodiles differ by the size of the membranous leaves with which they are furnished, and which form as it were several small walls. It is a vast reticulation, composed of a quantity of meshes similar to those whish are seen in the second stomach of ruminating animals. Each of these meshes is the edge and entrance of a small bag, which opens into a second, and sometimes into a third. They are composed of two kinds of fibres: the first circular, and parallel to each other; the second perpendicular, which transversely intersect the former at right angles. The centre of each pulmonary bag, entirely empty, serves in some measure as a receptacle for the air. The cells in opening become filled with it. They then compress it by shutting, and convey it to the blood, as we may say, without the concurrence of those organs which press on the whole pulmonary mass. They repeat this play until the air contained in the whole lung is vitiated. The crocodile, then, is not forced to come and respire at the surface of the water till after a certain time has elapsed. This structure of the lungs, which makes the crocodile deviate from lizards, brings it near to the sea-tortoise. I shall have occasion hereafter to remark, that this is not the only relation which it has to these animals.

IV. *Of the Organs of Generation.*

These organs are so complex, and have so little relation to what is known in the mammalia, that authors, as we may say, have been afraid to describe them, and have scarcely given a slight sketch of them. It has been said that the crocodile is only a lizard of a monstrous size, and Linnæus has consequently arranged it in his system under the genus *lacerta.*

lacerta. What I have already said of the configuration of the head and lungs of this reptile removes it no doubt from that genus, but the consideration of the organs of generation will obviate all uncertainty in regard to these natural relations.

Most lizards, like serpents, are furnished with two yards, situated on each side of the anus. Properly speaking, they are only two cavernous bodies, formed by a slight cutaneous expansion, and terminated by two cartilaginous appendices. In the inside of them are found two glands, which pour forth a liquor in such abundance, that it has given rise to mistakes in regard to its nature, and made it be considered as the seminal liquor. These yards move in a sheath formed by a duplicature of the skin: they are terminated behind by an elongated muscle, always inclosed in a membranous vagina, which, by contracting, forces them to re-enter.

If the crocodile retains any part of this general plan of organization, the combination is quite different. It has only one yard lodged at the anterior part, and in the fold of a common cloaca: it is imperforated, entirely cartilaginous, and terminated by a kind of gland 0·03 in length. It has two glands on the sides of the anus, from which oozes a whitish liquor through two distinct orifices at a considerable distance from each other. These glands produce no protuberance, and yet the retractor of the cavernous bodies of lizards exists. It is even of so considerable a volume (0·40 in length, and 0·11 in its greatest thickness), that it is this muscle with its congenerate that swells up the anterior part of the tail, so that it cannot be distinguished by a diminution of volume from the rest of the body. This muscle is terminated by a sharp edge, or ridge, where it is articulated with the caudal vertebræ, and by a free and round edge on the opposite side. What is remarkable besides is, that it is contained, like the retractor muscle of the cavernous bodies, in a proper sheath of great thickness, and of a cartilaginous nature. This sheath is continued forwards in an aponeurosis, which spreads and is inserted on the pelvis; so that, as the uses of this muscle change with the general system of organization, they are confined to contributing merely to the lateral motion of the tail.

The testicles in some measure approach near to those of fishes: they are narrow and elongated. They are observed a little above and before the kidneys.

The semen is conveyed in two pretty large vessels, contiguous, and lodged behind the common cloaca. These vesicles are in part closed by a cartilaginous bag: they open into the common cloaca by six or seven holes on each side, disposed in a circular manner around the urinary passage.

V. *Of the Liver.*

The liver is composed of two unequal lobes : one of them has the form of a parallelopipedon (0·14 metre by 0·09) ; the other is slender and more elongated (0·19). This viscus exhibited a very remarkable organization, which has never yet been noticed by any anatomist. The convex surface of each lobe is covered by a membrane, which is the aponeurosis of a muscle, the use of which I can hardly comprehend. This muscle, which begins at the posterior and inferior edge of each lobe, is inserted very near the pelvis, in the last piece of the sternum; for it must be recollected, that the latter is prolonged beyond the ribs, and terminates in a large piece articulated with the bones of the pelvis. These two muscles, which have not yet been found in any other animal, produce by their contraction the depression of the liver, and thereby give more capacity to the breast. This use makes them have an affinity with the diaphragm : the points to which they are attached might induce a belief of the same thing*. The gall-bladder is ovoid, 0·08 of a metre by 0·03, and adheres to the right lobe of the liver.

VI. *Of the other Viscera.*

As these have been already so well described by most of the authors before mentioned, I shall give only their relative positions, because this information may furnish some useful hints for determining the different kinds of crocodiles.

The *heart.*—Its height is 0·07 metre, its base 0·05. The right auricle is larger than the left.

The *spleen.*—Oval elongated, 0·10 by 0·04. On the inferior face it is somewhat concave ; and on the upper rises into two ridges, one of which is very small.

The *kidneys* are composed of papillæ, and numerous sinuosities formed by a collection of glands, 0·11 metre by 0·05½.

I had not resolved to publish these observations till after my return from Egypt; and at that time, notwithstanding the learned researches of several of my colleagues, they had still retained all their novelty. The object of Cuvier's excellent memoir is merely to establish the real differences between the crocodiles of the new continent and those of the old; and Daudin* has endeavoured, in particular, to enrich the history of the crocodile from the relations of travellers before unknown.

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* C. Cuvier observed these muscles in the crocodile of St. Domingo. He proposes to describe them at more length in his *Comparative Anatomy*.

Since I have occasion to quote the latter work, I must rectify an error which concerns me, and which C. Daudin introduces in consequence of the respect which he is pleased to entertain towards me. He announces that "I attempted, during my stay in Egypt, to tame crocodiles after the example of the antients, and that my attempts were not crowned with that success which I expected." It is a duty which I owe to truth, to assert that I never made any attempt of the kind.

The following is the circumstance that gave rise to this report, which was indeed circulated at the time of our triumphs. The period when the army of the East had at its head a chief worthy of its great exploits, the English sent to combat us could then find no opportunity of gratifying the desire of injuring us with which they were tormented. Tired of cruising backwards and forwards to no purpose, they wished to amuse themselves, and thought they could reach us with very feeble weapons, by endeavouring to turn into ridicule the principal persons in the army. They made some caricatures, which they sent to England, and which were thence conveyed to France. I had the honour of attracting their notice. They introduced into the scene several crocodiles; and this ephemeron production gave rise to the mistake to which I allude.

Explanation of the Figures. (Plate III.)

Fig. 1. A A, the lungs.
B, the pericardium.
C C, the two lobes of the liver.
D, the diaphragmatic muscles.
E, the stomach.
F, the intestinal canal.
G, the sternum and its muscles seen interiorly.
H, the same organs seen exteriorly.

Fig. 2. The cranium of the crocodile of the Nile. It is here represented,

1st, To give an idea of the manner in which the crocodile raises its upper jaw on the inferior.

2dly, To show the two condyles of the horns of the temporal bone, and the cavity with two facets, where they are articulated.

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* *Traité de Reptiles*, forming a continuation of the works of Buffon.

3dly, To render sensible the differences which exist between the large teeth of the crocodile of the Nile and those of the crocodile of St. Domingo.

XXIV. *Observations and Experiments on the Light emitted by rotten Wood, in the different Kinds of Gas, and in Fluids. By* C. W. Böckman, *of Carlsruhe.*

[Concluded from p. 26.]

Experiment XVII.

I introduced phosphorescent wood under the receiver of the air-pump, placed a bit of it in a glass filled with water, and another piece on a dish not immersed in water. When the air began to be rarefied, a quantity of air not inconsiderable issued from the wood. The light of the wood seemed to be somewhat weaker than at first, and proportionably less than that of a piece placed without the receiver. When the quicksilver in the barometer connected with it sunk about four lines, on the re-admission of atmospheric air I thought I observed a considerable increase in the phosphorescence. The piece of wood immersed in water was entirely penetrated by that fluid: it therefore fell to the bottom, but emitted as strong a light as at first. This experiment was several times repeated with the same result.

Experiment XVIII.

Oval glass flasks, capable of containing five cubic inches, and of equal thickness, were filled; some with oxygen gas, some with azotic gas, and some with carbonic acid gas, and well stopped with corks, to which were affixed wires supporting pieces of phosphorescent wood. I then introduced all these vessels at the same time into water at 70° of Reaumur. In about three-quarters of a minute the light in the carbonic acid gas perceptibly decreased, and then that in the azotic gas; in one minute and a half it appeared weaker in the oxygen gas; and in two minutes and a half the least phosphorescence was not to be observed in any of the vessels. It could not afterwards be revived in any of the pieces of wood by any means whatever. This experiment was also repeated several times with the like result.

Experiment XIX.

Rotten wood emitted light in spring water, in boiled water, and in distilled water, kept in a close vessel till the experiment was made at the temperature of from 8 to 12° of Reaumur, the same as in common air. The phosphorescence did not become weaker till the end of several hours, and in twenty-four hours it completely ceased. It could, however, be in part revived by wrapping up the pieces of wood in filtering paper; but when the water had the temperature of 45° the light was speedily extinguished, and could not be again revived.

Experiment XX.

Having introduced wood strongly phosphorescent into sulphurous acid, nitrous acid, and muriatic acid, the light either instantaneously ceased altogether, or in one or two minutes, and could not be again revived in any manner.

Experiment XXI.

I added a considerable quantity of water to the above acids, in such a manner that only a few drops of acid were put into a cubic inch of water. The light of the rotten wood decreased in three minutes, ceased in general in from six to ten minutes, and was not again visible when the wood was washed with water in the atmospheric air.

Experiment XXII.

Rotten wood appeared phosphorescent in muriate of ammonia, nitrate of potash, common salt, and tartaric acid; and even somewhat stronger in the second and third solution than in atmospheric air. The light continued longer than in common spring water.

Experiment XXIII.

The phosphorescence of rotten wood continued in a dilute solution of carbonate of potash as well as in liquid ammonia two or three minutes, and then became entirely extinct. In linseed oil the wood appeared phosphorescent without any diminution eighteen hours, and in thirty was entirely extinguished.

Experiment XXIV.

I immersed phosphorescent wood in spirit of wine, and found that it was extinguished in from four to eight minutes. In sulphuric ether the light strongly decreased after twenty minutes, and in a short time entirely disappeared. It could not be again revived by the common mode of treatment.

If these experiments, which I was not able to carry any further for want of phosphorescent wood, be compared with the experiments of other observers, the following differences will be found:

1st, According to the above experiments, rotten wood does not emit so remarkably clear a light in oxygen gas as Spallanzani observed. The light also was not so speedily extinguished, that is, in scarcely a minute, as found by Tychsen. And as my observations perfectly coincide with the experiments of Humboldt and Gartner, Spallanzani's and Tychsen's observations, which I will not venture to doubt, must have been attended with peculiar circumstances.

2dly, In regard to the diminution of the volume of gas, M. Gartner found it once in oxygen gas two-thirds of the whole. According to my experiments, however, it was always much less.

3dly, The residuum of the oxygen gas, in which the rotten wood had ceased to be luminous, was found by Gartner, on being subjected to proof, of such a nature that one measure of it mixed with a sufficient quantity of nitrous gas gave a diminution of 207°. I however found the diminution of the residuum in a bell glass to be 120°, and that in another only 21°, though our oxygen gas was prepared from the same material, namely, oxide of manganese, and of a quality equally good. This difference, perhaps, was owing to a difference in the size of the vessels employed, and even in the rotten wood.

4thly, The sudden extinction of phosphorescent wood observed by M. Humboldt in carbonic acid gas purified by means of phosphorus, I did not observe in exceedingly pure gas treated in the same manner.

5thly, M. Humboldt observed the same sudden extinction in azotic gas which had been freed by means of phosphorus from any oxygen it might contain. But this I was never able to observe, either in azotic gas as pure as possible, or in the same gas exposed to the contact of phosphorus.

6thly, In these experiments I did not pay very great attention to the absorption which might take place of the different kinds of gas, because in researches of this kind in regard to rotten wood no accurate results can be obtained. I however, in general, observed no diminution in the azotic gas; which, on the other hand, according to Tychsen's observations, was greater than that experienced by oxygen gas prepared from saltpetre.

7thly, M. Humboldt found that rotten wood was luminous only for a very short time in oil; whereas Carradori's experiments and mine show the contrary.

8thly, According to M. Gartner, a piece of rotten wood continued luminous in hydrogen gas in which another piece had emitted light, a much shorter time than in gas which had not been before used. I do not remember to have ever found any remark of this kind in the works of others; and as I paid particular attention to this point, in regard to every other kind of gas, I did not find Gartner's observation confirmed. A great part of these variations, however, are probably owing to this circumstance, that the wood employed was not of the same kind, and had not the same degree of rottenness and moisture. All our experiments, however, coincide as much as could be expected from the use of a substance such as phosphorescent wood.

If we still further compare the phænomena of rotten wood with those of phosphorus, the following differences will be observed between these substances:

Phosphorescent Wood,

1st, Is luminous in oxygen gas at low temperatures.

2dly, It is phosphorescent in all non-respirable gases, at least a short time, and in several of them in a pretty continued manner; as in phosphorized oxygen gas, and phosphorized azotic gas.

3dly, In muriatic acid gas its light is soon extinguished.

4thly, Its phosphorescence in rarefied air is weaker.

5thly, It emits light in a Torricellian vacuum, according to the testimony of Carradori.

6thly, Its light becomes extinct both in oxygen gas and in other gases when heated.

7thly, By the process of its phosphorescence in oxygen gas carbonic acid gas is produced.

8thly, The phosphorescence of rotten wood can be extinguished several times successively in the non-respirable gases, whether they contain oxygen gas or not, without the property of the gas to maintain the phosphorescence of a new piece of wood introduced into it being perceptibly lessened.

9thly, Moisture promotes the phosphorescence of rotten wood, and is essentially necessary for that purpose.

10thly, Rotten wood emits light also under water, in oil and in other liquids. Its splendour in some of them is even heightened.

Kunkel's Phosphorus.

1st, It becomes luminous in oxygen gas only at a temperature of about 16° to 22° of Reaumur.

2dly, Of all the non-respirable gases as pure as possible, it is luminous only in azotic gas, oxidated azotic gas, and muriatic acid gas.

3dly, In muriatic acid gas it inflames immediately of itself, and burns with great brightness.

4thly, The light of phosphorus is stronger in rarefied air.

5thly, It emits no light in vacuo.

6thly, On being subjected to heat, it inflames and burns with rapidity in oxygen gas; and in the non-respirable gases, not pure, its light is stronger.

7thly, By its phosphorescence in oxygen gas no carbonic acid is formed.

8thly, When artificial phosphorus has emitted light in the non-respirable gases not perfectly freed from oxygen gas, a fresh piece of phosphorus does not become luminous in them: azotic gas, however, is an exception, in which, after being purified, phosphorus becomes luminous for some time.

9thly, Moisture and wet are impediments to its being luminous.

10thly, Fluids are altogether contrary to the luminous property of artificial phosphorus.

Phosphorescent wood, therefore, differs essentially from artificial phosphorus by the conditions requisite for its being luminous; and therefore the assertion of Spallanzani, that the greatest analogy exists between the luminous phænomena of these two substances must lose some of its weight.

If I mistake not, the following probable conclusions may be deduced from the above experiments: that the extinction of the light of rotten wood in different mediums does not so immediately arise from a want of oxygen gas as from some change which the wood itself has experienced. For even if, according to the opinion of Humboldt, Spallanzani, and other philosophers, the oxygen gas concealed in different mediums be the immediate cause of its phosphorescence, in several experiments where rotten wood was immersed in different mediums I must have observed no light: at any rate, the phosphorescence must always have been weaker, and fresh wood repeatedly introduced would no longer have been luminous in it; which is contrary to what I experienced.

Besides, how difficult would it be to prove, with any degree of probability, that the undiminished phosphorescence of wood, which is observed in distilled water and in oil, arises from a small quantity of oxygen gas in these fluids? And is the luminous wood, placed in these fluids, capable of decomposing oxygen gas with which it is so little in contact,

tact, and which, as a component part in atmospheric air, must adhere to the azotic gas, and which is also in combination with the fluids themselves? Is artificial phosphorus, which in all probability has a greater affinity for oxygen than luminous wood, able to decompose the oxygen gas in water? May not its being oxidated under water arise rather from the slow decomposition of the water itself, than from the decomposition of the gasiform oxygen contained in it? If otherwise, phosphorus would be as luminous in water as wood; which, in the course of my numerous experiments, I did not find to be the case, though I had often in my hand flasks which contained half a pound of partly fresh melted and partly weak and strongly oxidated pieces of phosphorus: and in regard to the luminous stars which Messrs. Scherer and Jäger have described, and which I observed in boiling water in which phosphorus was put, I consider them to have been fine phosphoric particles which emitted light in the small air bubbles separated by means of the heat from the water. I observed also in the dark, on opening the above flasks, in which phosphorus had remained for some months, luminous vapours often arise from them. This luminous appearance, however, did not take place in water free from the contact of atmospheric air. It appears to me also, in consequence of several experiments, made with great care, on the quantity of oxygen gas decomposed during the combustion of phosphorus in atmospheric air, or in non-respirable gases in which a certain quantity of oxygen gas is mixed, possible to determine, in a certain degree, from the known quantity of the surface of the phosphorus, from the duration of the light and its intensity, how much oxygen gas is actually decomposed; and if this idea is not entirely groundless, it appears to me very improbable, that in the interstices of distilled water as much gaseous oxygen exists as is sufficient to account for the phosphorescence of rotten wood in it, according to the conjecture of M. Humboldt; and therefore it appears to me more probable that wood, to produce its phosphorescence, is in no immediate need of oxygen. I was induced, by some expressions of M. Humboldt, to enlarge more on this point than I otherwise should have done.

I am not much inclined to believe that wood, as a luminous substance, produces the observed diminution of oxygen gas. According to my opinion this is occasioned much more by the degree of the rottenness, as during all processes of fermentation and putrefaction the above gas is decomposed. M. Humboldt does not appear to me to have

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adopted a proper method, when, in some of his processes, he destroyed the phosphorescence by sudden heating, in order that, by comparing it with that of other wood not treated in the same manner, he might observe whether it effects the diminution of the gas as a luminous or as a putrescent substance. For it appears to me very probable, that by such a violent exaltation of temperature, besides the phosphorescence, the previous degree of putridity is changed, and consequently that this wood can neither decompose nor absorb any more oxygen gas.

This alteration of phosphorescent wood, by means of which its light is more or less speedily checked by certain mediums, appears to me, with some probability, to depend on the circumstance whether they are calculated more or less to check the putridity, or, on the contrary, to promote it. The putridity, therefore, and the phosphorescence connected with it, must continue not only in oxygen gas but also in atmospheric air, and in weak solutions of muriate of soda and nitrate of potash. Like the phosphorescence of wood, it is more or less checked by want of oxygen gas, and consequently in all the non-respirable gases, and particularly in nitrous gas, and in carbonic acid gas on account of its peculiar property of opposing putridity; also by exposure to heat, on account of the desiccation connected with it; by concentrated or diluted acids; by tartarous acid, &c. The latter acts, perhaps, so far as it speedily attracts the water, and therefore desiccates the rotten wood. But whether sulphurized hydrogen gas, ammoniacal gas, muriatic acid gas, which are all speedily miscible with water, exercise a prejudicial action on luminous wood by absorbing its moisture and acting in the same manner as fluid ammonia or acids; or whether, by means of a peculiar antiputrescent property, they extinguish the phosphorescence so speedily, I will not venture to determine.

In a word, this theory harmonizes pretty accurately with the well known experiments on phosphorescence, which continues in mediums that promote putridity, and is interrupted by fluids that oppose it. According to the abovementioned opinion of Carradori, this philosopher seems to have considered such action as possible, but he does not assign any cause.

The following observations of M. Humboldt, which he wrote however for a totally different object, I found so applicable, with a few variations which may be easily made, to the conjectures I have expressed on the luminous appearance of rotten wood, which continues in some mediums and in others is weakened or completely checked, that I consider it as my duty here to insert them; for coincidence with the unprejudiced opinion of a great man is always advantageous to a writer, and when thrown into the scale adds no small weight to an assertion. M. Humboldt says, "When the equilibrium between the component parts of organized matter is destroyed, and the important chemical process of putrefaction begins, it is variously modified by the temperature and nature of the surrounding mediums. Every fermenting substance, therefore, changes every moment the state of its mixture; and as its natural phosphorescence depends on this change of mixture, every thing that relates to the one must increase or destroy the other. There are two conditions, therefore, under which rotten wood is extinguished, one of which has an immediate and the other a mediate action. The first is only oxygen gas; the other, heat, oil, acids, alcohol, &c. Decomposition with the disengagement of light ceases until a new accession of oxygen gas. But the putrescent substance in contact with oxygen gas is brought to a new state of mixture by elevation of temperature, and quits its former state of putridity, which is an essential condition of the disengagement of light."

If the determinate degree of putridity is totally changed or destroyed by certain mediums, the extinguished light of the wood cannot be again revived; but if these fluids have exercised on the wood only a weak action and for a short time, and if the degree of the putridity be therefore only as it were superficial, the phosphorescence in this case may be again revived and strengthened by means that promote putridity, and consequently by moistening, exposure to atmospheric air, &c.

I, however, freely confess that it still appears to me difficult to explain, in a definitive manner, how wood in this phosphorescent state is decomposed, and what its luminous appearance really is. I have however formed several ideas on this subject, but they do not appear to me to be yet fit to be laid before the public. Nor will I venture to determine how near the truth the opinions of Spallanzani, Carradori, Humboldt, and Gartner, approach. I am inclined to think that time and experience are still necessary to bring them to maturity. This much, however, is certain, that to produce such phosphorescence several particular circumstances are necessary, otherwise this phænomenon would occur much oftener in nature.

XXV. *Conjectural Observations on the Mammoth. By* G. J. WRIGHT, *Esq.*[*]

THE direct or indirect benefit of mankind is universally allowed as the end of the creation of those various classes of animated beings which inhabit our globe. Among these some appear to be less entitled to arrangement under this axiom than others; or, in other words, the benefits to be derived from a certain class may appear more than counterbalanced by the evils incurred by their ravages: but, happily for us, the disproportionate increase of the various species of such, and particularly of the more destructive, is in a great measure prevented, not only by the indiscriminating rapacity of animals whose superiority of rank carries certain destruction wherever they haunt, but also by that wise provision of nature, which, the more effectually to curtail the diffusion of destructive animals, implants in other orders a peculiar specific and native antipathy, prompting them to unceasing warfare against such their appointed prey, whose undue increase, if not by some appropriate method prevented, would in process of time probably depopulate the world.

As then (for reasons which it is not our province to analyse) mankind are exposed to the ravages of such destructive beings, we must necessarily allow that class as most to be dreaded, against which we are acquainted with no effectual means of opposition from other animals, not excepting even man, who, though placed in the world as the lord of the creation, unassisted by those skilful manipulations in the arts by means of which he ranges unhurt amidst a thousand dangers, actually becomes the prey of tribes over which he is destined to rule.

If the above precautions are necessary in an æra when the greater part of the globe is replete with inhabitants, how much more requisite in the earlier ages, when, population being at a low ebb, the continuance of the human race in far separated climes may have hinged on the preservation of a few, and that at a time when these scattered individuals were unfurnished with the implements of security of which modern improvements have put us in possession!

And can we suppose for a moment, that while the increase of less hurtful creatures is limited by various means, the disproportionate extension of a class against which neither brutal ferocity nor human skill can avail, should be left overlooked, and not provided against? I say, if we assent to such a supposition, we form a chasm in zoological œconomy which the wisdom of nature throughout her various analogies stamps as unwarrantable.

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[*] Communicated by the Author.

So important a query, one would imagine, would scarcely have been left undiscussed, as applicable to so formidable a race of beings as the serpent tribe, against whose undue increase*, however desirable, no effectual bar seems to be provided, no means of evasion successful, and against which the opposition of other animals is attempted but to their certain fatality.

In the primitive ages of the world, when the paucity of mankind must have allowed of the unlimited extension of all the classes of inferior beings†, the tribes of the serpent kind, if no natural means of opposition from other animals were furnished, must have increased to an incalculable degree both in numbers and size. Combining in themselves at once insatiable rapacity with incredible abstinence, their age and growth apparently unlimited, their amphibious nature allowing an extension of their ravages to the inhabitants of the watery element as well as those of the land, and at the same time suffering them alike to evade the premature view of their prey; with such noxious qualities they hold a scale in the creation perhaps not less formidable than would appear the combined disadvantages of all other ferocious beings. Nor can we suppose that the general deluge, which at once destroyed the several tribes of land animals, at all diminished the numbers of the serpent kind. We have no reason to conclude that amphibious animals were included in the ark any more than fish; and although naturalists agree that the largest species of serpents are most frequently found in fresh water, yet it does not follow that so weak a saline solution as the mixture of the rain with the waters of the ocean, which together composed the waters of the deluge, should prove disagreeable to them: it is true, some suppose the sea to have been more saturated with saline matter in the primitive ages than since‡, on account of the subsequent mixture of the quantity of rain which fell during the awful crisis alluded to: but I am far from adopting that opinion, although it would not militate against the supposition above hazarded; for, if a class of creatures not included in the ark escaped the ravages of the deluge, it follows that, if the same are still in existence, the waters which composed the deluge, whether saline or not, did not prove deleterious to them *.

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* The smaller species casually become a prey to carnivorous birds and animals.

† The Indian missionaries reported that vast tracts of that country had long been uninhabitable by reason of the increase of the brute creation, so that whatever man cultivated for the support of life was destroyed without possibility of prevention.—*Encyclop. Britan.* vol. v. p. 743.

‡ Kirwan's Geological Essays, p. 377.

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* The salubrity of water not depending on its containing salt, but on its continual agitation, my own opinion is, that the sea was scarce at all saline before the deluge; and as it is necessary we should find some source for the saline matter therein contained, which the supposition of its rise from saline springs running into it, or mines of salt under its bed, is found inadequate to, so I imagine that the origin of the formation of muriatic acid (which combined with the alkaline and earthy matters at the bottom of the ocean forms the several marine salts contained in the same) is due to the putrefaction of the various animals floating in the ocean: but of this we must remain ignorant till experiment shall develop the actual elements of this acid so peculiar to marine situations: it is certain that wherever the external agents are most favourable to the putrefying process, there the sea is most salt; and this, as I think, depending conjointly on the greater evaporation which must needs happen wherever the temperature, as in hot climates, is most favourable to this process, together with the more speedy putrefaction of all animal and vegetable matters in equatorial latitudes. The deluge, in an indirect manner, was certainly a means of destruction of myriads of shell-fish, which, imbedded in the mire covering the surface of the earth, were by the induration of the same completely hemmed in on every side, forming extensive beds of that mixture of shells and earthy matters so frequently met with: but fish provided with fins to rise to the surface of the water, as also all that have the faculty of swimming, could not be included in this general destruction: hence the tribes of serpents and water-snakes would not be diminished by the waters of the deluge; which implies a still further necessity of some effectual means of their diminution; otherwise their numbers would present a most formidable obstacle to the new peopling of the earth after the deluge. With regard to fresh- and salt-water fish, though the former could not perhaps endure for a length of time so concentrated a saline solution as is our present ocean, yet we may reasonably conclude that both classes could exist in, and become naturalized to, a slightly saturated mixture, as must of necessity have been the waters which composed the general deluge.

In conformity with the above theory I consider the origin of sea salt to be in the sub-aqueous putrefaction of animal matters, especially during the decline of the deluge; and so far from the ocean being supplied from mines and springs of salt, I imagine the former to be merely the product of filtration, during the very gradual subsidence of the waters of the deluge into immense cavities, (which must needs have been found by the falling in of vast masses of earth, whose support worn down by the softening quality of the water no longer could resist the incumbent weight,) and that of the latter to arise of course from currents of fresh water, which, occasionally in wearing themselves an outlet, find a passage through these same primitive depositions of sea salt.

If then the disproportionate extension of the serpent kind is not provided against by any known means, and we allow as we do the superior necessity of some effectual restraint to the same, it behoves us to attempt to define the probable structure and habits of the animal which should be best able to quell this class of reptiles, not less prolific in themselves than they are obnoxious to all other creatures.

Such an one I presume to be the mammoth, whose stupendous appearance gives great air of probability that he is destined to oppose a class of beings unconquerable by other means. His allowed amphibious nature renders him a fit opponent to the serpent tribe: the position and form of his ribs, so well adapted to resist external pressure, render him calculated to oppose this class of reptiles, whose efforts to vanquish their prey are confined to attempts to entwine the same, and crush, as they do by their convulsive writhings, the bodies of the largest animals we are at present acquainted with. Nor can this strength, indicated in the position and structure of the ribs of the mammoth, be supposed to be for fitting him for resisting the pressure of water merely, since many of the tribes of fishes which visit the deepest parts of the ocean are unprovided with a similar barrier * for that purpose; at the same time that most of such as are furnished with ribs have them barely more than cartilage.

The tusks of the mammoth are admirably situated for tearing up bushes and digging in morasses, the better to dislodge his prey, the which if of shell-fish, as has been suspected †, would doubtless exhibit his teeth more abraded than they appear to be; nor is there in our present supposition concerning his diet any necessity for canine teeth, the absence of which in the mammoth gives great countenance to the idea that his food has been of a more soft consistence than the muscular flesh of land animals: the shortness of the neck would appear a wise provision in the case

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* Many fish are without ribs; such are rays, sharks, pipe-fish, sun-fish, porcupine-fish, lump-fish, &c. &c.—*Cuvier, Comp. Anatomy.*

† Several reasons lead me to object to the idea of fish being the ordinary food of the mammoth. Both shell and flat fish inhabiting solely the water, the animal which might be designed to feed on such diet would doubtless be an aquatic and not an amphibious animal. In the class of amphibia it is observable, that according as either the water or the land is most genial to the habits of any one species of these, so does their structure more or less verge toward that of the fishy tribe, and the contrary. It therefore reasonably follows, that the anatomical structure of the mammoth, if destined to live on fish, and of course in the water, should approach more nearly to other of the aquatic class than its skeleton appears to do.

of its combating with the larger serpents, as the neck appears the only portion vulnerable in the mammoth by the writhings of those reptiles, which if perhaps not well defended by the tusks of the former, might endanger its strangulation: the prominent ridgy back, and allowance of considerable motion on the part of the head, would certainly be favourable circumstances when viewed in the present light. Whether the hide of the mammoth may or may not have been susceptible of being pierced by the teeth of serpents I will not pretend to conjecture; but if it were so, it is to be recollected that the bite of venomous serpents is dangerous in proportion as the animal bitten is more or less of a hot-blooded temperament[*]: hence the amphibious nature, together with the food of the mammoth, rendering him a cold-blooded animal, fits him still better as the opponent of the serpent tribe. 'Tis most probable, that as the quickness of sight in serpents is even proverbial, the enemy of these formidable reptiles would be endowed with equal perfection in some other organ the better to search them out: perhaps the notorious odour of the majority of serpents will warrant us in concluding that the organs of smell would be destined for that purpose. As a serpent when desirous of evading the view of its natural enemy would most probably retreat to the neighbouring swamps, burying himself with all possible speed therein, so the latter should be capable of exerting a considerable degree of activity for at least a short space of time; and in this respect also the mammoth well corresponds.

With regard to the strength of the opponent of this tribe of deleterious creatures, we must at once see the propriety of it; but referring to his bulk, it certainly appears not so immediately necessary, were it not that, upon more mature consideration, it seems requisite that this opponent of the serpent kind should feed on the flesh of the same; otherwise the putrefying carcases of so bulky a class of beings as many of these are, would prove perhaps as deleterious to the inhabitants of the vicinity, human or brutal, as though their destruction had not taken place; not to mention also the necessity of his feeding on some such kind of diet to stamp him as a cold-blooded amphibious animal: otherwise he would not be adapted for the purposes of his creation, nor could he under contrary circumstances feed on the flesh of the venomous classes without deadly hazard.

If the mammoth is allowed to be the natural enemy of the serpent tribe, it may be asked, why is he not in existence at this time for the same purpose? why is he peculiar to the new world, while serpents have always alike abounded throughout the globe? and whence comes it that the remains of the mammoth have been usually found in higher latitudes than are congenial to the constitution of the majority of the serpent tribe? In reply to these queries I observe, that we have no proof that the mammoth is not at this present time in existence, although in situations remote from the view of man, as the classes of serpents are also observed to retreat in proportion as the population of the climates they inhabit increases. But admitting that the mammoth has become extinct for some centuries, it argues not against the present conjectures; for like instances have occurred in other classes *, and this especially, as, however important the restraining of the propagation of the serpent kinds might have been in the earlier ages, it has been much less so in subsequent periods, when mankind have little to fear from the ravages of the brute creation.

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[*] Asiatic Researches, vol. vi. p. 110.

Yet I confess I suppose the mammoth to be now in existence in the north-west parts of America, and perhaps equally savage and unpeopled regions of the old world; but till forced by famine to quit the accustomed haunts of his prey we must not expect to be visited by him: nor need we wonder that no accounts of such a formidable being have been handed down to us, as it is probable that few, and those perhaps the most ignorant and superstitious of savages of former times, ever witnessed the sight of this stupendous animal.

That the mammoth is peculiar to America is solely a conjecture. Till minute investigation warranted a contrary information, the various huge bones which have been at times discovered in different parts of the world have been uniformly referred to the elephant †; the which, if more carefully examined, would doubtless many of them be found to belong to the mammoth class. The bones of elephants, bears, whales, &c. &c. have been found in territories foreign to their temperament ‡; so also those of the mammoth may doubtless be expected to be found in most climates of the world.

That the remains of the mammoth should be met with in latitudes higher than the usual haunts of his prey, as supposed in the case of serpents, is not to be wondered at; for an animal, unless worn out with age, or prematurely de-

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* Plin. Hist., Nat. lib. 8.

† Phil. Mag. vol. xv. p. 327.

‡ Kirwan's Essays, p. 78.

stroyed, must not be expected to die in the midst of plenty. But, in fact, the serpent tribe inhabit every climate more or less: hence it is probable that the rapacity of the mammoth, no longer to be satisfied with the sparing diffusion of the serpent class in the tropical countries, which for a lapse of time had supplied them with their, perhaps, only diet, reduced by their ravages to almost perfect extinction, induced the mammoth to visit regions unfavourable to his constitution, or at least to that of his prey: hence, in process of time, the class would be, perhaps, wholly annihilated through want of food, and the ills of an inhospitable climate. But such as have lately been found on the banks of the Ohio must indubitably have perished there through some sudden and unnatural cause, perhaps a partial deluge or hurricane; for, if an animal dies by natural causes, the carcase remaining on the surface of the earth putrefies and disappears, the skeleton only remaining, which in its turn also moulders away, leaving the teeth, as the portions which longest resist the action of air and moisture: but so small a substance as a tooth of the mammoth, especially if lying in swamps and morasses, may long escape detection, even till such time as the decomposition of its constituent parts leaves no further trace of its original form.

Thus, the mammoth may have existed in the old world as well as the new, although its remains should never have been found in the former: a circumstance, as above affirmed, dependent on the nature of the soil and situation of the place wherein the same may have perished, conjointly with the manner in which its destruction may have happened. Thus, if the period of the mammoth be referred to some centuries back, we cannot expect to find its remains except in situations unfavourable to the process of decay; that is, totally defended from the action of external agents: of course such only must be expected to be met with as have perished in warmer climates by some convulsion of nature, or in those frigid regions where putrefaction is unknown.

But now that naturalists are perfectly convinced of the existence of the mammoth, the remains of huge animals will not, as formerly, be referred to the elephant tribe without more accurate examination; the result of which will, in my humble opinion, prove that the mammoth has existed in most climates of the world; holding a scale in the creation not less beneficial to the animal world at large, than illustrative of those wise dispositions of the Creator which at once draw forth our admiration and gratitude.

Kennington Croſs,
June 18th, 1803.

XXVI. *Ca-*

XXVI. *Catalogue of Animals belonging to the Class Vermes, found on the Coasts of Scotland. By* ROBERT JAMESON, *F. R. S. F. A. S. Edinb. F. L. S. Lond. Honorary Member of the Royal Irish Academy, &c.*[*]

THE animals belonging to the class vermes, although holding so low a rank in the animal kingdom, are in many respects deserving of our particular attention. First, they point out to us the intimate connection that exists between the animal and vegetable kingdoms. It has been supposed by many naturalists, that the most perfect plants, and the least perfect animals, form the link by which these two great classes of organic beings are connected together. On a nearer examination, however, we discover that their greatest approximation is in the lowest members of each class; in the vermes of the one, and the cryptogamia of the other.

2dly, By tracing the difference of their structure, from the most simple of all the monas to the more complicated mollusca, many curious anatomical discoveries have been made; and there is reason for believing that the study of their œconomy will throw light on philosophy.

3dly, By an extensive and critical acquaintance with the animals of this class, the geognost will be enabled to determine many of the petrifactions he meets with; and this knowledge will also, when other data fail, enable him to determine with certainty to what formation the rocks that contain these petrifactions belong.

On the natural history of the British vermes little has been written. Pennant's outline contained in his British Zoology, and Ellis's excellent treatise on Corallines, are almost the only works that treat professedly on this subject. Dr. Shaw, by means of his interesting work The Naturalist's Miscellany, has contributed much in exciting a taste for such inquiries; and it is to be hoped that the concluding volumes of his great work on zoology will be rich in information respecting this interesting class of animals. The chemical analysis of several has been very ably executed by Mr. Hatchet, as detailed in his masterly memoirs in the London Transactions.

In the catalogue I now communicate I have only mentioned those species that are rather rare on our coasts.

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[*] Communicated by the Author.

MOLLUSCA.

Tritonia.

T. papillosa. *Cuvier.*	Doris papillosa of Syst. Naturæ.
verrucosa. Ib.	verrucosa. Ib.

Both of these species are found on the rocks on Leith shore.

Doris.

D. argo. *Lin.*	This beautiful species occurs but rarely on our coasts. I have hitherto only found it on the sea-rocks in the neighbourhood of Leith.

Aphrodita.

A. scabra.	Leith shore.
squammata.	Ibid. and Orkney islands.
imbricata.	Ibid. ibid.

Amphitrite.

A. ventilabrum.	On sea-rocks near Queen's Ferry, on the banks of the Forth.
cristata. *Muller.*	Leith shore.

It is interesting to observe the gradation from the coarse tube of the animals of this genus through all the varieties of more regular form observable in other mollusca, and in many insects, to the finished cell of the bee. Dr. Steffens is of opinion that these different figures are caused by the crystallization of the exuded matter. The reader is referred for the proofs of this opinion to his Beiträge zur Kenntniss der innern Naturgeschichte der Erde.

Ascidia.

A. rustica. *Muller.*	Adhering to the roots of the fucus digitatus, on Leith shore.
prunum. *Id.*	Attached to fuci. Ibid.
conchilega. *Id.*	Ibid.

Actinia.

A. gemmacea. *Ellis.*	Leith shore.

Pedicellaria.

P. globifera. *Muller?*	On Leith shore, adhering to fuci.

Muller's globifera is characterised "capite sphærico, collo nullo." The species found on Leith shore wants the neck, and may probably turn out a new species.

Medusa.

M. æquorea. *Muller.*	Seas around the Shetland and Orkney islands.

The animals of this genus possess a powerful stinging quality, and emit a strong alkaline odour. Dr. Steffens conjectures that the stinging quality may be owing to the presence of an uncombined alkali.

Asterias.

A. aculeata.	Shetland and Orkney islands.
caput Medusæ.	In the sea off the main land. The Argus of the Shetland islands.

Echinus.

E. cidaris.	Island of Fula, the most western and recluse of the Shetlands.
placenta.	Orkney and Shetland islands.
spatagus.	Leith shore.

TESTACEA.

Chiton.

C. fascicularis.	Rocks on Leith shore.
ruba. *Fabricius.*	Rocks in the island of Unst, one of the Shetlands.
lævis.	Leith shore.
margenitus.	Ibid.

Lepas.

L. striata. *Pennant.*	Adhering to roots of fuci, Leith shore.
anatifera.	Orkney islands.

Pinna.

P. muricata.	Fished up in the sea off the island of Unst.

Dentalium.

D. entalis.	Orkney and Shetland islands.

ZOOPHYTA*.

Tubipora.

T. catenularia.	Orkney islands.
serpens.	Orkney and Shetland islands.
fascicularis.	Ibid.

Millepora.

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* The animals of this order appear to have been the first that were called into existence. This assertion does not rest on the metaphysical consi-

Millepora.

M. truncata.	Islands of Unst and Fula.
cellulosa.	Orkney and Shetland islands.
pumicosa.	Leith shore, investing sertularia.

Cellepora.

C. pumicosa.	Leith shore, attached to fuci.

Isis.

I. hippuris.	East coast of Scotland, and Orkney islands, but is very rare.

Gorgonia.

G. lepadifera.	I have not found this species myself, but have examined a specimen in the museum of the university of this place, which is said to have been taken up on the coast of Aberdeenshire.

Alcyonium.

A. Schlosseri.	Leith shore.
Cydonium.	Island of Fula.
arenosum. *Shaw.*	Leith shore.
gelatinosum.	Ibid.
digitatum.	Ibid.

Spongia.

S. ventilabrum.	Fished up in the vicinity of the islands of Unst and Fula.
infundibuliformis.	Island of Unst. Some specimens nearly a foot in diameter.
compressa. *Fabricius.*	Brassay Sound, main land, Shetland.

Flustra.

F. carbasea.	Pretty frequent at times on Leith shore, but rare in other parts.
hispida. *Pallas.*	Leith shore, adhering to fuci.
lineata.	Orkney islands.
membranacea.	Adhering to fuci on Leith shore and the Orkney islands.

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consideration of the most simple animals being first created, it has been demonstrated by the observations of the illustrious Werner. This profound naturalist discovered them in the oldest transition rocks, viz. those that are immediately on the newest primitive formations. These, therefore, [illegible] are to be viewed as the remains of the oldest, and consequently first created beings.

Tubularia.

Tubularia.

T. indivisa.	Leith shore and Shetland islands.
ramosa.	Ibid.

Sertularia.

Of this extensive genus a great number of species occur on our coasts. As it may be interesting to some to know the species in this neighbourhood, I shall here enumerate those I have found on the shores of Leith:

S. rosacea.
pumila.
operculata.
tamarisca.
abietina.
cupressoides.
cupressina.
argentea.
rugosa.

S. halecina.
thuya.
falcata.
antennina.
verticillata.
cuscuta.
filicula.
uva.
lendigera.

S. geniculata.
dichotoma.
spinosa.
loriculata.
fastigiata?
avicularia.
scruposa.
ciliata.
eburnea.

volubilis.	Orkney and Shetland islands.
nigra.	My friend Mr. Brown, who is now employed in exploring the natural history of New Holland, found this rare species near Aberdeen.

Pennatula.

P. phosphorea.	Firth of Forth.
mirabilis. *Muller.*	Prestonpans bay, Firth of Forth.

Hydra.

H. squammata. *Muller.*	This beautiful species I found on the shore of the island of Burra, and on the Holm of Cruster, in Brassay Sound, in Shetland. It was adhering to the fucus digitatus.

Edinburgh,
July 19th, 1803.

XXVII. *Letter from M.* HUMBOLDT *to* C. DELAMBRES, *one of the perpetual Secretaries of the National Institute**.

MY RESPECTED FRIEND, Lima, Nov. 25, 1802.

I HAVE just arrived from the interior of a country where in a large plain I made experiments on the small horary variations of the magnetic needle; and I learn, with regret, that the frigate Astigarraga, which was not to set out before the end of a fortnight, has hastened her departure for Cadiz, and will sail this very night. This is the first opportunity we have had during five months of writing to Europe from the solitude of the South Seas; and the want of time renders it impossible for me to write as I ought to the National Institute, which has given me the most affecting marks of the interest and kindness with which it honours me. A few days before my departure from Quito for Jaën and the river Amazon, I received the letter dated Pluviose 9th, addressed to me, through you, by that illustrious body. This letter employed two years to reach me in the Cordillera of the Andes: I received it the day after a second excursion which I made to the crater of the volcano of Pinchincha to carry thither a Volta's electrometer, and to measure the diameter of it, which I found to be 752 toises, while that of Vesuvius is only 312. This reminded me that on the summit of Guaguapichincha, where I have often been, and which I love as classical ground, La Condamine and Bouguer received their first letter from the ci-devant academy; and I figure to myself that Pinchincha, *si magna licet componere parvis*, is fortunate to philosophers. It is impossible for me to express the joy with which I read that letter of the Institute, and the repeated assurances of your remembrance. How agreeable it is to know that one lives in the memory of those whose labours continually tend to favour the progress of the human mind! In the deserts of the plains of the Apure, in the thick forests of Casiguiare and of the Oronoko, every where, in short, your names have been present to me; and, while reviewing the different periods of my erratic life, I stopped with pleasure on that of the years 6 and 7, when I lived among you, and when Laplace, Fourcroy, Vauquelin, Guyton, Chaptal, Jussieu, Desfontaines, Hallé, Lalande, Prony, and you in particular, loaded me with kindness in the plains of Lieusaint. Receive together the homage of my tender attachment and of my constant gratitude.

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* From *Annales du Museum d'Histoire Naturelle*, No. 8.

Long before I received the letter which you wrote to me in your quality of secretary to the Institute, I successively addressed to the Class of the Physical and Mathematical Sciences three letters, two from Santa-Fé di Bogota, accompanied with a work on a genus of cinchona; that is to say, specimens of seven kinds of bark, with coloured drawings of these vegetables; the anatomy of the flower so different by the length of its stamina, and the skeletons carefully dried,

dried. Dr. Mutis, who showed me a thousand marks of kindness, and for whose sake I went up the river in forty days, gave me a manuscript of nearly a hundred magnificent drawings representing new genera, and new species of his Flora of Bogota. I have thought that this collection, as interesting to botany as remarkable for the beauty of the colouring, could not be in better hands than in those of Jussieu, Lamarck, and Desfontaines; and I have presented it to the National Institute as a small mark of my attachment. This collection and cinchona were sent off for Carthagena of the Indies about the month of June this year; and Dr. Mutis took upon him to transmit them to Paris. A third letter for the Institute was dispatched from Quito with a geological collection of the productions of Pinchincha, Catopaxi, and Chimborazo. It is distressing to remain under a melancholy uncertainty in regard to the arrival of those objects, as well as to that of the collection of rare seeds, which three years ago I addressed to the *Jardin des Plantes* at Paris.

Want of time at present will not allow me to give you an account of my travels and occupations since our return from Rio Negro. You know that at the Havannah we received the false intelligence of the departure of captain Baudin for Buenos Ayres. Faithful to the promise which I made of joining him wherever I could, and persuaded that I should be more useful to the sciences by uniting my labours to those of the naturalists who accompanied captain Baudin, I did not hesitate a moment to sacrifice the little glory of finishing my own expedition; and I immediately freighted a small vessel to Bataban, that I might proceed to Carthagena of the Indies. This short passage was lengthened more than a month by stormy weather; the winds had ceased in the South Seas, where I expected to find captain Baudin; and I entered on the difficult route to Quito by Honda, Ibagué, the passage of the mountain of Quindin, Popayan, and Pastos. My health continued in a wonderful manner to withstand the change of temperature to which one is exposed in this route, descending every day from snowy regions of 2460 toises in height to scorching valleys where the thermometer does not fall below 26° or 24°. My companion Bonpland, whose knowledge, courage, and immense activity were of great assistance to me in my botanical researches and comparative anatomy, was afflicted for two months with a tertian fever. The season of the great rains came upon us in the most critical passage, the high plain of Pastos, and after a journey of eight months we arrived

at

at Quito, where we learned that captain Baudin had pursued his voyage from west to east by the Cape of Good Hope. Accustomed to misfortunes, we consoled ourselves with the idea of having made so great sacrifices for an intention of doing good: casting our eyes on our herbals, our barometric and geodesic measurements, our drawings, and our experiments on the air of the Cordillera, we did not regret our having traversed that country, a great part of which has never been visited by any naturalists. We were sensible that man ought never to depend on any thing but what is produced by his own energy. The province of Quito, the highest land in the world, and torn by the grand catastrophe of the 4th of February 1797, furnished us with a vast field for physical observations. Volcanoes so enormous, the flames of which often rise to the height of 500 toises, have never been able to produce a drop of liquid lava; they vomit up fire, sulphurous hydrogen gas, mud, and carbonated argil. Since 1797 this whole part of the world has been in agitation; we every moment experience frightful shocks, and the subterranean noise in the plains of Rio Bamba resembles that of a mountain crumbling to pieces under our feet. The atmospheric air and moistened earth (all these volcanoes are in decomposed porphyry) appear to be the grand agents of these combustions and these subterranean fermentations.

It has hitherto been believed at Quito that 2470 toises is the greatest height at which men could resist the rarity of the air. In the month of March 1802 we spent some days in the large plains which surround the volcano of Antisana at 2107 fathoms, where the oxen, when hunted, often vomit up blood. On the 16th of March we found out a passage over the snow, a gentle acclivity, on which we ascended to the height of 2773 toises. The air there contained 0·008 of carbonic acid, 0·218 of oxygen, and 0·774 of azote. Reaumur's thermometer was only at 15°; it was not at all cold, but the blood issued from our lips and eyes. The situation did not permit me to make a trial of Borda's compass but in a grotto lower down at the height of 2167 toises; the intensity of the magnetic forces was greater at that height than at Quito in the ratio of 230 to 218: but it must not be forgotten that the number of oscillations often increases when the inclination decreases, and that this intensity is increased by the mass of the mountain, the porphyry of which affects the magnet. In the expedition I undertook on the 23d of June 1802 to Chimborazo, we proved that with patience it is possible to sustain a greater rarity of the air. We ascended 500 toises higher than Condamine (on Carazon), and on Chimborazo we carried our instruments to the height of 3031 toises, where we saw the barometer fall to 13 inches 11·2 lines: the thermometer was at 1·3° below zero. We still bled at the lips; our Indians deserted us as usual; C. Bonpland, and M. Montufar, son of the marquis de Salvalegre of Quito, were the only persons who remained. We all experienced an uneasiness, debility, and desire to vomit, which certainly arose as much from the want of oxygen in these regions as from the rarity of the air. At that immense height I found only 0·20 of oxygen. A frightful chasm prevented us from reaching the summit of Chimborazo, of which we were within 236 toises. You know that a great uncertainty still prevails in regard to the height of this colossus, which La Condamine measured only at a very great distance, assigning to it the height of nearly 3220 toises, whereas Don Juan makes it 3380 toises; nor does this difference arise from the different heights which these astronomers adopted for the signal of Carabura. I measured in the plain of Tapia a base of 1702 metres. Pardon me if I speak sometimes of toises and sometimes of metres, according to the nature of my instruments. You know that in publication every thing may be reduced to the metre and centigrade thermometer. Two geodosic operations gave me for Chimborazo 3267 toises above the level of the sea; but the calculations must be rectified by the distances of the sextant from the artificial horizon and by other circumstances. The volcano of Tunguragua has decreased a great deal since the time of La Condamine: instead of 2620 toises I found no more than 2531; and, in my opinion, this does not arise from an error in the operations, because in my measures of Cayambe, Antisana, Cotopaxi, and Iliniza, I seldom differ ten or fifteen toises from the results of La Condamine and Bouguer. The inhabitants of these unfortunate countries all say that Tunguragua has visibly decreased in height: on the other hand, I find that Cotopaxi, which has been subject to such immense explosions, is of the same height as in 1744, or rather somewhat higher. But the stony summit of Cotopaxi indicates that it is a chimney, which resists and retains its figure. The operations we made from January to July in the Andes of Quito gave to their inhabitants the dismal intelligence that the crater of Pinchincha, which La Condamine saw full of snow, burns again; and that Chimborazo, which was thought to be so peaceable and innocent, has been a volcano, and perhaps will one day be so again. We found

[illegible] which [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] the [illegible] [illegible] the [illegible] [illegible] [illegible] we have [illegible] [illegible] others [illegible] and [illegible] [illegible] from [illegible] [illegible] Timana, the town of Ibarra, [illegible] we have confirmed the existence of [illegible] from the river Ohio [illegible] latitude. I spent a very [illegible] at Quito. The president of audience baron [illegible] and for three years [illegible] agents of the [illegible] have every where treated me [illegible] I must ever retain a [illegible] How much the times and manners have changed! I have paid particular attention to the pyramids and their foundation, which I do not think at all deranged in regard to the mill-stones (*pierres molaires*). A generous individual, a friend to the sciences, and to those men who have done honour to them, such as La Condamine, Godin, and Bouguer, the marquis de Salvalegre, at Quito, thinks of reconstructing them; but this leads me too far.

After passing Assuay and Cuença, where they gave us bull fights, we pursued our way by the Oxa to complete our labours on cinchona. We then spent a month in the province of Jaen de Bracamorros and among the Pongos of the river Amazon, the banks of which are ornamented with the *andira* and *buganvillæa* of Jussieu. It appeared to me of importance to fix the longitude of Tomependa and Chucungat,

cungat, where Condamine's chart begins, and to connect these points with the coast. La Condamine was able to determine the longitude only of the mouth of the Napa: time-keepers were not then in existence, so that the longitude of these countries requires a great many changes. My chronometer by Louis Berthoud does wonders; as I see by making observations from time to time on the first satellite, and comparing point for point my differences of meridian with those found during the expedition of M. Fidalgo, who by order of the king performed trigonometrical operations from Cumana to Carthagena.

From the river Amazon we crossed the Andes at the mines of Hualgayoc, which produce a million of piastres per annum, and where the mine of gray argentiferous copper is found at the height of 2065 toises. We descended by Casamasca (where in the palace of Atahualpa I delineated the arches of the Peruvian vaults) to Truxilla, proceeding thence by the deserts of the coast of the South Sea to Lima, where for one-half of the year the heavens are obscured by thick vapours. I hastened to Lima, that I might observe there the transit of Mercury on the 9th of November 1802.

Our collections of plants, and the drawings which I made in regard to the anatomy of genera, agreeably to the ideas communicated to me by Jussieu in conversations in the Society of Natural History, have greatly increased by the riches which we found in the province of Quito, at Loxa, at the river Amazon, and in the Cordilleras of Peru. We found a great many of the plants seen by Joseph de Jussieu; such as the *lloqua affinis*, the *quillajae*, and others. We have a new species of *jussiæa* which is charming, *colletia*, several *passifiores*, and the *loranthus* in a tree of sixty feet of height. We are particularly rich in palms and gramineous plants, on which C. Bonpland has made a very extensive work. We have at present 3784 very complete descriptions in Latin, and nearly a third more of plants in herbals which for want of time we have not been able to describe. Of every vegetable we can indicate the rock where it resides, and the height in toises at which it grows; so that in our manuscripts will be found very correct materials for the geography of plants. To do still better, I and Bonpland have often described the same plant separately. But two-thirds and more of the descriptions belong to the assiduity of Bonpland alone, whose zeal and devotion for the sciences cannot be too much admired. Jussieu, Desfontaines, and Lamarck, have in him formed a pupil who [illegible] with [illegible] books [illegible] con- [illegible] species. [illegible] determine [illegible] ceous [illegible] East Indies. [illegible] knots of a [illegible] with the [illegible] musa [illegible] has re- [illegible] by the [illegible] ric acid [illegible] men. In- [illegible] matic milks. [illegible] discovered [illegible] re the negroes [illegible] by [illegible] banks by [illegible] hour- [illegible] tests [illegible] towards the [illegible] proceed to [illegible] mnan: and there [illegible] rains in September [illegible]

Humboldt.

[illegible] February, and at the Ha- [illegible]

[illegible] Spencer for the Preservation [illegible] Shipwreck or other Accidents at Sea. [illegible] Bread-street, Cheapside.

[illegible] seems likely to be of great uti- [illegible] Society were pleased to award to [illegible] silver medallion on the 25th of [illegible]

Explanation.

[illegible] circle of a diameter to fit the body, six [illegible] composed of about 800 old tavern corks, [illegible] twine, well lashed together with lay-cord, covered with canvas, and painted in oil, so as to make it water-proof.

BB are tapes or cords about two feet long fastened to the back of the girdle with loops at the ends.

C is another tape or cord about three feet long, in the middle of which a few corks are strung, covered with canvas, and painted as above.

D is a pin of hard wood three inches long and half an inch diameter, fastened to the front of the girdle by a tape or cord about two inches long.

E the same.

When the marine spencer is to be used, slide it from the feet close up under the arms; bring the tapes or cords B B one over each shoulder, and fasten them by the loops to the pin D; bring the tape or cord C between the legs, and fasten it to the pin E.

A person thus equipped, though unacquainted with swimming, may safely trust himself to the waves; for he will float head and shoulders above water in any storm, and by paddling with his hands may easily gain the shore.

N.B. A marine spencer constructed as above, and covered with strong canvas unpainted, will have nearly the same buoyancy, though more liable to damage from the effects of sea water.

XXIX. *On the Electric Fluid**.

The hypothesis of the electric phænomena being caused by *two* fluids is now nearly given up for the more simple one of a *single* fluid. Cavallo in his Treatise on Electricity, 4th edit. vol. i. p. 106, 107, says: "When a body does not show any electrical appearances, it is then supposed to contain its natural quantity of electric fluid (but whether that quantity bears any proportion to the quantity of matter in general or not, is uncertain, and therefore that body is said to be in its *natural* or *non-electrified state*): but if a body shows any electrical appearances, it is then said to be electrified, and it is supposed that it has either acquired an additional quantity of electric fluid, or that it has lost some of its natural share. A body having received an additional quantity of electric fluid is said to be *overcharged*, or *positively electrified*; and a body that has lost part of its natural quantity of electric fluid is said to be *undercharged*, or *nega-*

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* Communicated by a Friend to Physical Inquiries.

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cellulosa.	Orkney and Shetland islands.
pumicosa.	Leith shore, investing sertularia.

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abietina.
cupressoides.
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argentea.
rugosa.
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thuya.
falcata.
antennina.
verticillata.
cuscuta.
filicula.
uva.
lendigera.
S. geniculata.
dichotoma.
spinosa.
loriculata.
fastigiata?
avicularia.
scruposa.
ciliata.
eburnea.

volubilis.	Orkney and Shetland islands.
nigra.	My friend Mr. Brown, who is now employed in exploring the natural history of New Holland, found this rare species near Aberdeen.

Pennatula.

P. phosphorea.	Firth of Forth.
mirabilis. *Muller.*	Prestonpans bay, Firth of Forth.

Hydra.

H. squammata. *Muller.*	This beautiful species I found on the shore of the island of Burra, and on the Holm of Cruster, in Brassay Sound, in Shetland. It was adhering to the fucus digitatus.

Edinburgh,
July 19th, 1803.

XXVII. *Letter from M.* HUMBOLDT *to* C. DELAMBRES, *one of the perpetual Secretaries of the National Institute*[*].

MY RESPECTED FRIEND, Lima, Nov. 25, 1802.

I HAVE just arrived from the interior of a country where in a large plain I made experiments on the small horary variations of the magnetic needle; and I learn, with regret, that the frigate Astigarraga, which was not to set out before the end of a fortnight, has hastened her departure for Cadiz, and will sail this very night. This is the first opportunity we have had during five months of writing to Europe from the solitude of the South Seas; and the want of time renders it impossible for me to write as I ought to the National Institute, which has given me the most affecting marks of the interest and kindness with which it honours me. A few days before my departure from Quito for Jaën and the river Amazon, I received the letter dated Pluviose 9th, addressed to me, through you, by that illustrious body. This letter employed two years to reach me in the Cordillera of the Andes: I received it the day after a second excursion which I made to the crater of the volcano of Pinchincha to carry thither a Volta's electrometer, and to measure the diameter of it, which I found to be 752 toises, while that of Vesuvius is only 312. This reminded me that on the summit of Guaguapichincha, where I have often been, and which I love as classical ground, La Condamine and Bouguer received their first letter from the ci-devant academy; and I figure to myself that Pinchincha, *si magna licet componere parvis*, is fortunate to philosophers. It is impossible for me to express the joy with which I read that letter of the Institute, and the repeated assurances of your remembrance. How agreeable it is to know that one lives in the memory of those whose labours continually tend to favour the progress of the human mind! In the deserts of the plains of the Apure, in the thick forests of Casiguiare and of the Oronoko, every where, in short, your names have been present to me; and, while reviewing the different periods of my erratic life, I stopped with pleasure on that of the years 6 and 7, when I lived among you, and when Laplace, Fourcroy, Vauquelin, Guyton, Chaptal, Jussieu, Desfontaines, Hallé, Lalande, Prony, and you in particular, loaded me with kindness in the plains of Lieusaint. Receive together the homage of my tender attachment and of my constant gratitude.

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[*] From *Annales du Museum d'Histoire Naturelle*, No. 8.

Long before I received the letter which you wrote to me in your quality of secretary to the Institute, I successively addressed to the Class of the Physical and Mathematical Sciences three letters, two from Santa-Fé di Bogota, accompanied with a work on a genus of cinchona; that is to say, specimens of seven kinds of bark, with coloured drawings of these vegetables; the anatomy of the flower so different by the length of its stamina, and the skeletons carefully dried,

dried. Dr. Mutis, who showed me a thousand marks of kindness, and for whose sake I went up the river in forty days, gave me a manuscript of nearly a hundred magnificent drawings representing new genera, and new species of his Flora of Bogota. I have thought that this collection, as interesting to botany as remarkable for the beauty of the colouring, could not be in better hands than in those of Jussieu, Lamarck, and Desfontaines; and I have presented it to the National Institute as a small mark of my attachment. This collection and cinchona were sent off for Carthagena of the Indies about the month of June this year; and Dr. Mutis took upon him to transmit them to Paris. A third letter for the Institute was dispatched from Quito with a geological collection of the productions of Pinchincha, Catopaxi, and Chimborazo. It is distressing to remain under a melancholy uncertainty in regard to the arrival of those objects, as well as to that of the collection of rare seeds, which three years ago I addressed to the *Jardin des Plantes* at Paris.

Want of time at present will not allow me to give you an account of my travels and occupations since our return from Rio Negro. You know that at the Havannah we received the false intelligence of the departure of captain Baudin for Buenos Ayres. Faithful to the promise which I made of joining him wherever I could, and persuaded that I should be more useful to the sciences by uniting my labours to those of the naturalists who accompanied captain Baudin, I did not hesitate a moment to sacrifice the little glory of finishing my own expedition; and I immediately freighted a small vessel to Bataban, that I might proceed to Carthagena of the Indies. This short passage was lengthened more than a month by stormy weather; the winds had ceased in the South Seas, where I expected to find captain Baudin; and I entered on the difficult route to Quito by Honda, Ibagué, the passage of the mountain of Quindin, Popayan, and Pastos. My health continued in a wonderful manner to withstand the change of temperature to which one is exposed in this route, descending every day from snowy regions of 2460 toises in height to scorching valleys where the thermometer does not fall below 26° or 24°. My companion Bonpland, whose knowledge, courage, and immense activity were of great assistance to me in my botanical researches and comparative anatomy, was afflicted for two months with a tertian fever. The season of the great rains came upon us in the most critical passage, the high plain of Pastos, and after a journey of eight months we arrived

at

at Quito, where we learned that captain Baudin had pursued his voyage from west to east by the Cape of Good Hope. Accustomed to misfortunes, we consoled ourselves with the idea of having made so great sacrifices for an intention of doing good: casting our eyes on our herbals, our barometric and geodesic measurements, our drawings, and our experiments on the air of the Cordillera, we did not regret our having traversed that country, a great part of which has never been visited by any naturalists. We were sensible that man ought never to depend on any thing but what is produced by his own energy. The province of Quito, the highest land in the world, and torn by the grand catastrophe of the 4th of February 1797, furnished us with a vast field for physical observations. Volcanoes so enormous, the flames of which often rise to the height of 500 toises, have never been able to produce a drop of liquid lava; they vomit up fire, sulphurous hydrogen gas, mud, and carbonated argil. Since 1797 this whole part of the world has been in agitation; we every moment experience frightful shocks, and the subterranean noise in the plains of Rio Bamba resembles that of a mountain crumbling to pieces under our feet. The atmospheric air and moistened earth (all these volcanoes are in decomposed porphyry) appear to be the grand agents of these combustions and these subterranean fermentations.

It has hitherto been believed at Quito that 2470 toises is the greatest height at which men could resist the rarity of the air. In the month of March 1802 we spent some days in the large plains which surround the volcano of Antisana at 2107 fathoms, where the oxen, when hunted, often vomit up blood. On the 16th of March we found out a passage over the snow, a gentle acclivity, on which we ascended to the height of 2773 toises. The air there contained 0·008 of carbonic acid, 0·218 of oxygen, and 0·774 of azote. Reaumur's thermometer was only at 15°; it was not at all cold, but the blood issued from our lips and eyes. The situation did not permit me to make a trial of Borda's compass but in a grotto lower down at the height of 2467 toises: the intensity of the magnetic forces was greater at that height than at Quito in the ratio of 230 to 218: but it must not be forgotten that the number of oscillations often increases when the inclination decreases, and that this intensity is increased by the mass of the mountain, the porphyry of which affects the magnet. In the expedition I undertook on the 23d of June 1802 to Chimborazo, we proved that with patience it is possible to sustain a greater rarity of the air. We ascended 500 toises higher than Condamine (on Carazon), and on Chimborazo we carried our instruments to the height of 3031 toises, where we saw the barometer fall to 13 inches 11·2 lines: the thermometer was at 1·3° below zero. We still bled at the lips; our Indians deserted us as usual; C. Bonpland, and M. Montufar, son of the marquis de Salvalegre of Quito, were the only persons who remained. We all experienced an uneasiness, debility, and desire to vomit, which certainly arose as much from the want of oxygen in these regions as from the rarity of the air. At that immense height I found only 0·20 of oxygen. A frightful chasm prevented us from reaching the summit of Chimborazo, of which we were within 236 toises. You know that a great uncertainty still prevails in regard to the height of this colossus, which La Condamine measured only at a very great distance, assigning to it the height of nearly 3220 toises, whereas Don Juan makes it 3380 toises; nor does this difference arise from the different heights which these astronomers adopted for the signal of Carabura. I measured in the plain of Tapia a base of 1702 metres. Pardon me if I speak sometimes of toises and sometimes of metres, according to the nature of my instruments. You know that in publication every thing may be reduced to the metre and centigrade thermometer. Two geodosic operations gave me for Chimborazo 3267 toises above the level of the sea; but the calculations must be rectified by the distances of the sextant from the artificial horizon and by other circumstances. The volcano of Tunguragua has decreased a great deal since the time of La Condamine: instead of 2620 toises I found no more than 2531; and, in my opinion, this does not arise from an error in the operations, because in my measures of Cayambe, Antisana, Cotopaxi, and Iliniza, I seldom differ ten or fifteen toises from the results of La Condamine and Bouguer. The inhabitants of these unfortunate countries all say that Tunguragua has visibly decreased in height: on the other hand, I find that Cotopaxi, which has been subject to such immense explosions, is of the same height as in 1744, or rather somewhat higher. But the stony summit of Cotopaxi indicates that it is a chimney, which resists and retains its figure. The operations we made from January to July in the Andes of Quito gave to their inhabitants the dismal intelligence that the crater of Pinchincha, which La Condamine saw full of snow, burns again; and that Chimborazo, which was thought to be so peaceable and innocent, has been a volcano, and perhaps will one day be so again. We found burnt rocks and pumice-stone at the height of 3031 toises. It will be unfortunate for the human race if the volcanic fire, for it may be said that the whole high land of Quito is one volcano with several summits, should force a passage through Chimborazo. It has often been said that this mountain is granite, but a single atom of it is not to be found: it is porphyry, here and there disposed in columns inclosing vitreous feld-spar, corncerre and olivin. This stratum of porphyry is 1900 toises in thickness. On this subject I could mention a *polarizing porphyry* which we discovered at Voisaco near Pasto; a porphyry which, analogous to the serpentine I described in the *Journal de Physique*, has poles without attraction. I might mention other facts relative to the grand law of the parallelism of the strata, and of their enormous thickness near the equator: but this is too much for a letter, which perhaps will be lost; and besides, I shall recur to this subject another time: I shall only add, that besides the elephants teeth which we sent to C. Cuvier from the land of Santa-Fé, 1350 toises in height, we have preserved for him others more beautiful; some of the carnivorous elephant, and others of a species a little different from those of Africa, brought from the valley of Timana, the town of Ibarra, and from Chili. Here then we have confirmed the existence of that carnivorous monster from the river Ohio from 50° northern latitude to 35° south latitude. I spent a very agreeable time at Quito. The president of audience baron de Carondelet loaded us with kindness, and for three years I have not once had reason to complain of the agents of the Spanish government, which have every where treated me with a delicacy and distinction of which I must ever retain a grateful remembrance. How much the times and manners have changed! I have paid particular attention to the pyramids and their foundation, which I do not think at all deranged in regard to the mill-stones (*pierres molaires*). A generous individual, a friend to the sciences, and to those men who have done honour to them, such as La Condamine, Godin, and Bouguer, the marquis de Salvalegre, at Quito, thinks of reconstructing them; but this leads me too far.

After passing Assonay and Cuença, where they gave us bull-fights, we pursued our way by the Oxa to complete our labours on cinchona. We then spent a month in the province of Jaën de Bracamorros and among the Pongos of the river Amazon, the banks of which are ornamented with the *andiva* and *buganvillæa* of Jussieu. It appeared to me of importance to fix the longitude of Tomependa and Chucungat,

cungat, where Condamine's chart begins, and to connect these points with the coast. La Condamine was able to determine the longitude only of the mouth of the Napa: time-keepers were not then in existence, so that the longitude of these countries requires a great many changes. My chronometer by Louis Berthoud does wonders; as I see by making observations from time to time on the first satellite, and comparing point for point my differences of meridian with those found during the expedition of M. Fidalgo, who by order of the king performed trigonometrical operations from Cumana to Carthagena.

From the river Amazon we crossed the Andes at the mines of Hualgayoc, which produce a million of piastres per annum, and where the mine of gray argentiferous copper is found at the height of 2065 toises. We descended by Casamasca (where in the palace of Atahualpa I delineated the arches of the Peruvian vaults) to Truxilla, proceeding thence by the deserts of the coast of the South Sea to Lima, where for one-half of the year the heavens are obscured by thick vapours. I hastened to Lima, that I might observe there the transit of Mercury on the 9th of November 1802.

Our collections of plants, and the drawings which I made in regard to the anatomy of genera, agreeably to the ideas communicated to me by Jussieu in conversations in the Society of Natural History, have greatly increased by the riches which we found in the province of Quito, at Loxa, at the river Amazon, and in the Cordilleras of Peru. We found a great many of the plants seen by Joseph de Jussieu; such as the *lloqua affinis*, the *quillajue*, and others. We have a new species of *jussiæa* which is charming, *colletia*, several *passifiores*, and the *loranthus* in a tree of sixty feet of height. We are particularly rich in palms and gramineous plants, on which C. Bonpland has made a very extensive work. We have at present 3784 very complete descriptions in Latin, and nearly a third more of plants in herbals which for want of time we have not been able to describe. Of every vegetable we can indicate the rock where it resides, and the height in toises at which it grows; so that in our manuscripts will be found very correct materials for the geography of plants. To do still better, I and Bonpland have often described the same plant separately. But two-thirds and more of the descriptions belong to the assiduity of Bonpland alone, whose zeal and devotion for the sciences cannot be too much admired. Jussieu, Desfontaines, and Lamarck, have in him formed a pupil who will do great things. We compared our herbals with those of Dr. Mutis, and we consulted a great many books in the immense library of that great man. We are convinced that we have a great many new genera and species, but much time and labour will be required to determine what is really new. We shall bring with us also a siliceous substance analogous to the *tabascher* of the East Indies, which M. Macé has analysed. It exists in the knots of a gigantic gramineous plant which is confounded with the bambou, but which in its flower differs from the *bambusa* of Schreiber. I do not know whether Fourcroy has received the milk of the *vegetable cow*, a tree so called by the Indians. It is a milk which when treated with nitric acid gave me a caout-chouc of a balsamic odour, but which, instead of being caustic and hurtful like all vegetable milks, is nourishing, and agreeable to drink. We discovered it in the road to Oronoko, in a plantation, where the negroes drink a great deal of it. I have sent also to Fourcroy by the way of Guadaloupe, as well as to sir Joseph Banks by Trinidad, our *dapiché*, or white oxygenated caout-chouc, which exudes from the roots of a tree in the forests of Pimichin in the most remote corner of the world towards the sources of the Rio Negro.

I shall not go to the Philippines. I shall proceed to Europe by Acapulco, Mexico, and the Havannah; and I hope to have the pleasure of seeing you at Paris in September or October 1803.

Health and respect,

HUMBOLDT.

P. S. I shall be at Mexico in February, and at the Havannah in June.

XXVIII. *Account of the Marine Spencer for the Preservation of Lives in Cases of Shipwreck or other Accidents at Sea.* *By* KNIGHT SPENCER, *Esq. of Bread-street, Cheapside.*

FOR this invention, which seems likely to be of great utility, the Royal Humane Society were pleased to award to Mr. Knight their honorary silver medallion on the 25th of March 1803.

Explanation.

A (Plate IV.) is a girdle of a diameter to fit the body, six inches broad, composed of about 800 old tavern corks, strung upon a strong twine, well lashed together with lay-cord, covered with canvas, and painted in oil, so as to make it water-proof.

BB are tapes or cords about two feet long fastened to the back of the girdle with loops at the ends.

C is another tape or cord about three feet long, in the middle of which a few corks are strung, covered with canvas, and painted as above.

D is a pin of hard wood three inches long and half an inch diameter, fastened to the front of the girdle by a tape or cord about two inches long.

E the same.

When the marine spencer is to be used, slide it from the feet close up under the arms; bring the tapes or cords B B one over each shoulder, and fasten them by the loops to the pin D; bring the tape or cord C between the legs, and fasten it to the pin E.

A person thus equipped, though unacquainted with swimming, may safely trust himself to the waves; for he will float head and shoulders above water in any storm, and by paddling with his hands may easily gain the shore.

N.B. A marine spencer constructed as above, and covered with strong canvas unpainted, will have nearly the same buoyancy, though more liable to damage from the effects of sea water.

XXIX. *On the Electric Fluid**.

The hypothesis of the electric phænomena being caused by *two* fluids is now nearly given up for the more simple one of a *single* fluid. Cavallo in his Treatise on Electricity, 4th edit. vol. i. p. 106, 107, says: "When a body does not show any electrical appearances, it is then supposed to contain its natural quantity of electric fluid (but whether that quantity bears any proportion to the quantity of matter in general or not, is uncertain, and therefore that body is said to be in its *natural* or *non-electrified state*): but if a body shows any electrical appearances, it is then said to be electrified, and it is supposed that it has either acquired an additional quantity of electric fluid, or that it has lost some of its natural share. A body having received an additional quantity of electric fluid is said to be *overcharged*, or *positively electrified*; and a body that has lost part of its natural quantity of electric fluid is said to be *undercharged*, or *negatively*

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* Communicated by a Friend to Physical Inquiries.

tively electrified." This is, it is believed, the theory now generally adopted by electricians, and is probably true as far as it goes: but the writer of this paper is of opinion that the electrical effects do not only depend on an *additional* quantity in one case, and a *loss* of the fluid in the other, but on the state of *condensation* (including in this term *compression*) and *rarefaction* (including *dilatation*) of the fluid, the *condensed* state being what is usually called the *positive* or *plus*; and the *rarefied* the *negative* or *minus* state. If the electric fluid be, as is commonly thought, elastic, there is little doubt but that, when an additional quantity is thrown on to a body, it suffers *compression*; and, on the contrary, when a portion is taken away, that the remaining fluid is *dilated*. With non-elastic substances a state of *condensation*, or rather *density*, may exist without *compression*; and, on the contrary, *rarefaction* without *dilatation*, if the particles of such matter are of different sizes; for suppose a box were filled with wooden balls of an inch diameter, there would be many vacancies between, which might be filled up with smaller balls, and the bulk not increased: here would be *density* without *compression*: take the small balls away, and there will be *rarefaction* without *dilatation*. This case we can hardly suppose to exist amongst elastic substances, though certainly possible, if each of the particles were at their greatest state of *compression* or *dilatation*, and were of different magnitudes.

In the Philosophical Magazine, vol. xii. p. 186, an optical experiment was suggested which might perhaps give some insight into the two electric states: hitherto there do not seem any *decisive* experiments which determine the *state* or the *course* of the electric fluid. In order to illustrate the hypothesis here advanced, let the conductor which is connected to the cushion, commonly called the negative conductor, and the other, or positive conductor, be compared to the receiver of a common (rarefying) air pump and the receiver of a condensing machine (or condensing air pump, as it might be called), both united, as they sometimes are, on one frame: here, whilst the air in one receiver is rarefied, in the other it becomes condensed. It is, according to the opinion now brought forward, the *equilibrium* of *condensation* and *compression*, not simply of *quantity*, which produces the shock, the spark, and other electrical effects.

Would not the terms *supplying conductor* and *receiving conductor* be preferable to *positive* and *negative* conductor? Perhaps electrical *supplyer* and *receiver* alone would be sufficient when speaking of the machine.

XXX. *Proceedings of Learned Societies, Societies of the Arts, &c.*

BATAVIAN SOCIETY OF THE SCIENCES AT HAARLEM.

This society held its fifty-first yearly general meeting on the 21st of May, and, after examining the answers sent in to prize questions since the last meeting, resolved to continue the following to which no answers had been received:

1st, What light has the new chemistry thrown on our knowledge of the nature of the human body?

2d, How far have physicians, in consequence of the light thrown on the nature of the human body by the new chemistry, become better acquainted than before with the nature and causes of certain diseases? and what consequences, more or less confirmed by experience and useful to the practice of medicine, can be thence deduced?

3d, How far has the new chemistry made physicians fully acquainted with the action of certain medicines which have either been long used or lately recommended? and what advantages arise from this knowledge in the treatment of certain diseases?

As some celebrated philosophers, in their application of the fundamental principles of the new chemistry to the knowledge of the human body, to diseases and to the cure of them, have indulged too much in hypothesis not founded on experience; and as great injury must thereby arise to the practice of medicine, to which the new chemistry seems to promise so much advantage, provided Lavoisier's rule of admitting nothing in it which is not founded on experience be observed; the society requires that, in answering these three questions, a distinction will be made between what is fully proved and what rests on weak grounds, and that the uncertainty of the latter only will be briefly pointed out.

4th, How far are the causes of the corruption of stagnant water known? And from what is already known and can be proved on the subject, what are the best and least hurtful means which can be employed to preserve stagnant water from corruption?

The answers to these questions are to be sent in before the 1st of November, 1804.

The following questions are again proposed to be answered before the 1st of January, 1804:

A natural history and description of whales, to serve as the means of tracing out the places where these animals are to be found; together with the safest and best methods either already known or practised, or which can be brought into practice, to kill them with the greatest speed and safety.

To be answered before the 1st of November, 1804:

I. How far can the meteorological observations made in the Netherlands serve for acquiring a knowledge of the nature of the winds of these countries? which are the most prevalent winds? what is their regular or general succession? from what previous circumstances can the inhabitants on some occasions, and with any degree of certainty, foretel changes of the winds? and what influence have they in general or sometimes on changes of the weather?

II. An accurate nomenclature of the mammalia, birds, and amphibia, not formerly introduced, which are found in the Netherlands, with their synonyms and the characteristic marks of the species and genera arranged according to the Linnæan system, with an indication of one or more of the best figures of each animal.

The following is proposed in consequence of the fund established by the late director, N. W. Kops:

III. As it is of great importance to the diffusion of each branch of natural knowledge to have the principal truths of it briefly and clearly exposed, the society requires that, from the great number of writings on the action of the Voltaic pile, whether given in journals or other periodical works, an essay may be composed containing an account of what has been learned in regard to the Voltaic pile and the experiments made with it.

The following questions are still proposed to be answered under the usual conditions:

For an unlimited period.

I. What have we been taught by experience in regard to the utility of some animals which appear to be noxious, and especially in the Netherlands? and what precautions are to be employed in order to extirpate them?

II. What indigenous plants, hitherto not tried, can be employed with advantage in the materia medica in the room of exotic plants?

III. What indigenous vegetables, never yet used, can be employed as wholesome and cheap food? and what foreign vegetables, not used in this country, can be introduced for the same purpose?

IV. What indigenous plants, not yet used, are capable, according to well established proofs, of furnishing good dye-stuffs, which can be prepared and employed with advantage? and what exotics, fit for the same purpose, can be introduced and cultivated on waste land with advantage?

The answers, written in the usual manner in Dutch, French, Latin, or German (but not in German characters), accompanied with a sealed billet containing the author's name, must be transmitted (post paid) to M. Van Marum, secretary to the society.

The prize for each of the above questions is a gold medal, with the usual impression of the society, and the name of the author and the date on the edge, or thirty ducats, at the option of those to whom the gold medal is adjudged.

CHALCOGRAPHIC SOCIETY, LONDON.

It must be highly gratifying to the lovers of the fine arts, to those who know their powerful influence on the public mind, to be informed that a society of engravers has been formed under the immediate patronage of his royal highness the prince of Wales. To alleviate the misfortunes and miseries resulting from sickness and the decays of nature, is the immediate object of the institution; and we have no doubt but the funds arising from the subscriptions of artists, aided by the support which it merits, and will no doubt obtain, not only from the lovers of the arts, but from those who entertain liberal and enlarged views of the true interests of their country, will prove equal to the support of those who, in spite of their exertions, may need such aid.

To the public this institution promises the most solid advantages; advantages impossible to be commanded by any other means. . Talent, energy, and power, with the peculiar acquirements and excellencies of each artist in each line, united and concentrated in single productions, must give them a degree of excellence which could never be obtained from insulated talents, however splendid, and must increase in proportion the commercial interests of the country.

The institution had its first anniversary dinner on Wednesday, the 6th of July, at the Crown and Anchor, in the Strand. It was numerously and respectably attended, and the afternoon was spent amidst that social enjoyment which arises from a mutual interchange of sentiments enlivened by genius, and polished and corrected by taste. Great harmony prevailed during the whole evening, and the company separated highly gratified with the result of their meeting, and animated by the hope of that success to which, by their laudable exertions, they are so well entitled.

The following is a list of the officers and committee for the present year:

Francis Bartolozzi, R. A. engraver to the king, president.

Peltro William Tomkins, esq. historical engraver to the queen, vice-president.
Anker Smith, esq. A. R. A. treasurer.
Abraham Raimbach, esq.
Charles Warren, esq.
James Parker, esq.
Cosmo Armstrong, esq.
Robert Mitchell Meadows, esq.
Thomas Medland, esq.
James Fitler, esq. A. R. A. naval engraver to the king.
Thomas Milton, esq. honorary secretary.
Wilson Lowry, esq. } auditors.
John Samuel Agar, esq. }
Philip Hammersley Leaths, of the Middle Temple, honorary consulting member.

FRENCH NATIONAL INSTITUTE.

Notice of the Labours of the Mathematical and Physical Class of the Sciences since the last Public Sitting.

The name of tautochrones is given to curves in which the oscillations of a heavy body are always of the same duration, whatever may be their extent. Tautochrones have been rendered celebrated by the labours of the greatest geometricians, who have successively endeavoured to overcome the difficulties exhibited by the different hypotheses which may be formed in regard to the laws of gravity and resistance. But though their formulæ had all the generality possible, they sought only for plane tautochrones, while for each hypothesis there exist an infinite number which are of a double curvature.

An examination of these new tautochrones, and their relation with plane tautochrones, form the object of a memoir by C. Biot. From a consideration of the formulæ the author has deduced some theorems remarkable for their simplicity. But whatever may be their elegance or novelty, we shall not enter into any detail respecting a matter so abstract, that Bossut in his Essay on the History of the Mathematics thought it his duty to give an exculpation of those geometricians who have employed their powers and their genius on these problems, which are merely theoretical. Nothing can be added to the solidity of the reasons which he adduces in favour of these speculations, which on the first view appear barren, but which in the end may be epplied to useful purposes. But this may be rendered more sansible by a striking example. When the antient geometricians were endeavouring with so much care to discover

ver all the properties of the conic sections, and when Apollonius made them the subject of a profound treatise, the books of which have been long regretted, and which have not been handed down to us; might not the same reproach of the loss of time in meditations which might have been better employed, have been addressed to them? Who could then foresee the numerous applications which have been made of these curves to several branches of the mathematics? and who could suspect that the ellipsis is the figure of all the planetary orbits?

Of the most useful elementary problems, none has received more solutions than that the object of which is to correct the apparent distances of the moon from the sun and stars, in order to deduce from it the longitude at sea. This problem is not indeed very difficult; but it is of daily use, and those who have occasion to employ it are not always well versed in calculation: on this account, the easiest approximations sufficiently exact for practice have been substituted for the rigorous methods. The subject was thought to be exhausted, and yet very simple considerations, which never before occurred to any one, have furnished C. Legendre with an entirely new solution. His formula possesses a remarkable symmetry, which serves to engrave it on the memory. Nothing was wanting but a little brevity in the calculation; and the author has found means to give it this merit by including in two tables several terms, the suppression of which shortens the operation a third. Other known formulæ possessed the latter advantage; but the new solution has, above all others, the merit of elegant symmetry; which ought to be considered of great importance, since it contributes to facilitate the operation.

If the phænomena of the tides were subjected only to the combined action of the sun and moon, they might be predicted with the same precision as the celestial phænomena. With a few data obtained by observation, one might announce before-hand, both the exact moment and the precise elevation of the waves. The action of the winds, which will doubtless always escape our calculation, may serve indeed to account for the principal and periodical causes of the tides: but it at least modifies their effects; it can increase or diminish, accelerate or retard them; and if the sun and moon shall happen to be so placed as to produce the strongest tide, and if the wind conspire also to raise the waters, they may then produce extraordinary inundations, of which it is of importance to be forewarned

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in order that the necessary precautions may be taken. It is on this account that for some time the *Connoissance des Tems* has announced for all the new and full moons the force of the tides, abstracting from every local and accidental circumstance. The tides of Ventose and Germinal last were announced as about to be the highest in the course of the year. They attracted the attention of observers and of the curious. If the expectation of the latter was not fully answered, the former had reason to be more satisfied. These tides, indeed, were the highest of any remembered: but the atmosphere was calm; and consequently none of those accidents, the possibility of which only was foreseen, could take place. C. Rochon communicated to the class what he observed at Brest, and C. Septfontaines has transmitted to us what he saw at Calais. Their notices induced C. Laplace to read a memoir; at the conclusion of which the class, sensible of the necessity of a series of observations made at different ports, and according to an uniform method, appointed a commission charged with drawing up instructions proper for serving as a guide to observers.

The report of the commission has been printed, to be distributed in the ports. The ministers have promised to give their orders; and the series of observations destined to make known what part of the phænomena of the tides depends on periodical and general causes, and what depends on local or accidental causes, will soon be begun.

The new planets discovered by Piazzi and Olbers continue to engage the attention of astronomers. Notwithstanding the smallness of the arc which they have passed through in our sight, and notwithstanding the considerable perturbations which they experience from Jupiter, we have already obtained the elements of their orbits with sufficient precision to find again these bodies in the place indicated by calculation, when they become visible, after having been several months lost in the rays of the sun. The greatest difficulty arises from their extreme smallness, which sometimes causes us to doubt whether we have them in the field of the telescope. This is true in regard to Pallas in particular, which appears sometimes like a star of the 10th and 11th, or even the 12th magnitude, while Ceres appears of the 7th or 8th. But as there is something too arbitrary in this distribution of the stars according to the order of their magnitudes, it will be better to say with Messier, that Pallas is the smallest object that can be distinguished with an excellent telescope.

An extraordinary circumstance has given to this imperceptible star for a moment a more sensible diameter and a stronger light. On the 28th of May, the weather being very fine, C. Messier was surprised to find in it a light double to that which it had before; and yet, according to calculation, the distances of the sun and moon being nearly the same, the brightness of the planet ought not to have changed. The cause of this appearance was soon discovered. The small planet in its course met with a star, to which it appeared to be so close that the least interval could not be observed between them. Forty-two minutes after, a separation took place, and, according to the known motion of the planet, the interval must have been 15″. The position of the small star may be determined at leisure; and from the repeated observations which may be made of it, there will result for the moment of the observation of Messier a determination of the place of the planet more exact than any of those which could have been procured in a direct manner. Those observations known under the name of *appulses* are exceedingly rare. However numerous the small stars may appear, the intervals which they leave between them are sufficiently large for the planets to make the tour of the heavens without concealing one of them, or at least any of those which can be observed. The moon, however, ought to eclipse some of them every day: but their faint light becomes extinct on the approach of a stronger light; and the observation of these eclipses is too difficult and too uncertain to induce astronomers to attend to them: they pay no attention but to stars of the 4th or 5th magnitude, and below.

The arc of the meridian employed by the French astronomers for determining the fundamental unity of the metric system was the greatest ever measured. C. Mechain, during his residence in Spain, remarked that it could still be extended two degrees by forming two triangles, which touching the coast of Spain between Barcelona and Tortosa should terminate at the islands of Majorca and Ivica. The difficulty was to measure the angles, and perceive in a telescope not half a metre in length signals at the distance of two hundred miles. These observations could succeed only under the most favourable and consequently the rarest circumstances; they could not be attempted but in the middle of winter, and then they could have been attended only with half success. C. Mechain found himself obliged to abandon a project highly interesting, the plan of which was already drawn up. The reciprocal dispositions of France and Spain were not then sufficiently amicable to allow us to flatter ourselves with the hope of obtaining that aid and concert indispensably necessary for operations so difficult; but these dispositions having happily changed, the French government gave orders for the continuation of our meridian to the Balearic islands. C. Mechain is already at Barcelona with instruments the best suited to the difficulty of the observations, which will be begun as soon as he has concerted the proper measures with the Spanish commissioners. This new enterprise promises two advantages. The first is, that it will add two degrees to the arc already measured, which will be sufficient to indemnify us for the time and labour it may cost. Another advantage, still more important in the eyes of some persons, is, that we shall have a whole arc equally divided into two in the parallel of 45°, and from which, without any supposition on the figure of the earth, we may deduce the whole extent of the meridian.

The fame of these operations, of which France set the example, has more than once excited the emulation of neighbouring nations. Hence, after the measurements performed by the French in Peru, at the polar circle, in France itself, and at the Cape of Good Hope, we have seen degrees measured at Rome, Turin, Vienna, Hungary, Pennsylvania, and Milan. The Swedes have lately repeated and extended with instruments made in France, and with all the means furnished by the present state of the sciences and the arts, the operations performed in 1736 at the polar circle. The details of the new measurement have not yet been published: but we learn by letters from M. Melanderhielm, perpetual secretary of the Academy of Sciences at Stockholm, and the projector of the new operation, that the conclusions deduced from it do not accord with what resulted from the first. The latter gave a degree which was considerably different from all the rest, and supposed so great a flattening, that it excited some suspicions in regard to the exactness of the measures. The new one reconciles the whole. This degree, compared with that of France, gives for the flattening nearly the same quantity as the degree of France compared with that of Peru. This result would be so satisfactory that we scarcely dare to give credit to it. There were some doubts in regard to the correctness of the operations performed in 1736; but the error which it would be necessary to acknowledge in it far exceeds the limits in which it was supposed to be included.—Until the publication of the labour of the Swedes has produced complete conviction, we have reason to think that the irregularities of our globe are not so great as has hitherto been believed, and that the curve of the meridian, abstracting some local circumstances, deviates much less from the regular elliptical figure.

It is acknowledged by the ablest naval officers that the port of Brest in time of war cannot be supplied with provisions and stores by sea, and the burthensome method of employing carts can be recurred to only on the most urgent occasions. The counsellor of state Bruix has already proved in a printed memoir the indispensable necessity of an internal communication between Brest and the Loire, Boats of the burthen of ten tons at most, and a canal. would be sufficient for the continual wants of the navy. C. Rochon, who has been long occupied with projects of internal navigation proposed to the states of Brittany, has given more extent to his ideas in a memoir which he read to the class. He shows in what manner a highly useful communication might be established between Nantes, L'Orient, and Brest, by rendering navigable the rivers Erdre, Isac, Ourt, Blavet, and Châteaulin.

We shall take this opportunity to say a few words respecting some experiments lately made by C. Rochon with a telescope, of which he gave a description, with an account of its uses, in a memoir printed in the year 9.

It is well known that rock crystal has the property of double refraction, and of producing two images. This property C. Rochon has found the ingenious means of converting to advantage. A prism of this crystal placed in the interior part of a telescope causes two images of the observed object to appear, and these images approach or recede from each other according as the prism is brought nearer to, or removed from, the eye. When the images are brought into contact, a scale engraved on the outside of the telescope indicates to the observer how many times the diameter of the observed object is contained in the distance. Hence, when the distance is known the diameter can be determined, and the diameter when known will give a sufficiently correct idea of the distance. If you observe a ship at sea, which you wish to come up with or avoid, bring in contact the two images; if you are approaching the vessel the two images will encroach on each other; but, on the other hand, if the two vessels are receding from each other, the images will be soon separated. It is easy to distinguish the rate of the observed vessel, and by these means you may know very nearly the dimensions of her masts: bring

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into contact, and end to end, the two images of the main mast, and you will know how many lengths of that mast you are distant from the vessel. At land, you observe the images of a corps of the enemy, and place these images in such a manner that the feet of the one may stand on the heads of the other; and if you estimate the mean height of a soldier at five feet ten inches, the telescope will show how many times that quantity is contained in the distance you are from the enemy.—This short explanation sufficiently proves of what utility this instrument may be, and which would even be interesting were it only an object of mere curiosity. Experiments on this subject were repeated at Saint-Cloud on Tuesday, the 31st of May, before the First Consul, who ordered several telescopes of this kind to be constructed. This discovery may be of great use also to astronomy. C. Rochon has already employed it to measure the diameters of Mars, Jupiter, and Saturn. At first he was not able to apply it to the sun and moon, the diameter of which is about 30′, because the angle of refraction is only 20′; but Rochon and Torelli de Narcy, by cutting the crystal in an ingenious manner, were able to double and even to triple the angle of refraction. There is no planet, therefore, the diameter of which cannot be measured in this manner, provided it be sufficiently luminous; for it is evident that the two images are necessarily weaker and fainter than one image would be. There is no inconvenience of this kind in regard to the moon and sun, which have always too much light; and one of these prisms is going to be adapted to the best telescope in the national observatory.

XXXI. *Intelligence and Miscellaneous Articles.*

NEW FULMINATING POWDER.

Professor Proust, in a letter to A. I. C. Delametherie, says: "I have made in the course of my lectures an experiment which had almost been attended with serious consequences to my auditors. It is not exaggeration, perhaps, to say, that a mixture of oxygenated muriate[*] with arsenic takes fire with the rapidity of lightning. I am accustomed to compare the duration of different kinds of powder by burning them in cannons of equal diameter, which are cased in cork, so that they may be afterwards placed in water and immersed almost entirely lengthwise. I was desirous of burning in the same manner an arsenical mixture along with an equal quantity of the same powder, that I might compare their flames and duration : but scarcely had I time to retire a little, after having set fire to the two cannons, when the first made an explosion and burst, breaking the glass vessel, and throwing the water in all directions like radii from a common centre. Though the mouth of the tube was not confined, so that the escape of the flame was free, the cannon split from the top to the bottom, and unrolled itself like a card. This powder is so violent in its effects that in my opinion it would be dangerous to try to make any application of it. If two long trains, one of gunpowder and the other of the above mixture, be formed on a table, and if they be made to coincide at one extremity in order to set fire to them at the same time, you will be astonished to see the one disappear like lightning, while the other seems to burn exceedingly slow.

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[*] The Journal de Physique, from which we have taken this notice, does not say what oxygenated muriate was employed.

PREPARATION OF A FULMINATING OXALATE OF SILVER.

Extracted from a Letter of Brugnatelli.

Take 100 grains of lapis infernalis in powder, and having put them into a beer glass pour over them first an ounce of alcohol, and then as much concentrated nitrous acid. The mixture becomes heated, enters into ebullition, and there is visibly formed ether, which is changed into a gaseous fluid. The matter gradually becomes milky and opake, and is filled with small very white flakes : when the whole gray powder of the lapis infernalis has assumed this form, and when the liquor has acquired consistence, you must immediately add distilled water to suspend the ebullition, and to prevent the matter from being re-dissolved, so that nothing may be found but the solution of silver. Then collect the white precipitate on a filter, and suffer it to dry. This precipitate is fulminating silver: a little more than half the weight of the lapis infernalis employed is obtained. The detonating force of this preparation even in a much smaller quantity far surpasses that of fulminating mercury prepared according to the process of Mr. Howard. It detonates in a terrible manner when scarcely touched with a glass tube the extremity of which has been dipped in concentrated sulphuric acid, or even that of the shops. A grain of this fulminating silver put upon a burning coal made so loud a report that it stunned the by-standers. The same effect

always contains in greater or less quantity. This process, which was considered as the only one proper for obtaining pure molybdic acid, besides its not answering the proposed end, is too expensive to be followed, and therefore another more œconomical and more proper for obtaining an acid free from foreign mixture is desirable to be substituted in its stead.

The third process consists in distilling the sulphurated molybdena with arsenic acid, and in expelling by volatilization the oxide and sulphuret of arsenic which are formed. This method also is so expensive and so dangerous that we shall not enter into any further discussion of it.

The fourth process, which belongs also to Scheele, is that which furnishes the acid in the speediest manner; but it does not furnish it pure. It consists in causing the molybdena to deflagrate with nitrate of potash in proportions respecting which authors vary. Scheele * took four parts of nitrate to one of sulphuret of molybdena; Lampadius † recommends six parts of nitrate to one of molybdena, and Fourcroy ‡ three parts of nitrate to one of molybdena.

The sulphur and the metal are here converted into acids, and in that state combine with the potash. A portion of the nitrate remains undecomposed, and there is formed at the same time deoxygenated nitrate: the remaining mass is dissolved in warm water; and the molybdate of potash, still mixed with its other salts, is decomposed (or this may be done after it has been separated by crystallization) by means of the nitric, muriatic, or sulphuric acids. The molybdic acid, when the solution is not too dilute, separates under the concrete form: this precipitate is not pure molybdic acid, but, as Scheele before announced, molybdic acid holding potash, or, according to Fourcroy, acidulous molybdate of that alkali.

This acidulous molybdate is much more soluble in water than pure molybdic acid, which for its solution requires 500 parts of that liquid, whereas acidulous molybdate dissolves in three or four parts of water. The latter fuses in the fire sooner than the acid, and more readily corrodes the crucible. After cooling, it is of a beautiful yellow colour: the acid, on the contrary, exhibits a radiant mass of a whitish gray colour, having a slight metallic brilliancy. The acidulous molybdate does not suffer itself to be sublimated; whereas the pure acid is vaporized by heat, and deposits it-

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* Opuscula Physica et Chemica, tom. i. p. 202.

† Handbuch der Chemischen Analyse, p. 325.

‡ Systême des Connoissances Chimiques.

self, on cold bodies presented to it, under the form of needles and yellow scales. It appears that acidulous molybdate has often been employed for pure molybdic acid. Thus Lampadius gives reason to believe that the acid which he prepared by the above process was not pure acid but acidulous molybdate. He recommends decomposing the mass which remains after deflagration by muriatic acid in excess. It will be seen by the following experiments that the molybdic acid cannot be entirely freed from the potash. To remove this obstacle to the preparation of pure molybdic acid, Scheele recommends dissolving the precipitate obtained in the smallest quantity of water possible, and boiling it for some minutes with a new quantity of nitric acid. When the alkali has been thus separated, the molybdic acid is supposed to deposit itself in very small crystals. It appears to me confirmed that in this manner there is separated a little pure acid; but the very large part of the acid which remains dissolved with the acid added in excess is obtained separately with very great difficulty; and when enough of acid is not added, a mixture of pure acid and acid holding alkali is obtained. As this fourth process appeared to me far preferable to the rest, being much shorter and more œconomical, I wished to try whether it was not possible still to improve it, and to render it common in the preparation of the molybdic acid.

As the proportion of nitre, even in the process of Fourcroy, appeared to me still too great, I was desirous to examine whether something might not be saved in this point of view.

Experiment I.

I heated to redness in a Hessian crucible six gros of saltpetre, and projected on it, in parts, pure sulphurated molybdena pulverized. After I had added two gros and twenty grains of this mineral, the deflagration was still very brisk. I maintained the matter in fusion for some minutes longer, after which I dissolved the mass in water. The solution was complete, a few grains of oxide of iron and of silex excepted. On proceeding to the crystallization of the salts contained in this mass, I obtained some small crystals of undecomposed nitre.

Experiment II.

According to the indication furnished by the preceding experiment, I took two pounds three quarters of purified nitre, which I brought to red fusion, and gradually projected on it a pound of pulverized sulphuret of molybdena.

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[illegible] molybdate conjointly [illegible] nitrate of potash [illegible] be decomposed [illegible] therefore resolved to re- [illegible] acid.

Experiment IV.

[illegible] above solution of acidulous molybdate.

molybdate, adding an excess of nitric acid. A large portion of the precipitate was redissolved. After some hours digestion I tried the nature of the precipitate, after having collected and washed it on a filter. On putting a little of the precipitate, still moist, into a Hessian crucible, and exposing it to heat, it resolved itself into a transparent liquor. When the whole moisture was dissipated, and the crucible had begun to be red, the matter entered into fusion; but only a very weak volatilization was observed by an increase of heat. The fused mass was of a yellowish gray colour. A great part of it had passed through the crucible. It resulted from these phænomena, compared with those which the free acid exhibited with water and in the fire, that the precipitate obtained in this experiment was a mixture of free molybdic acid and acidulous molybdate of potash.

After this imperfect success I resolved to try the muriatic acid to decompose acidulous molybdate of potash.

Experiment V.

I decomposed the above-mentioned solution by means of muriatic acid. An abundant precipitate was formed. I added a strong excess of acid, which dissolved a remarkable quantity of the precipitate. I digested the mixture for some hours in a warm place, which completed the solution of almost the whole it. After cooling, there was formed in the liquor a considerable number of very small crystals. I tried these crystals, and found that they were almost entirely soluble in three or four parts of warm water; that they readily fused in an ignited crucible without emitting much vapour; that they penetrated the matter of the crucible; and that after cooling they had a grayish yellow colour. I thence concluded that the precipitate obtained by the muriatic acid contained scarcely any free acid, but was almost entirely composed of acidulous molybdate. I then evaporated to one-half, in a moderate heat, the solution which still contained molybdic acid. During the evaporation, but in particular after cooling, there was separated a quantity of small yellowish white crystals: there, however, remained in the solution a still greater quantity of molybdic acid, as was indicated by its acid taste and the trials to which it was subjected. The crystals obtained exhibited the same phænomena with water, and in the fire, as pure molybdic acid.

These experiments did not furnish a method of preparing, with safety and advantage, pure molybdic acid, and of obtaining the whole quantity contained in the sulphurated molybdena; or there was formed acidulous molybdate of potash which was not decomposed; or only a little molybdic acid was separated; or there remained in the liquid too large a quantity of acid, which could be separated only incompletely and with difficulty. In this unfavourable state of things I thought of several means for decomposing the molybdate by the help of double affinities, but I was not able by any of the known means to separate the acid completely from the alkali. I therefore resolved to recur to the old method, which consists in oxygenating the sulphuret by nitric acid, taking care to correct the process by separating the greater part of the sulphur by calcination. By this preliminary separation of the sulphur a great quantity of the acid is saved, and the labour is considerably shortened. I consequently proceeded to calcination in the following manner.

Experiment VI.

Five ounces and a half of sulphuret of molybdena in fine powder, and which were perfectly pure, a few small particles of quartz and oxide of iron excepted, were introduced into a large Hessian crucible, which, for the greater convenience, was placed obliquely in a furnace and surrounded by charcoal. When the matter was red it was stirred incessantly, and in turns, by me and my friend M. Haberle, to whom I was indebted for the molybdena employed in these different experiments. During the first hour, and before the greater part of the sulphur was dissipated, the matter retained its lightness: it then united into a mass, and its black colour passed successively to gray, reddish gray, and then to whitish gray.

When the whole sulphur was driven off, which required two hours, the mass by an increase of heat coagulated more and more, and even began to fuse at the bottom of the crucible, and it appeared by the vapour which rose that the molybdena was volatilizing. Having maintained a moderate fire for half an hour, I took the crucible from the fire and examined the matter. It had a whitish gray colour, exhibited here and there splendour and a crystalline form, and weighed a little more than four ounces; which approaches nearer to the 0·23 of sulphur which Lampadius said he found in molybdena than the proportion of Kirwan, which is 0·55. The sensible metallic taste of the calcined matter, its brilliant and crystalline aspect, and the experiments made by Ilsemann* and Heyer*, in which calcined molybdena exhibited the phænomena of an acid, made me suspect that this molybdena might be molybdic acid like that sublimated. In this case the nitric acid might have been spared; which would have rendered the process still shorter and more œconomical.

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* Crell's Chemische Annalen 1787, vol. i. p. 410.

To clear up my doubts in this respect I made the following preliminary experiments.

Experiment VII.

I reduced to fine powder some grains of calcined molybdena, poured over it a gros of water, and heated it above a lamp. I then instilled into it some drops of a solution of carbonate of soda: on each new instillation a strong effervescence took place, and a great part of the matter was dissolved. For the moment this result was sufficient to enable me to conclude from it the acid nature of the calcined molybdena. The solution was filtered, and decomposed by nitric acid. There was formed a precipitate of molybdic acid, which placed it beyond a doubt that there was formed a combination of this acid with soda.

Experiment VIII.

I pulverized some grains of calcined molybdena, and boiled them for some minutes with an ounce of water. The taste of the liquor, which was sensibly acid and metallic, as well as the red colour it communicated to turnsole paper, again proved the nature of the acid of calcined molybdena.

It gave me great satisfaction to have found so short and so œconomical a method of obtaining molybdic acid. I cannot help wondering, as the experiments of Scheele, Heyer, and Ilsemann, have been so long known, that chemists have not followed this method indicated to us by the nature of calcined molybdena, and to which they had so nearly approached. In order to ascertain more fully whether calcined molybdena is pure molybdic acid, and to determine the means of separating from it the heterogeneous substances with which it might be still combined or mixed, I made the following experiments.

Experiment IX.

As the molybdic acid has the property of combining easily with a portion of potash, and of retaining that base, if not entirely, at least in a great part, against the action of all acids, this salt could not be of any use for purifying that

acid. The alkaline earths, and earths which with the molybdic acid form salts difficult of solution, could not answer my purpose to separate the acid from the insoluble substances with which they are mixed. It remained to try soda and ammonia, which I found to be both equally proper under circumstances peculiar to each of them, for accomplishing the proposed end.

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* Crell's Chemische Annalen, vol. ii. p. 26 and 125.

Experiment X.

I boiled two gros of calcined molybdena with eight ounces of water, and instilled into the solution a ley of carbonate of soda until no more effervescence took place. An excess of alkali was added, and the matter was left to boil for half an hour: it was then filtered, and the concrete matter which remained on the filter was edulcorated. The different liquors were evaporated to two ounces, and nitric acid to excess was instilled into the warm liquid. During the instillation of the nitric acid a crystalline white powder was deposited, and after cooling there were formed in the liquor crystals of a certain size. A new quantity was obtained by evaporation. The different salts were washed and dried. The residuum on the filter, after being washed and dried, weighed thirty grains, and had the reddish colour of oxide of iron.

Experiment XI.

The preceding experiment was repeated, with this difference: the liquor, after the effervescence had ceased, was boiled for half an hour longer. The matter, after being reduced to two ounces, and filtered, was suffered to cool before the nitric acid was added. The addition of this acid caused to be precipitated a larger quantity of the white powder, which had a more crystalline aspect, and which after edulcoration and desiccation had the splendour of mother-of-pearl. As I did not add so much acid in this experiment as in the former, the liquor was less charged with molybdic acid. The residuum, when washed and dried, weighed also thirty grains.

As I suspected that this residuum might contain a portion of molybdic acid less soluble than the preceding, I tried to extract this acid by boiling the residuum with nitric acid. With this view I made the following experiment.

Experiment XII.

The thirty grains of the residuum of the two last experiments were boiled with an ounce of nitric acid of the specific gravity 1·250 and half an ounce of water, until the whole moisture was dissipated. During the evaporation I observed no disengagement of nitrous gas, from which I could conclude that there was in the residuum a little molybdena not yet acidified. I then boiled the residuum for half an hour with a ley of carbonate of soda, after which there was added to the solution nitric acid to decompose it; but I observed no precipitation of molybdic acid, though an excess of nitric acid had been added. The solution had only a metallic acid taste. On examining the residuum more closely, it was found to be composed of alumine, oxide of iron, and a few fragments of silex, which, if I except the oxide of iron contained in the sulphurated molybdena, arose in part from the molybdena and partly from the crucible.

Experiment XIII.

The white precipitates of experiments X. and XI. were put into an ignited crucible. By an increase of heat they first entered into fusion, and were then volatilized and condensed, partly under the pulverulent form and partly under the crystalline. By continuing the fusion the matter penetrated through the crucible. The fused matter was very liquid, and on cooling it formed itself into a crystalline radiant mass of a whitish gray colour having a slight metallic splendour. In other respects it exhibited the same phænomena as pure molybdic acid.

This last method furnishes a very short and œconomical method of obtaining molybdic acid. There can be no doubt that if the molybdena had been entirely free from matrix the calcined mass would have entirely dissolved, except an atom of iron, from which sulphuret of molybdena is never free.

I was desirous also to try the separation of the molybdic acid by the help of ammonia. With this view I made the following experiments.

Experiment XIV.

I digested for twelve hours, in a moderate heat, a gros of the calcined mineral in fine powder with half an ounce of concentrated liquid ammonia, shaking the matter from time to time; after which the liquor was filtered. It had still a strong odour of ammonia. The half of it was decomposed by nitric acid, which precipitated from it molybdic acid in abundance. The other half was evaporated in a small porcelain capsule, aud then heated to a slight degree of incandescence. The ammonia was entirely dissipated, and left a grayish blue residuum, which entered into fusion

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[illegible] after cooling, [illegible] perfectly similar to [illegible] collected after the [illegible] with the ammonia weighed [illegible] the following experiment.

Experiment XV.

[illegible] above mentioned with two [illegible] evaporation, and digested the new [illegible] of liquid ammonia. The residuum [illegible] and dried. It weighed [illegible] evaporated, and after incandescence [illegible] of molybdic acid. The residuum of [illegible] of Nos. X. XI. and XII. [illegible] and quartzy sand.

[illegible] not hesitate to prefer the [illegible] ammonia to that by soda [illegible] For it may happen that [illegible] vitreous fusion with the [illegible] and then ammonia [illegible] molybdic acid from that [illegible] inconvenience may be pre[illegible] too strong towards the end [illegible] the method by ammonia [illegible] perceived [illegible] more expeditious and less ex[illegible]

[illegible] deprived of its [illegible] nitrate of [illegible] means [illegible] may [illegible]

[illegible] Manganese [illegible] that can be found, and reduce it to powder; introduce it into an inclined retort, and torrefy it, to separate the sulphur, and to oxygenate the metal, in a heat which must be immediately raised to a very high degree. When the molybdena has lost its brilliancy, and its black colour has changed to gray, lessen the heat, and continue to expel the last portions of the sulphur, and to complete the oxygenation of the metal at a more moderate heat, continually stirring the matter with an iron spatula. By these means the matter will be prevented from entering into fusion with the oxide of iron and a small quantity of alumine with which they are mixed, and from forming a mass which the ammonia can afterwards decompose only with difficulty, and nothing will remain adhering to the crucible. The mineral is then to be reduced to a very fine powder, and it must be digested several times with a sufficient quantity of liquid ammonia to extract the whole molybdic acid. The different liquids are then to be united, and being poured into a glass retort are distilled to dryness. Molybdate of ammonia will remain in the retort. The heat is increased to incandescence, in order to expel the ammonia. If the retort be too large, the matter must be removed into a smaller.

This process would not answer where it is required to analyse ore of molybdena. In this case it would be necessary to oxygenate the sulphur and the metal by the help of nitric acid. This method will contribute in all probability to ameliorate and render easier the analysis of sulphuret of molybdena.

Addition to the preceding Experiments.

I had undertaken four times, and with the same success, the separation of the molybdic acid by the help of ammonia in the proportions of experiment XIV, when in treating in this manner a pretty large quantity of calcined molybdena I observed a surprising phænomenon which hitherto I had not remarked. I separated the acid from two ounces of molybdena by the help of ammonia. The filtered liquor was left at rest for some days, to see whether any thing would be deposited; which actually was the case. Some flakes which exhibited the same phænomena as oxide of iron were deposited. I again placed the liquor on one side, to see whether a new separation would take place. I had, however, evaporated and decomposed a quantity of the solution of molybdate of ammonia equal to a sixteenth part of the whole liquid. The residuum exhibited the same phænomena exactly as that obtained in experiment XIV, and was pure molybdic acid. Numerous occupations prevented me from undertaking new experiments with the solution of molybdate of ammonia until six weeks after. Having evaporated the solution to dryness, I put the saline mass into a new Hessian crucible in order to expel the ammonia by incandescence. To my great astonishment I observed that the mass deprived of ammonia, which under other circumstances fuses so readily, and which, after being some time in fusion, penetrates into the substance of the crucible, could not this time be brought into fusion even in the strongest white heat, and was only softened, emitting vapours of molybdic acid. On examining the cooled mass, I found at its surface several irregular depressions. It had an argentine splendour inclining to gray. On its fracture it exhibited a cupreous blue colour of steel with a metallic tint; its texture was scaly and very compact; it was difficult to be broken and pulverized. Its specific gravity was 5050. By pouring over it moderately concentrated sulphuric acid the mass acquired a considerable heat, emitting nitrous gas in abundance, and transforming itself into white oxide.

After all these phænomena, it can be considered only as metallic molybdena, or oxide of molybdena very near to the metallic state. I can explain this in no other manner than by admitting, that in consequence of the long contact of the ammonia with the molybdic acid the latter was decomposed. I dare not, however, assert that this was actually the case, and that some other cause which I did not observe may not have concurred to produce the same effect. However this may be, it is remarkable that under the circumstances above mentioned the molybdic acid, the reduction of which is so difficult, should be reduced so easily and by so weak a fire; and that the metallic molybdena, which is so little fusible, should be fused, or at least softened into a homogeneous mass of a scaly texture. The want of molybdena has hitherto prevented me from making the experiments necessary to ascertain the cause of this singular effect.

XXXIII. *Of the general Relation between the Specific Gravities and the Strengths and Values of Spirituous Liquors, and the Circumstances by which the former are influenced.*

[Continued from p. 33.]

Of the Standard of Proof.

§ 14. The standard of comparison which, in conformity to the principles already spoken of in § 3 has been established in this country, is called *proof spirit;* and it appears that some such standard was in use long before we were in the possession of any means of correctly appreciating its strength. This, indeed, has never yet been done; and it is from the want of correctly defining the strength of this standard that a great part of the uncertainty at present existing, with regard to the relative strengths and values of spirituous liquors in general, has originated.

§ 15. When a spirituous compound of any kind contains about a certain quantity of alcohol (which quantity, however, varies considerably in different liquors, and even in liquors of the same description, but differing in their purity), it will afford, on shaking it in a phial, a crown of small bubbles of a peculiar appearance, which gradually go off, or subside without breaking; and which has generally been called by the French the *chapelet,* and by us the *proof.* Liquors of any kind which were capable of exhibiting this phænomenon have, for upwards of two centuries past, been denominated of proof strength.

Upon the average, perhaps, an experienced person may by this means guess within about 6 or 8 per cent. of the real strength of such liquors as are capable of being examined in this way, if in a tolerable state of purity.

The following curious account of the processes for proving spirituous liquors, which is extracted from Mr. Highmore's Treatise on the Excise Laws, published in 1796, vol. ii. p. 283, may, perhaps, serve to give a better idea of the degree of reliance which ought to be placed on them, and of the necessity which there is for a total revision of this important subject:

"Proof spirits, or common saleable goods, are spirits of any kind of a determinate strength, being the same with those of good brandy, and the malt and sugar spirits of the distillery, as they are usually sold, containing equal quantities of rectified spirit and water."

"The best proof spirit is that distilled from French wine;

but for common use may be employed the spirit drawn from melasses."

"The common method of judging of the due strength is by striking the bottom of the sample phial, filled half way with common malt spirit, with the palm of the hand; the bubbles raised on the surface will go off again in a strong manner, without breaking or swelling; and this is the method constantly used by the traders: it is said to be fallacious, and easily open to deception; for if a little treacle, syrup, &c. be added to a quantity of highly rectified spirit of wine, it will give a brandy proof to that spirit. The true strength may, however, always be known by carefully burning away a measured quantity of brandy, &c.; since if it leaves one half water, it is right; if more or less, it is too strong or too weak."

"Perfect proof is that crown of bubbles before mentioned, of a certain size, arising as a head upon a small quantity of a well qualified spirit shaken in a slender phial."

"Proof more than perfect is that in which the bubbles raised by shaking the spirit are larger than those on the common or perfect proof, and go off more suddenly; that is, according as the spirit is higher, or approaches more to the nature of rectified spirit, or, as it is usually called, spirit of wine."

"Proof less than perfect is that wherein the bubbles are smaller, and go off quicker and fainter than in perfect proof; the spirit, in this case, being mixed with more than its own quantity of phlegm, or being too poor for sale."

"The most exact of all methods of determining the strength of any spirit is by distillation, rectifying it up to an alcohol, or totally inflammable spirit; but this, though liable to no error, is too tedious to come into common use. And, upon the whole, the best method of all others seems to be that of *deflagration;* namely, by setting it on fire: if, after it will no longer burn, the remainder is half as much as the quantity measured out for the trial was, then the spirit tried is found to consist of half water, and half totally inflammable spirit; that is, exactly *perfect proof:* and according as the remainder is more or less than half the original quantity, it is so much below or so much above proof, or the due strength of brandy."

"In commerce, with regard to spirits, it would certainly be a much better method to abolish such uncertain proofs, and to make all the goods of the strength of what is called spirits of wine; that is, a totally inflammable spirit, whose purity is much greater, whose strength may always be found with exactness, and whose bulk, stowage, carriage, and incumbrance, would be only half in regard to that of brandy, or proof spirit; and it might at all times, as occasion called for it, be mixed into a great variety of extemporaneous liquors, and the exact degree of strength would be always precisely known."

"This operation, indeed, in the common way, proves so tedious and expensive, and, after all, so short of expectation, and so generally unsatisfactory, that it is not to be expected that the common distillers, till they have fallen into a better manner of working, should come into the proposal. But if, instead of the common way of rectifying by the hot still, they would try the using a large *balneum Mariæ*, made of a large rectangular boiler, and a set of tall conical vessels, they will find that little fire and little attendance, and consequently very little expense, will, in this manner, furnish them with spirits reduced at once to this standard, and greatly superior, in all respects, to the common ones of the same strength. In this case there would be no need of any addition of salts; but the distiller may work more perfectly and more expeditiously without them, and thus preserve the fine essential vinosity of the spirit, which in the common way of working they constantly lose."

"The advantage of this method would be yet greater to apothecaries, to the makers of compound cordial waters, who want only a pure spirit of such a strength, and suffer greatly in the fineness and perfection of their commodities by the spirit they are obliged to use having in it a fulsome and nauseous oil of its own, which will always mix itself with their compositions, and the oils of the aromatics, &c. which they add to it. If spirits were brought to this standard for the market, there would be no possibility of deceit; and no further examination need be made of it by the buyer than its burning perfectly dry in a spoon."

So much for the general state of science with respect to matters of this nature.

§ 16. The only trace which we are able to find of any attempt to define by parliamentary authority what any denomination of strength of a spirituous liquor really signifies, is by a loose passage in the act 2 Geo. III. c. 5, which is as follows: "And for the purposes of this act it is hereby enacted, by the authority aforesaid, that each gallon of brandy or spirits, of the strength of one to six under hydrometer-proof, shall be taken and reckoned at seven pounds and thirteen ounces the gallon."

The reader will in a moment perceive the striking inac-

curacy

effect was produced by putting a little of the same preparation on an electric pile, with a piece of paper interposed, and making a spark pass through the middle of it by means of a metallic plate: the paper will be either perforated or torn.

NEW METHOD OF PREPARING FULMINATING MERCURY.

By Brugnatelli.

The process described by Mr. Howard for obtaining fulminating mercury, which I have several times prepared in my laboratory, suggested to me the idea of preparing this singular production by making ether by nitric acid on mercurial oxides without the application of heat, which frequently produces so rapid and so spontaneous an effervescence that it occasions the loss of a great part of the materials. My process is as follows:

On two gros of the oxidulous sulphate of mercury called *turbith mineral* pour an ounce of pure alcohol, and add at two different times ten gros of concentrated and rutilating nitrous acid. The mixture will immediately enter into effervescence, and the alcohol becomes etherized and is reduced to vapour. These vapours are at first rare and light, but they gradually become more copious and dense: they pass into the receiver, whence they issue in clouds and occupy the lower parts of the apartment, diffusing themselves in the air to a great distance. I never saw any effervescence produce so many vapours. The mercurial mass loses its yellow colour and becomes gray. When the matter has been suffered to cool, collect the concrete part on a filter, and wash and dry it. This substance fulminates like Howard's mercury, and possesses all the other properties of that preparation.

By treating in the same manner the other mercurial oxides, I was able to convert them all into fulminating mercury.

The quantity of white vapours disengaged during this process, and their singular gravity, which far surpasses that of air, induced me to make researches respecting their nature. For this purpose I proceeded in the following manner:—I put into a glass tubulated retort two gros of turbith mineral, adapted to it a double-necked retort, and to the latter a second large tubulated receiver. Having luted the joinings, I poured over the mercurial powder in the retort an ounce of alcohol, and then a gros of concentrated rutilating nitrous acid, closing exactly the tubulature. An effervescence took place, and the white vapours soon filled the lower part of the receivers. When the effervescence had subsided, and the apparatus had cooled, there was found in the retort a mercurial mass of a very white colour and highly fulminating. The receivers contained an ethereous oily liquor, and crystals of salt, which were carefully collected, were observed on their sides. They were exceedingly soluble in water. On examination I found them to be nitrate of mercury.

The vapours disengaged during this operation are therefore composed of ether by nitric acid and nitrous gas holding in solution nitrate of mercury. The latter circumstance, on which the great gravity of these vapours depends, is truly worthy of attention. I was not able to ascertain whether there was a concomitant formation of ammonia, as Berthollet supposes.

DISCOVERY OF A DETONATING OXYGENATED MURIATE OF LEAD. *By the same.*

I obtained an oxygenated muriate of lead by saturating with oxygenated muriatic acid a solution of lead in nitric acid. This solution, which was transparent and colourless, acquired by the addition of oxygen gas a yellow colour. I reduced it to two-thirds by slow evaporation, and left it at rest. By cooling, it gave a quantity of very small brilliant cubes, having a sweet and cool taste. These crystals attract humidity in humid air, and become dry in dry air. Sulphuric acid decomposes them with a disengagement of simple muriatic acid gas: when struck with a small bit of phosphorus they detonate violently, and are fused on burning coals.

If simple muriatic acid gas be made to pass into a solution of muriate of lead impregnated with a little oxygenated muriatic acid gas, the metal is precipitated under the form of very small crystals of muriate of lead: on evaporating the liquid there are obtained small needles of the same salt. Crystallization then furnishes the two salts above mentioned. Simple muriate of lead neither detonates with phosphorus, nor fuses on burning coals.

NEW ANIMALS.

A letter from Paris says: " A couple of living quadrupeds, entirely unknown to naturalists, have been sent home by captain Baudin, in the *Naturaliste* lately arrived. Professor Geoffroi calls them *fascolomes.* They come from the western coast of New Holland; their fur may be of some utility; and their flesh, in the opinion of captain Hamelin and his crew, affords excellent food. The *fascolomes*

acid. The alkaline earths, and earths which with the molybdic acid form salts difficult of solution, could not answer my purpose to separate the acid from the insoluble substances with which they are mixed. It remained to try soda and ammonia, which I found to be both equally proper under circumstances peculiar to each of them, for accomplishing the proposed end.

Experiment X.

I boiled two gros of calcined molybdena with eight ounces of water, and instilled into the solution a ley of carbonate of soda until no more effervescence took place. An excess of alkali was added, and the matter was left to boil for half an hour: it was then filtered, and the concrete matter which remained on the filter was edulcorated. The different liquors were evaporated to two ounces, and nitric acid to excess was instilled into the warm liquid. During the instillation of the nitric acid a crystalline white powder was deposited, and after cooling there were formed in the liquor crystals of a certain size. A new quantity was obtained by evaporation. The different salts were washed and dried. The residuum on the filter, after being washed and dried, weighed thirty grains, and had the reddish colour of oxide of iron.

Experiment XI.

The preceding experiment was repeated, with this difference: the liquor, after the effervescence had ceased, was boiled for half an hour longer. The matter, after being reduced to two ounces, and filtered, was suffered to cool before the nitric acid was added. The addition of this acid caused to be precipitated a larger quantity of the white powder, which had a more crystalline aspect, and which after edulcoration and desiccation had the splendour of mother-of-pearl. As I did not add so much acid in this experiment as in the former, the liquor was less charged with molybdic acid. The residuum, when washed and dried, weighed also thirty grains.

As I suspected that this residuum might contain a portion of molybdic acid less soluble than the preceding, I tried to extract this acid by boiling the residuum with nitric acid. With this view I made the following experiment.

Experiment XII.

The thirty grains of the residuum of the two last experiments were boiled with an ounce of nitric acid of the specific gravity 1·250 and half an ounce of water, until the whole a fourth and fifth pile composed one of disks of flesh suffered to putrefy in the sun for fifteen days, and the other formed of disks of the same substance moistened in oxygenated muriatic acid mixed with nine parts of distilled water. The results of these experiments, by demonstrating more and more that the Galvanic fluid, drawn from the different substances which compose the pile, has the power of carrying with it, in its circulation, different matters analogous to the respective bodies through which it passes, have induced Rossi, Vassalli-Eandi, and Giulio, to conclude,

1stly, That Galvanism, though arising from electricity, which, as we may say, is its basis, is not simple electricity, but electricity so modified, that its effects are in no manner similar to those of electricity properly so called.

2dly, That the oxygenated muriatic acid combined with distilled water, in proportions always determined by the different cases in which it is employed, may be used with the greatest advantage in the cure of various maladies.

The latter discovery made by C. Rossi has been applied by him with the completest success. He has employed oxygenated muriatic acid externally in the manner above mentioned for the cure of very extensive gangrenous ulcers. The effects of this new remedy have been exceedingly great, both in the hospital of Moncalieri, where several individuals, treated without success by the common means for several months, were entirely cured by this method; and in the hospital of St. John, where the effects were so speedy, that in the course of twenty-four hours gangrenous ulcers of the legs were reduced to the state of simple ulcers.

ELECTRICITY.

Vassalli-Eandi has confirmed, by a series of experiments publicly repeated before Lelievre the celebrated mineralogist, member of the National Institute of France, what he had proved in the year 1790,

1stly, That metals and their oxides, thrown on his electrometer, bring thither a contrary kind of electricity: the metal positive electricity, and its oxide negative.

2dly, That the electric fluid does not affect the fluid of the Voltaic pile, the action of which is not altered by the union of positive electricity to the negative of the pile, nor by another combination of electric and Galvanic conductors. It is from these and other experiments of the same kind that he has deduced the theory of Galvanism and its effects, which he explained in the last sitting of his public experiments at the Athenæum of Turin.

EXPEDITION OF CAPTAIN BAUDIN.

Copy of a Letter addressed to C. Gregoire, Member of the French Senate.

Straits of Basle, King's Island,
Nov. 27th, 1802.

You have certainly heard the result of the expedition of captain Baudin since our departure from France. You must have seen that, after staying forty days at the Isle of France, we proceeded to the coast of New Holland, which we explored from Cape Lewin to the Bay of Seals. All this sandy coast, nearly destitute of fresh water, is almost uninhabited. The few inhabitants who reside on it are still as savage as in the time of Dampierre. Being nearer to the state of nature than any other people, they are almost as fierce, and possess no art except that of sharpening sticks to defend themselves from their enemies, or to procure those provisions with which they can be supplied either by hunting or fishing.

On quitting this coast we proceeded to Timor, one of the islands to the south of the Moluccas, where we found a mild and lively people in a state of demi-civilization. The inhabitants of the coast have become hospitable towards strangers by their commerce with the Dutch; but those who reside in the interior, to whom an European visage is still unknown, behave with cruelty towards those who venture to penetrate into their country.

From Timor we directed our course to Van Diemen's Land. This island is inhabited by people of a different race from those of New Holland. The latter have long hair like the Asiatics, though their skin is as black as that of the African negroes, while the former have woolly curled hair like the inhabitants of Congo. The difference between them is shown by other characters also: the former, habituated to see European vessels from time to time on their coasts, are less savage than the other tribes of these countries.

From Van Diemen's Land we proceeded to Port Jackson. This infant colony is the first the inhabitants of which have no cause of complaint against the Europeans. Here they are treated with every kind of attention, but they have always rejected civilization: though they have lived for fifteen years with the English they have not yet adopted any of their customs. Clothes are still to them a superfluity: they sometimes, though seldom, wear any thing to defend them from the cold, but never to conceal their nudity. Their language alone has undergone some changes.

The English, in the course of the fifteen years they have been settled here, have carried cultivation to a very advanced state. The antient forests have disappeared, and nothing is seen but fields of wheat, which are remarkably productive. We have found here towns and villages where every European article, and even superfluities, are to be had in abundance. The population amounts to nearly 8000, without any slaves. I have sent you a specimen of the wool of the sheep of this country. They came originally from Peru, Paraguai, the Cape of Good Hope, and Bengal. They have already improved in a singular manner, and still promise more. Those of Bengal, which were clothed only with hair, have already produced young ones covered with rich fleeces. A residence of five months enabled me to traverse the whole country. We have just left it in order to explore those parts of New Holland which we have not yet visited. The commodore has sent the *Naturaliste* back to France laden with the collections we have made. I have left that vessel to go on board the *Geographe*, and to supply the place of my friend Depuch, who returns to France on account of his health.

BAILLY, Mineralogist.

FIRE-BALL.

On the 4th instant (July) a ball of fire struck the White Bull, public-house, kept by John Hubbard, at East Norton. The chimney was thrown down by it, the roof in part torn off, the windows shattered to atoms, and the dairy, pantry, &c. converted into a heap of rubbish. It appeared like a luminous ball of considerable magnitude; and, on coming in contact with the house, exploded with a great noise and a very oppressive sulphureous smell. Some fragments of this ball were found near the spot, and subjected to chemical analysis by a gentleman in the neighbourhood, who found them to consist of the same ingredients as those stones of similar origin analysed by Mr. Howard and other chemists, and nearly in the same proportions. The surface of these stones is of a dark colour, and varnished as if by fusion. From some indentures on the surface it appears probable that the ball was soft when it descended; and it was obviously in a state of ignition, as the grass, &c. is burnt up where the fragments fell. Its motion while in the air was very rapid, and apparently parallel to the horizon.

SINGULAR CAVERN.

A singular cavern has lately been discovered two leagues from Nizza, in the district of Falcion. It has a narrow entrance, and the interior divisions, which have not yet been sufficiently explored, contain natural temples supported by columns formed by crystallizations. Some of these columns are so large that 400 persons cannot encompass them. On account of the great reflection, very little light is required to illuminate this cavern.

ASTRONOMY.

The astronomers Delalande junior and Burckhardt observe with great assiduity the planet discovered by Dr. Olbers on the 28th of March 1802. Its longitude on the 1st of July at 11^h 45′ was 9 signs 7° 14′ 25″, and its latitude 46° 23′ 18″. Burckhardt has thence deduced its revolution to be 1682 days, or four years seven months and twelve days; which is a day less than what was found some months ago, as may be seen in my *Bibliographie Astronomique* just published; but at present there is scarcely an uncertainty of a few hours. He is employed in calculating the derangements it must experience from the attraction of Jupiter, and which are very complex; but he has presented to the Institute a learned memoir which leads to this research.

JEROME DELALANDE.

Table of the geocentric motions of the two new planets for the month of August:

	A. R. of Pallas.			Declin. North.		A. R. Ceres Ferd.			Declin. South.	
Aug. 4	17^h	59^m	52^s	19°	3′	18^h	15^m	52^s	29°	48′
7	17	58	44	18	31	18	14	32	29	51
10	17	57	48	17	59	18	13	20	29	55
13	17	56	52	17	27					
16	17	56	20	16	53					
19	17	55	56	16	19					
22	17	55	44	15	44					
25	17	53	40	15	9					
28	17	53	48	14	33					

The calculations of Ceres after the 10th have not yet come to hand.

XXXII. *Materials towards a more intimate Knowledge of Native Molybdena, containing a very advantageous Method of extracting Molybdic Acid from that Sulphuret.* *By* C. F. Bucholz *.

After the repeated labours of a great many chemists, such as Quist, Scheele, Bergman, Heyer, Ilsemann, Pelletier, Richter, &c. on sulphuret of molybdena, both in regard to a better method of extracting molybdic acid from that mineral, and in regard to the phænomena it exhibits with other bodies, one might believe that no further researches remain to be made on this subject. But by reflecting properly on what has been done, we shall soon be convinced of the contrary. As my principal object in this memoir is to make known a more advantageous method of preparing molybdic acid, it will be requisite, to justify the necessity of such a method, to recapitulate the old and new processes, in order to show their defects, and to compare them afterwards with mine.

The first process for extracting molybdic acid from molybdena consists in separating the sulphur from the molybdena by a strong heat, and converting the remaining metal into acid by continuing to heat it in the air. The vapours of the molybdic acid are received in a proper vessel, where they are condensed under the form of yellowish white scales, which are alone considered to be molybdic acid. As these scales are formed very slowly and in small quantity, and as the volatility of the molybdic acid in a moderately strong heat on the one hand, and on the other the property of the same acid of fusing in a very strong heat, and of penetrating, during its fusion, the matter of the crucible, render it impossible to obtain in this manner a certain quantity of acid, this method cannot in any point of view be considered as advantageous or even practicable: we shall therefore proceed to examine the second method.

The second process, which like the first belongs to Scheele, consists in distilling off at different times nitric acid of a moderate strength from molybdena until the sulphur and the metal are both converted into acid. This operation requires from 20 to 24 parts of nitric acid for one part of sulphuret of molybdena, and it leaves with the molybdic acid the iron and earths which the purest molybdena

always contains in greater or less quantity. This process, which was considered as the only one proper for obtaining pure molybdic acid, besides its not answering the proposed end, is too expensive to be followed, and therefore another more œconomical and more proper for obtaining an acid free from foreign mixture is desirable to be substituted in its stead.

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* From Scherer's *Allgemeinen Journal der Chemie* 1802, no. 5. p. 485.

The third process consists in distilling the sulphurated molybdena with arsenic acid, and in expelling by volatilization the oxide and sulphuret of arsenic which are formed. This method also is so expensive and so dangerous that we shall not enter into any further discussion of it.

The fourth process, which belongs also to Scheele, is that which furnishes the acid in the speediest manner; but it does not furnish it pure. It consists in causing the molybdena to deflagrate with nitrate of potash in proportions respecting which authors vary. Scheele[*] took four parts of nitrate to one of sulphuret of molybdena; Lampadius[†] recommends six parts of nitrate to one of molybdena, and Fourcroy[‡] three parts of nitrate to one of molybdena.

The sulphur and the metal are here converted into acids, and in that state combine with the potash. A portion of the nitrate remains undecomposed, and there is formed at the same time deoxygenated nitrate: the remaining mass is dissolved in warm water; and the molybdate of potash, still mixed with its other salts, is decomposed (or this may be done after it has been separated by crystallization) by means of the nitric, muriatic, or sulphuric acids. The molybdic acid, when the solution is not too dilute, separates under the concrete form: this precipitate is not pure molybdic acid, but, as Scheele before announced, molybdic acid holding potash, or, according to Fourcroy, acidulous molybdate of that alkali.

This acidulous molybdate is much more soluble in water than pure molybdic acid, which for its solution requires 500 parts of that liquid, whereas acidulous molybdate dissolves in three or four parts of water. The latter fuses in the fire sooner than the acid, and more readily corrodes the crucible. After cooling, it is of a beautiful yellow colour: the acid, on the contrary, exhibits a radiant mass of a whitish gray colour, having a slight metallic brilliancy. The acidulous molybdate does not suffer itself to be sublimated; whereas the pure acid is vaporized by heat, and deposits itself, on cold bodies presented to it, under the form of needles and yellow scales. It appears that acidulous molybdate has often been employed for pure molybdic acid. Thus Lampadius gives reason to believe that the acid which he prepared by the above process was not pure acid but acidulous molybdate. He recommends decomposing the mass which remains after deflagration by muriatic acid in excess. It will be seen by the following experiments that the molybdic acid cannot be entirely freed from the potash. To remove this obstacle to the preparation of pure molybdic acid, Scheele recommends dissolving the precipitate obtained in the smallest quantity of water possible, and boiling it for some minutes with a new quantity of nitric acid. When the alkali has been thus separated, the molybdic acid is supposed to deposit itself in very small crystals. It appears to me confirmed that in this manner there is separated a little pure acid; but the very large part of the acid which remains dissolved with the acid added in excess is obtained separately with very great difficulty; and when enough of acid is not added, a mixture of pure acid and acid holding alkali is obtained. As this fourth process appeared to me far preferable to the rest, being much shorter and more œconomical, I wished to try whether it was not possible still to improve it, and to render it common in the preparation of the molybdic acid.

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[*] Opuscula Physica et Chemica, tom. i. p. 202.

[†] Handbuch der Chemischen Analyse, p. 325.

[‡] Système des Connoissances Chimiques.

As the proportion of nitre, even in the process of Fourcroy, appeared to me still too great, I was desirous to examine whether something might not be saved in this point of view.

Experiment I.

I heated to redness in a Hessian crucible six gros of saltpetre, and projected on it, in parts, pure sulphurated molybdena pulverized. After I had added two gros and twenty grains of this mineral, the deflagration was still very brisk. I maintained the matter in fusion for some minutes longer, after which I dissolved the mass in water. The solution was complete, a few grains of oxide of iron and of silex excepted. On proceeding to the crystallization of the salts contained in this mass, I obtained some small crystals of undecomposed nitre.

Experiment II.

According to the indication furnished by the preceding experiment, I took two pounds three quarters of purified nitre, which I brought to red fusion, and gradually projected on it a pound of pulverized sulphuret of molybdena.

lomes are interesting to naturalists in particular on account of the singularity of their organization. They resemble the marmot in the form of the head; the number, the nature, and arrangement of their teeth; and by the conformation of their fore-feet, which they employ for burrowing in the earth: but they differ by the existence of a bag under the belly of the female, and by the whole apparatus of the organs of generation, which are like those of the *sarique* of Buffon. The hind-feet also are formed like those of that animal, the thumb being separated from the other toes, and destitute of claws. The tail is so short that it remains concealed among the hair: the latter is brown, tufted, and very long.

"The *fascolomes* in the menagerie are still young, and larger than rabbits. They are remarkably gentle. They may be touched, and carried from one place to another, without showing the least fear, anger, or discontent. Their gait is heavy and embarrassed: they live under the earth, sleep in the day-time, and in the night go in quest of food: in general they have little activity and energy: they scratch themselves like apes. They are fed with bread, milk, roots, and all sorts of herbs."

GALVANISM.

A letter from Turin, dated 25th June, contains the following particulars:—"The experiments made by the Galvanic Committee of Turin, and by several members of the Academy of Sciences, have greatly contributed to the rapid progress made by this part of the physical sciences: but what ought to distinguish the success of the learned philosophers of which this committee is composed, is the advantage with which they have applied Galvanism to the animal œconomy, and the well-arranged series of experiments they have made to determine its influence on the different diseases with which man may be afflicted. Of the various trials made with great success by Rossi, Vassalli-Eandi, and Giulio, we shall mention that only of Rossi with Galvanic piles of a new composition. Animated by the most ardent desire of rendering Galvanism useful to suffering humanity, Rossi constructed disks with the cancerous tumours extirpated from a man in the hospital of St. John. He varied this apparatus by employing these new disks sometimes without moistening them, and sometimes moistening them in water mixed with a tenth of its volume of oxygenated muriatic acid. He compared the results produced by these two different piles with those obtained by means of the common pile, and by a fourth and fifth pile composed one of disks of flesh suffered to putrefy in the sun for fifteen days, and the other formed of disks of the same substance moistened in oxygenated muriatic acid mixed with nine parts of distilled water. The results of these experiments, by demonstrating more and more that the Galvanic fluid, drawn from the different substances which compose the pile, has the power of carrying with it, in its circulation, different matters analogous to the respective bodies through which it passes, have induced Rossi, Vassalli-Eandi, and Giulio, to conclude,

1stly, That Galvanism, though arising from electricity, which, as we may say, is its basis, is not simple electricity, but electricity so modified, that its effects are in no manner similar to those of electricity properly so called.

2dly, That the oxygenated muriatic acid combined with distilled water, in proportions always determined by the different cases in which it is employed, may be used with the greatest advantage in the cure of various maladies.

The latter discovery made by C. Rossi has been applied by him with the completest success. He has employed oxygenated muriatic acid externally in the manner above mentioned for the cure of very extensive gangrenous ulcers. The effects of this new remedy have been exceedingly great, both in the hospital of Moncalieri, where several individuals, treated without success by the common means for several months, were entirely cured by this method; and in the hospital of St. John, where the effects were so speedy, that in the course of twenty-four hours gangrenous ulcers of the legs were reduced to the state of simple ulcers.

ELECTRICITY.

Vassalli-Eandi has confirmed, by a series of experiments publicly repeated before Lelievre the celebrated mineralogist, member of the National Institute of France, what he had proved in the year 1790,

1stly, That metals and their oxides, thrown on his electrometer, bring thither a contrary kind of electricity: the metal positive electricity, and its oxide negative.

2dly, That the electric fluid does not affect the fluid of the Voltaic pile, the action of which is not altered by the union of positive electricity to the negative of the pile, nor by another combination of electric and Galvanic conductors. It is from these and other experiments of the same kind that he has deduced the theory of Galvanism and its effects, which he explained in the last sitting of his public experiments at the Athenæum of Turin.

EXPEDITION OF CAPTAIN BAUDIN.

Copy of a Letter addressed to C. Gregoire, Member of the French Senate.

Straits of Basle, King's Island,
Nov. 27th, 1802.

You have certainly heard the result of the expedition of captain Baudin since our departure from France. You must have seen that, after staying forty days at the Isle of France, we proceeded to the coast of New Holland, which we explored from Cape Lewin to the Bay of Seals. All this sandy coast, nearly destitute of fresh water, is almost uninhabited. The few inhabitants who reside on it are still as savage as in the time of Dampierre. Being nearer to the state of nature than any other people, they are almost as fierce, and possess no art except that of sharpening sticks to defend themselves from their enemies, or to procure those provisions with which they can be supplied either by hunting or fishing.

On quitting this coast we proceeded to Timor, one of the islands to the south of the Moluccas, where we found a mild and lively people in a state of demi-civilization. The inhabitants of the coast have become hospitable towards strangers by their commerce with the Dutch; but those who reside in the interior, to whom an European visage is still unknown, behave with cruelty towards those who venture to penetrate into their country.

From Timor we directed our course to Van Diemen's Land. This island is inhabited by people of a different race from those of New Holland. The latter have long hair like the Asiatics, though their skin is as black as that of the African negroes, while the former have woolly curled hair like the inhabitants of Congo. The difference between them is shown by other characters also: the former, habituated to see European vessels from time to time on their coasts, are less savage than the other tribes of these countries.

From Van Diemen's Land we proceeded to Port Jackson. This infant colony is the first the inhabitants of which have no cause of complaint against the Europeans. Here they are treated with every kind of attention, but they have always rejected civilization: though they have lived for fifteen years with the English they have not yet adopted any of their customs. Clothes are still to them a superfluity: they sometimes, though seldom, wear any thing to defend them from the cold, but never to conceal their nudity. Their language alone has undergone some changes.

The English, in the course of the fifteen years they have been settled here, have carried cultivation to a very advanced state. The antient forests have disappeared, and nothing is seen but fields of wheat, which are remarkably productive. We have found here towns and villages where every European article, and even superfluities, are to be had in abundance. The population amounts to nearly 8000, without any slaves. I have sent you a specimen of the wool of the sheep of this country. They came originally from Peru, Paraguai, the Cape of Good Hope, and Bengal. They have already improved in a singular manner, and still promise more. Those of Bengal, which were clothed only with hair, have already produced young ones covered with rich fleeces. A residence of five months enabled me to traverse the whole country. We have just left it in order to explore those parts of New Holland which we have not yet visited. The commodore has sent the *Naturaliste* back to France laden with the collections we have made. I have left that vessel to go on board the *Geographe*, and to supply the place of my friend Depuch, who returns to France on account of his health.

BAILLY, Mineralogist.

FIRE-BALL.

On the 4th instant (July) a ball of fire struck the White Bull, public-house, kept by John Hubbard, at East Norton. The chimney was thrown down by it, the roof in part torn off, the windows shattered to atoms, and the dairy, pantry, &c. converted into a heap of rubbish. It appeared like a luminous ball of considerable magnitude; and, on coming in contact with the house, exploded with a great noise and a very oppressive sulphureous smell. Some fragments of this ball were found near the spot, and subjected to chemical analysis by a gentleman in the neighbourhood, who found them to consist of the same ingredients as those stones of similar origin analysed by Mr. Howard and other chemists, and nearly in the same proportions. The surface of these stones is of a dark colour, and varnished as if by fusion. From some indentures on the surface it appears probable that the ball was soft when it descended; and it was obviously in a state of ignition, as the grass, &c. is burnt up where the fragments fell. Its motion while in the air was very rapid, and apparently parallel to the horizon.

SINGULAR CAVERN.

A singular cavern has lately been discovered two leagues from Nizza, in the district of Falcion. It has a narrow entrance, and the interior divisions, which have not yet been sufficiently explored, contain natural temples supported by columns formed by crystallizations. Some of these columns are so large that 400 persons cannot encompass them. On account of the great reflection, very little light is required to illuminate this cavern.

ASTRONOMY.

The astronomers Delalande junior and Burckhardt observe with great assiduity the planet discovered by Dr. Olbers on the 28th of March 1802. Its longitude on the 1st of July at 11^h 45′ was 9 signs 7° 14′ 25″, and its latitude 46° 23′ 18″. Burckhardt has thence deduced its revolution to be 1682 days, or four years seven months and twelve days; which is a day less than what was found some months ago, as may be seen in my *Bibliographie Astronomique* just published; but at present there is scarcely an uncertainty of a few hours. He is employed in calculating the derangements it must experience from the attraction of Jupiter, and which are very complex; but he has presented to the Institute a learned memoir which leads to this research.

JEROME DELALANDE.

Table of the geocentric motions of the two new planets for the month of August:

	A. R. of Pallas.	Declin. North.	A. R. Ceres Ferd.	Declin. South.
Aug. 4	17^h 59^m 52^s	19° 3′	18^h 15^m 52^s	29° 48′
7	17 58 44	18 31	18 14 32	29 51
10	17 57 48	17 59	18 13 20	29 55
13	17 56 52	17 27		
16	17 56 20	16 53		
19	17 55 56	16 19		
22	17 55 44	15 44		
25	17 53 40	15 9		
28	17 53 48	14 33		

The calculations of Ceres after the 10th have not yet come to hand.

XXXII. *Materials towards a more intimate Knowledge of Native Molybdena, containing a very advantageous Method of extracting Molybdic Acid from that Sulphuret. By* C. F. Bucholz*.

After the repeated labours of a great many chemists, such as Quist, Scheele, Bergman, Heyer, Ilsemann, Pelletier, Richter, &c. on sulphuret of molybdena, both in regard to a better method of extracting molybdic acid from that mineral, and in regard to the phænomena it exhibits with other bodies, one might believe that no further researches remain to be made on this subject. But by reflecting properly on what has been done, we shall soon be convinced of the contrary. As my principal object in this memoir is to make known a more advantageous method of preparing molybdic acid, it will be requisite, to justify the necessity of such a method, to recapitulate the old and new processes, in order to show their defects, and to compare them afterwards with mine.

The first process for extracting molybdic acid from molybdena consists in separating the sulphur from the molybdena by a strong heat, and converting the remaining metal into acid by continuing to heat it in the air. The vapours of the molybdic acid are received in a proper vessel, where they are condensed under the form of yellowish white scales, which are alone considered to be molybdic acid. As these scales are formed very slowly and in small quantity, and as the volatility of the molybdic acid in a moderately strong heat on the one hand, and on the other the property of the same acid of fusing in a very strong heat, and of penetrating, during its fusion, the matter of the crucible, render it impossible to obtain in this manner a certain quantity of acid, this method cannot in any point of view be considered as advantageous or even practicable: we shall therefore proceed to examine the second method.

The second process, which like the first belongs to Scheele, consists in distilling off at different times nitric acid of a moderate strength from molybdena until the sulphur and the metal are both converted into acid. This operation requires from 20 to 24 parts of nitric acid for one part of sulphuret of molybdena, and it leaves with the molybdic acid the iron and earths which the purest molybdena always

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* From Scherer's *Allgemeinen Journal der Chemie* 1802, no. 5. p. 485.

always contains in greater or less quantity. This process, which was considered as the only one proper for obtaining pure molybdic acid, besides its not answering the proposed end, is too expensive to be followed, and therefore another more œconomical and more proper for obtaining an acid free from foreign mixture is desirable to be substituted in its stead.

The third process consists in distilling the sulphurated molybdena with arsenic acid, and in expelling by volatilization the oxide and sulphuret of arsenic which are formed. This method also is so expensive and so dangerous that we shall not enter into any further discussion of it.

The fourth process, which belongs also to Scheele, is that which furnishes the acid in the speediest manner; but it does not furnish it pure. It consists in causing the molybdena to deflagrate with nitrate of potash in proportions respecting which authors vary. Scheele* took four parts of nitrate to one of sulphuret of molybdena; Lampadius† recommends six parts of nitrate to one of molybdena, and Fourcroy‡ three parts of nitrate to one of molybdena.

The sulphur and the metal are here converted into acids, and in that state combine with the potash. A portion of the nitrate remains undecomposed, and there is formed at the same time deoxygenated nitrate: the remaining mass is dissolved in warm water; and the molybdate of potash, still mixed with its other salts, is decomposed (or this may be done after it has been separated by crystallization) by means of the nitric, muriatic, or sulphuric acids. The molybdic acid, when the solution is not too dilute, separates under the concrete form: this precipitate is not pure molybdic acid, but, as Scheele before announced, molybdic acid holding potash, or, according to Fourcroy, acidulous molybdate of that alkali.

This acidulous molybdate is much more soluble in water than pure molybdic acid, which for its solution requires 500 parts of that liquid, whereas acidulous molybdate dissolves in three or four parts of water. The latter fuses in the fire sooner than the acid, and more readily corrodes the crucible. After cooling, it is of a beautiful yellow colour: the acid, on the contrary, exhibits a radiant mass of a whitish gray colour, having a slight metallic brilliancy. The acidulous molybdate does not suffer itself to be sublimated; whereas the pure acid is vaporized by heat, and deposits itself,

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* Opuscula Physica et Chemica, tom. i. p. 202.

† Handbuch der Chemischen Analyse, p. 325.

‡ Système des Connoissances Chimiques.

self, on cold bodies presented to it, under the form of needles and yellow scales. It appears that acidulous molybdate has often been employed for pure molybdic acid. Thus Lampadius gives reason to believe that the acid which he prepared by the above process was not pure acid but acidulous molybdate. He recommends decomposing the mass which remains after deflagration by muriatic acid in excess. It will be seen by the following experiments that the molybdic acid cannot be entirely freed from the potash. To remove this obstacle to the preparation of pure molybdic acid, Scheele recommends dissolving the precipitate obtained in the smallest quantity of water possible, and boiling it for some minutes with a new quantity of nitric acid.

When the alkali has been thus separated, the molybdic acid is supposed to deposit itself in very small crystals. It appears to me confirmed that in this manner there is separated a little pure acid; but the very large part of the acid which remains dissolved with the acid added in excess is obtained separately with very great difficulty; and when enough of acid is not added, a mixture of pure acid and acid holding alkali is obtained. As this fourth process appeared to me far preferable to the rest, being much shorter and more œconomical, I wished to try whether it was not possible still to improve it, and to render it common in the preparation of the molybdic acid.

As the proportion of nitre, even in the process of Fourcroy, appeared to me still too great, I was desirous to examine whether something might not be saved in this point of view.

Experiment I.

I heated to redness in a Hessian crucible six gros of saltpetre, and projected on it, in parts, pure sulphurated molybdena pulverized. After I had added two gros and twenty grains of this mineral, the deflagration was still very brisk. I maintained the matter in fusion for some minutes longer, after which I dissolved the mass in water. The solution was complete, a few grains of oxide of iron and of silex excepted. On proceeding to the crystallization of the salts contained in this mass, I obtained some small crystals of undecomposed nitre.

Experiment II.

According to the indication furnished by the preceding experiment, I took two pounds three quarters of purified nitre, which I brought to red fusion, and gradually projected on it a pound of pulverized sulphuret of molybdena.

When the whole quantity of molybdena was introduced, a very brisk deflagration took place. I dissolved the saline mass in distilled water. The whole dissolved except about ten gros, which were oxide of iron and silex. The ley furnished by crystallization a little undecomposed saltpetre, nitrite of potash, and gaso-nitrous sulphates and molybdates of potash, or holding nitrous gas.

These experiments prove that 2¾ parts of nitre are sufficient to deprive of its sulphur one part of sulphurated molybdena, and to oxygenate the metal. This quantity might even be diminished, were it not to be apprehended that the small portion of undecomposed sulphur which is found in the mass towards the end of the operation would with difficulty be attacked by too small a quantity of nitre.

To find the means of separating the molybdic acid from the acidulous molybdate of potash thus obtained, I made the following experiments.

Experiment III.

To a portion of the above solution I added sulphuric acid till nothing more was precipitated and the mixture had acquired a strongly acid taste, after which it was digested cold for two hours. The precipitate obtained was washed several times with distilled water, and it was heated with twenty parts of the same water. The whole of it readily dissolved.

To this solution I added nitric acid until there was a considerable excess. No precipitate was formed. I evaporated the liquor to dryness. The residuum contained no crystallized part, had no taste of saltpetre, and did not deflagrate when thrown on burning coals. This residuum, which was yellow, easily fused in an ignited crucible without being evaporated by a strong heat. After cooling, it was found to have assumed a yellower colour. This experiment evidently proves that acidulous molybdate of potash is not decomposed either by sulphuric acid assisted by a digesting heat, or by nitric acid; for the precipitate obtained was exceedingly soluble, and was not volatile at a white heat.

As I had evaporated the solution of molybdate conjointly with nitric acid, it was possible that the nitrate of potash formed in the first instance might afterwards be decomposed by the molybdic acid separated. I therefore resolved to repeat the same operation with nitric acid.

Experiment IV.

I decomposed a part of the above solution of acidulous molybdate, adding an excess of nitric acid. A large portion of the precipitate was redissolved. After some hours digestion I tried the nature of the precipitate, after having collected and washed it on a filter. On putting a little of the precipitate, still moist, into a Hessian crucible, and exposing it to heat, it resolved itself into a transparent liquor. When the whole moisture was dissipated, and the crucible had begun to be red, the matter entered into fusion; but only a very weak volatilization was observed by an increase of heat. The fused mass was of a yellowish gray colour. A great part of it had passed through the crucible. It resulted from these phænomena, compared with those which the free acid exhibited with water and in the fire, that the precipitate obtained in this experiment was a mixture of free molybdic acid and acidulous molybdate of potash.

After this imperfect success I resolved to try the muriatic acid to decompose acidulous molybdate of potash.

Experiment V.

I decomposed the above-mentioned solution by means of muriatic acid. An abundant precipitate was formed. I added a strong excess of acid, which dissolved a remarkable quantity of the precipitate. I digested the mixture for some hours in a warm place, which completed the solution of almost the whole it. After cooling, there was formed in the liquor a considerable number of very small crystals. I tried these crystals, and found that they were almost entirely soluble in three or four parts of warm water; that they readily fused in an ignited crucible without emitting much vapour; that they penetrated the matter of the crucible; and that after cooling they had a grayish yellow colour. I thence concluded that the precipitate obtained by the muriatic acid contained scarcely any free acid, but was almost entirely composed of acidulous molybdate. I then evaporated to one-half, in a moderate heat, the solution which still contained molybdic acid. During the evaporation, but in particular after cooling, there was separated a quantity of small yellowish white crystals: there, however, remained in the solution a still greater quantity of molybdic acid, as was indicated by its acid taste and the trials to which it was subjected. The crystals obtained exhibited the same phænomena with water, and in the fire, as pure molybdic acid.

These experiments did not furnish a method of preparing, with safety and advantage, pure molybdic acid, and of obtaining the whole quantity contained in the sulphurated

molybdena;

molybdena; or there was formed acidulous molybdate of potash which was not decomposed; or only a little molybdic acid was separated; or there remained in the liquid too large a quantity of acid, which could be separated only incompletely and with difficulty. In this unfavourable state of things I thought of several means for decomposing the molybdate by the help of double affinities, but I was not able by any of the known means to separate the acid completely from the alkali. I therefore resolved to recur to the old method, which consists in oxygenating the sulphuret by nitric acid, taking care to correct the process by separating the greater part of the sulphur by calcination. By this preliminary separation of the sulphur a great quantity of the acid is saved, and the labour is considerably shortened. I consequently proceeded to calcination in the following manner.

Experiment VI.

Five ounces and a half of sulphuret of molybdena in fine powder, and which were perfectly pure, a few small particles of quartz and oxide of iron excepted, were introduced into a large Hessian crucible, which, for the greater convenience, was placed obliquely in a furnace and surrounded by charcoal. When the matter was red it was stirred incessantly, and in turns, by me and my friend M. Haberle, to whom I was indebted for the molybdena employed in these different experiments. During the first hour, and before the greater part of the sulphur was dissipated, the matter retained its lightness: it then united into a mass, and its black colour passed successively to gray, reddish gray, and then to whitish gray.

When the whole sulphur was driven off, which required two hours, the mass by an increase of heat coagulated more and more, and even began to fuse at the bottom of the crucible, and it appeared by the vapour which rose that the molybdena was volatilizing. Having maintained a moderate fire for half an hour, I took the crucible from the fire and examined the matter. It had a whitish gray colour, exhibited here and there splendour and a crystalline form, and weighed a little more than four ounces; which approaches nearer to the 0·23 of sulphur which Lampadius said he found in molybdena than the proportion of Kirwan, which is 0·55. The sensible metallic taste of the calcined matter, its brilliant and crystalline aspect, and the experiments made by Ilsemann* and Heyer*, in which calcined molybdena exhibited the phænomena of an acid, made me suspect that this molybdena might be molybdic acid like that sublimated. In this case the nitric acid might have been spared; which would have rendered the process still shorter and more œconomical.

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* Crell's Chemische Annalen 1787, vol. i. p. 410.

To clear up my doubts in this respect I made the following preliminary experiments.

Experiment VII.

I reduced to fine powder some grains of calcined molybdena, poured over it a gros of water, and heated it above a lamp. I then instilled into it some drops of a solution of carbonate of soda: on each new instillation a strong effervescence took place, and a great part of the matter was dissolved. For the moment this result was sufficient to enable me to conclude from it the acid nature of the calcined molybdena. The solution was filtered, and decomposed by nitric acid. There was formed a precipitate of molybdic acid, which placed it beyond a doubt that there was formed a combination of this acid with soda.

Experiment VIII.

I pulverized some grains of calcined molybdena, and boiled them for some minutes with an ounce of water. The taste of the liquor, which was sensibly acid and metallic, as well as the red colour it communicated to turnsole paper, again proved the nature of the acid of calcined molybdena.

It gave me great satisfaction to have found so short and so œconomical a method of obtaining molybdic acid. I cannot help wondering, as the experiments of Scheele, Heyer, and Ilsemann, have been so long known, that chemists have not followed this method indicated to us by the nature of calcined molybdena, and to which they had so nearly approached. In order to ascertain more fully whether calcined molybdena is pure molybdic acid, and to determine the means of separating from it the heterogeneous substances with which it might be still combined or mixed, I made the following experiments.

Experiment IX.

As the molybdic acid has the property of combining easily with a portion of potash, and of retaining that base, if not entirely, at least in a great part, against the action of all acids, this salt could not be of any use for purifying that

acid. The alkaline earths, and earths which with the molybdic acid form salts difficult of solution, could not answer my purpose to separate the acid from the insoluble substances with which they are mixed. It remained to try soda and ammonia, which I found to be both equally proper under circumstances peculiar to each of them, for accomplishing the proposed end.

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* Crell's Chemische Annalen, vol. ii. p. 26 and 125.

Experiment X.

I boiled two gros of calcined molybdena with eight ounces of water, and instilled into the solution a ley of carbonate of soda until no more effervescence took place. An excess of alkali was added, and the matter was left to boil for half an hour: it was then filtered, and the concrete matter which remained on the filter was edulcorated. The different liquors were evaporated to two ounces, and nitric acid to excess was instilled into the warm liquid. During the instillation of the nitric acid a crystalline white powder was deposited, and after cooling there were formed in the liquor crystals of a certain size. A new quantity was obtained by evaporation. The different salts were washed and dried. The residuum on the filter, after being washed and dried, weighed thirty grains, and had the reddish colour of oxide of iron.

Experiment XI.

The preceding experiment was repeated, with this difference: the liquor, after the effervescence had ceased, was boiled for half an hour longer. The matter, after being reduced to two ounces, and filtered, was suffered to cool before the nitric acid was added. The addition of this acid caused to be precipitated a larger quantity of the white powder, which had a more crystalline aspect, and which after edulcoration and desiccation had the splendour of mother-of-pearl. As I did not add so much acid in this experiment as in the former, the liquor was less charged with molybdic acid. The residuum, when washed and dried, weighed also thirty grains.

As I suspected that this residuum might contain a portion of molybdic acid less soluble than the preceding, I tried to extract this acid by boiling the residuum with nitric acid. With this view I made the following experiment.

Experiment XII.

The thirty grains of the residuum of the two last experiments were boiled with an ounce of nitric acid of the specific gravity 1·250 and half an ounce of water, until the whole moisture was dissipated. During the evaporation I observed no disengagement of nitrous gas, from which I could conclude that there was in the residuum a little molybdena not yet acidified. I then boiled the residuum for half an hour with a ley of carbonate of soda, after which there was added to the solution nitric acid to decompose it; but I observed no precipitation of molybdic acid, though an excess of nitric acid had been added. The solution had only a metallic acid taste. On examining the residuum more closely, it was found to be composed of alumine, oxide of iron, and a few fragments of silex, which, if I except the oxide of iron contained in the sulphurated molybdena, arose in part from the molybdena and partly from the crucible.

Experiment XIII.

The white precipitates of experiments X. and XI. were put into an ignited crucible. By an increase of heat they first entered into fusion, and were then volatilized and condensed, partly under the pulverulent form and partly under the crystalline. By continuing the fusion the matter penetrated through the crucible. The fused matter was very liquid, and on cooling it formed itself into a crystalline radiant mass of a whitish gray colour having a slight metallic splendour. In other respects it exhibited the same phænomena as pure molybdic acid.

This last method furnishes a very short and œconomical method of obtaining molybdic acid. There can be no doubt that if the molybdena had been entirely free from matrix the calcined mass would have entirely dissolved, except an atom of iron, from which sulphuret of molybdena is never free.

I was desirous also to try the separation of the molybdic acid by the help of ammonia. With this view I made the following experiments.

Experiment XIV.

I digested for twelve hours, in a moderate heat, a gros of the calcined mineral in fine powder with half an ounce of concentrated liquid ammonia, shaking the matter from time to time; after which the liquor was filtered. It had still a strong odour of ammonia. The half of it was decomposed by nitric acid, which precipitated from it molybdic acid in abundance. The other half was evaporated in a small porcelain capsule, and then heated to a slight degree of incandescence. The ammonia was entirely dissipated, and left a grayish blue residuum, which entered into fusion in a stronger heat, and which, after cooling, exhibited a radiant whitish gray mass perfectly similar to fused pure molybdic acid. The residuum collected after the digestion of the calcined molybdena with the ammonia weighed twenty grains: I subjected it to the following experiment.

Experiment XV.

I boiled the residuum above mentioned with two gros of nitric acid to perfect evaporation, and digested the new residuum with two gros of liquid ammonia. The residuum was then filtered, washed, and dried. It weighed sixteen grains. The liquid was evaporated, and after incandescence I obtained four grains of molybdic acid. The residuum of this experiment, like those of Nos. X. XI. and XII. contained oxide of iron, alumine, and quartzy sand.

After these experiments I will not hesitate to prefer the method of separation by ammonia to that by soda, especially in operating with pure soda. For it may happen that the molybdena will enter into vitreous fusion with the foreign bodies contained in its ore, and then ammonia would not be capable of separating the molybdic acid from that combination. However, this inconvenience may be prevented by not making the fire too strong towards the end of the calcination. When the method by ammonia is properly considered, it will be readily perceived that it would be impossible to devise one more expeditious and less expensive.

The molybdena may be deprived of its sulphur without the aid of the nitric acid or of nitrate of potash. The molybdic acid formed is purified from the oxide of iron with which it is naturally united, and from the quartz and argil with which they are accidentally mixed, by digesting it with ammonia; which can be done only with difficulty by means of nitric acid. Molybdate of ammonia is decomposed by a heat of incandescence, which separates from it the alkali in the state of purity and without being decomposed. It may therefore be collected without great loss in a distilling apparatus, and the acid, almost without any decrease, will be found in the retort. So many advantages united are certainly not to be found in any other method, not even in that of separation by soda.

By following these different indications we are led to the following process, which is the most advantageous for preparing molybdic acid.

New Method of preparing pure Molybdic Acid.

Take the purest sulphuret of molybdena that can be found, and reduce it to powder; introduce it into an inclined retort, and torrefy it, to separate the sulphur, and to oxygenate the metal, in a heat which must be immediately raised to a very high degree. When the molybdena has lost its brilliancy, and its black colour has changed to gray, lessen the heat, and continue to expel the last portions of the sulphur, and to complete the oxygenation of the metal at a more moderate heat, continually stirring the matter with an iron spatula. By these means the matter will be prevented from entering into fusion with the oxide of iron and a small quantity of alumine with which they are mixed, and from forming a mass which the ammonia can afterwards decompose only with difficulty, and nothing will remain adhering to the crucible. The mineral is then to be reduced to a very fine powder, and it must be digested several times with a sufficient quantity of liquid ammonia to extract the whole molybdic acid. The different liquids are then to be united, and being poured into a glass retort are distilled to dryness. Molybdate of ammonia will remain in the retort. The heat is increased to incandescence, in order to expel the ammonia. If the retort be too large, the matter must be removed into a smaller.

This process would not answer where it is required to analyse ore of molybdena. In this case it would be necessary to oxygenate the sulphur and the metal by the help of nitric acid. This method will contribute in all probability to ameliorate and render easier the analysis of sulphuret of molybdena.

Addition to the preceding Experiments.

I had undertaken four times, and with the same success, the separation of the molybdic acid by the help of ammonia in the proportions of experiment XIV, when in treating in this manner a pretty large quantity of calcined molybdena I observed a surprising phænomenon which hitherto I had not remarked. I separated the acid from two ounces of molybdena by the help of ammonia. The filtered liquor was left at rest for some days, to see whether any thing would be deposited; which actually was the case. Some flakes which exhibited the same phænomena as oxide of iron were deposited. I again placed the liquor on one side, to see whether a new separation would take place. I had, however, evaporated and decomposed a quantity of the solution of molybdate of ammonia equal to a sixteenth part of the whole liquid. The residuum exhibited the same phænomena exactly as that obtained in experiment XIV, and was pure molybdic acid. Numerous occupations prevented me from undertaking new experiments with the solution of molybdate of ammonia until six weeks after. Having evaporated the solution to dryness, I put the saline mass into a new Hessian crucible in order to expel the ammonia by incandescence. To my great astonishment I observed that the mass deprived of ammonia, which under other circumstances fuses so readily, and which, after being some time in fusion, penetrates into the substance of the crucible, could not this time be brought into fusion even in the strongest white heat, and was only softened, emitting vapours of molybdic acid. On examining the cooled mass, I found at its surface several irregular depressions. It had an argentine splendour inclining to gray. On its fracture it exhibited a cupreous blue colour of steel with a metallic tint; its texture was scaly and very compact; it was difficult to be broken and pulverized. Its specific gravity was 5050. By pouring over it moderately concentrated sulphuric acid the mass acquired a considerable heat, emitting nitrous gas in abundance, and transforming itself into white oxide.

After all these phænomena, it can be considered only as metallic molybdena, or oxide of molybdena very near to the metallic state. I can explain this in no other manner than by admitting, that in consequence of the long contact of the ammonia with the molybdic acid the latter was decomposed. I dare not, however, assert that this was actually the case, and that some other cause which I did not observe may not have concurred to produce the same effect. However this may be, it is remarkable that under the circumstances above mentioned the molybdic acid, the reduction of which is so difficult, should be reduced so easily and by so weak a fire; and that the metallic molybdena, which is so little fusible, should be fused, or at least softened into a homogeneous mass of a scaly texture. The want of molybdena has hitherto prevented me from making the experiments necessary to ascertain the cause of this singular effect.

XXXIII. *Of the general Relation between the Specific Gravities and the Strengths and Values of Spirituous Liquors, and the Circumstances by which the former are influenced.*

[Continued from p. 33.]

Of the Standard of Proof.

§ 14. THE standard of comparison which, in conformity to the principles already spoken of in § 3 has been established in this country, is called *proof spirit;* and it appears that some such standard was in use long before we were in the possession of any means of correctly appreciating its strength. This, indeed, has never yet been done; and it is from the want of correctly defining the strength of this standard that a great part of the uncertainty at present existing, with regard to the relative strengths and values of spirituous liquors in general, has originated.

§ 15. When a spirituous compound of any kind contains about a certain quantity of alcohol (which quantity, however, varies considerably in different liquors, and even in liquors of the same description, but differing in their purity), it will afford, on shaking it in a phial, a crown of small bubbles of a peculiar appearance, which gradually go off, or subside without breaking; and which has generally been called by the French the *chapelet*, and by us the *proof*. Liquors of any kind which were capable of exhibiting this phænomenon have, for upwards of two centuries past, been denominated of proof strength.

Upon the average, perhaps, an experienced person may by this means guess within about 6 or 8 per cent. of the real strength of such liquors as are capable of being examined in this way, if in a tolerable state of purity.

The following curious account of the processes for proving spirituous liquors, which is extracted from Mr. Highmore's Treatise on the Excise Laws, published in 1796, vol. ii. p. 283, may, perhaps, serve to give a better idea of the degree of reliance which ought to be placed on them, and of the necessity which there is for a total revision of this important subject:

"Proof spirits, or common saleable goods, are spirits of any kind of a determinate strength, being the same with those of good brandy, and the malt and sugar spirits of the distillery, as they are usually sold, containing equal quantities of rectified spirit and water."

"The best proof spirit is that distilled from French wine;

but for common use may be employed the spirit drawn from melasses."

"The common method of judging of the due strength is by striking the bottom of the sample phial, filled half way with common malt spirit, with the palm of the hand; the bubbles raised on the surface will go off again in a strong manner, without breaking or swelling; and this is the method constantly used by the traders: it is said to be fallacious, and easily open to deception; for if a little treacle, syrup, &c. be added to a quantity of highly rectified spirit of wine, it will give a brandy proof to that spirit. The true strength may, however, always be known by carefully burning away a measured quantity of brandy, &c.; since if it leaves one half water, it is right; if more or less, it is too strong or too weak."

"Perfect proof is that crown of bubbles before mentioned, of a certain size, arising as a head upon a small quantity of a well qualified spirit shaken in a slender phial."

"Proof more than perfect is that in which the bubbles raised by shaking the spirit are larger than those on the common or perfect proof, and go off more suddenly; that is, according as the spirit is higher, or approaches more to the nature of rectified spirit, or, as it is usually called, spirit of wine."

"Proof less than perfect is that wherein the bubbles are smaller, and go off quicker and fainter than in perfect proof; the spirit, in this case, being mixed with more than its own quantity of phlegm, or being too poor for sale."

"The most exact of all methods of determining the strength of any spirit is by distillation, rectifying it up to an alcohol, or totally inflammable spirit; but this, though liable to no error, is too tedious to come into common use. And, upon the whole, the best method of all others seems to be that of *deflagration;* namely, by setting it on fire: if, after it will no longer burn, the remainder is half as much as the quantity measured out for the trial was, then the spirit tried is found to consist of half water, and half totally inflammable spirit; that is, exactly *perfect proof:* and according as the remainder is more or less than half the original quantity, it is so much below or so much above proof, or the due strength of brandy."

"In commerce, with regard to spirits, it would certainly be a much better method to abolish such uncertain proofs, and to make all the goods of the strength of what is called spirits of wine; that is, a totally inflammable spirit, whose purity is much greater, whose strength may always be found with exactness, and whose bulk, stowage, carriage, and incumbrance, would be only half in regard to that of brandy, or proof spirit; and it might at all times, as occasion called for it, be mixed into a great variety of extemporaneous liquors, and the exact degree of strength would be always precisely known."

"This operation, indeed, in the common way, proves so tedious and expensive, and, after all, so short of expectation, and so generally unsatisfactory, that it is not to be expected that the common distillers, till they have fallen into a better manner of working, should come into the proposal. But if, instead of the common way of rectifying by the hot still, they would try the using a large *balneum Mariæ*, made of a large rectangular boiler, and a set of tall conical vessels, they will find that little fire and little attendance, and consequently very little expense, will, in this manner, furnish them with spirits reduced at once to this standard, and greatly superior, in all respects, to the common ones of the same strength. In this case there would be no need of any addition of salts; but the distiller may work more perfectly and more expeditiously without them, and thus preserve the fine essential vinosity of the spirit, which in the common way of working they constantly lose."

"The advantage of this method would be yet greater to apothecaries, to the makers of compound cordial waters, who want only a pure spirit of such a strength, and suffer greatly in the fineness and perfection of their commodities by the spirit they are obliged to use having in it a fulsome and nauseous oil of its own, which will always mix itself with their compositions, and the oils of the aromatics, &c. which they add to it. If spirits were brought to this standard for the market, there would be no possibility of deceit; and no further examination need be made of it by the buyer than its burning perfectly dry in a spoon."

So much for the general state of science with respect to matters of this nature.

§ 16. The only trace which we are able to find of any attempt to define by parliamentary authority what any denomination of strength of a spirituous liquor really signifies, is by a loose passage in the act 2 Geo. III. c. 5, which is as follows: "And for the purposes of this act it is hereby enacted, by the authority aforesaid, that each gallon of brandy or spirits, of the strength of one to six under hydrometer-proof, shall be taken and reckoned at seven pounds and thirteen ounces the gallon."

The reader will in a moment perceive the striking inaccuracy of the above definition. The weight of the averdupois pound, and the length of the inch and consequent solid content of the wine-gallon, neither of which was at the time of making the statute appreciated with sufficient accuracy, we shall for the present take to be so, and assume that the former is equal to 7000 grains troy; and that the general relation between the measures and weights of this country is such, that the cubic inch of distilled water, at 60° of Fahrenheit's thermometer, would weigh, in air at the same temperature when the barometer stands at 29½, 252½ of the same grains. [*Vide* vol. xv. p. 278.] There still remain, however, two very important questions which require to be answered before we can ascertain the real strength of proof spirit.

1stly, What is meant by *one to six* under proof? Is it intended to signify that compound which would be produced by the addition of one part of water to six of proof spirit by measure—or that of which a given quantity by measure shall contain and consequently be equal in value to six-sevenths of *that measure* of proof spirit? These are by no means convertible terms, as will be easily perceived by considering what is before mentioned in § 10. We must take one supposition or the other; and we shall, for the reasons which are stated hereafter in § 24, adopt the latter.

2dly, What is the temperature at which it is intended that this spirit should weigh seven pounds thirteen ounces? A compound of this strength would vary in its weight upwards of two ounces per gallon in different temperatures in this climate, and still more in some other countries; and every liquor of which a gallon at any possible temperature would be of that weight, would answer equally to the description of the statute. A compound apparently 12 per cent. under proof at 35°, would, if judged of by this criterion, appear to be over proof at 70°; and vice versâ.

§ 17. The omission to fix the temperature, therefore, at which the compound mentioned in the statute is to weigh seven pounds thirteen ounces per gallon, coupled with the circuity of describing a mixture differing from proof strength rather than proof spirit itself, and from which the strength of the latter can only be determined by a calculation in which different data may be assumed, are the leading causes of all the uncertainty relative to this subject. Something must be guessed at with respect to each; and the following is the mode in which the authors of this work have hitherto proceeded in the appreciation of the strength of proof spirit.

Assuming that the avoirdupois pound and the content of the wine-gallon of 231 cubic inches bear such a relation to each other as is already described in the last section, the specific gravity of a liquor weighing seven pounds thirteen ounces avoirdupois, or 54687·5 grains per wine-gallon, will be 937·59. Now sir Charles Blagden tells us, that, although no temperature is mentioned in the statute, yet the understanding of the trade is, that it means that the diluted spirit thus spoken of should be considered as being at 55°; whilst others contend that the temperature should be rated higher, for the following reason. It seems that, at the time of making the fundamental experiment on which the statute was founded, the spirit which is there described was produced by lowering either alcohol, or some other kind of rectified spirit, to the strength there spoken of with water, and immediately weighing it; under which circumstances it was then found to weigh 7 pounds 13 ounces per gallon, and to be one to six under proof. Now, if this was really the case, the mixture could not probably be lower than 60°. We will however suppose that the truth lies somewhere between them, and that the temperature was in fact 57½°. Now we find, from Mr. Gilpin's tables, that a spirit which is of the specific gravity of 937·59 at 57½° is similar to one composed of 100 parts of water by weight, and 86¾ of his alcohol of the specific gravity of 825. at 60°, and of which it contains a quantity which would be by measure, when at 60°, equal to ·5278 of the bulk of the compound at 57½°. But proof spirit is, by the directions of the act, according to the construction of it which we adopt, to contain 7-6ths of this proportion, or ·6157 of its quantity of such alcohol by measure when at 60°; that is, it will be equal to a compound which will be found by the same tables to have exactly the specific gravity of 920 at 60°. This appreciation, therefore, of proof spirit has been adopted by the authors of this tract in all the instruments which they have made for many years, and appears now to have become that of the trade in general. The wine gallon at 60° of spirit of this strength weighs 7 pounds 10 ounces and 10·47 drachms avoirdupois; and the same measure at 55°, 7 pounds 10 ounces and 15·59 drachms.

The commissioners of the customs have, however, (according to the statement of Mr. Ramsden, in his "Account of Experiments to determine the Specific Gravities of Fluids, thereby to obtain the Strength of Spirituous Liquors," printed in 1792,) been in the habit of considering proof spirit as weighing 7 pounds 12 ounces per gallon, at 55°, which would, according to the foregoing estimation of the relation between this weight and measure, give no less than

928 for its specific gravity at 60°. In fact, the real standard of proof spirit has never yet been tolerably defined, and it is therefore nugatory to talk about it. The language of the legislature, *hydrometer proof*, perhaps, conveys a more accurate idea of it than any other; for it has hitherto been just what the hydrometer-maker chose to consider it.

§ 18. The real strength of proof spirit, which was already so indefinitely described by the act of 2 Geo. III. c. 5. was, however, rendered still more doubtful by a clause which was inserted into a subsequent act (27 Geo. III. c. xxxi. § 17) enacting and declaring, "That, until the fifth day of April one thousand seven hundred and eighty-eight, all spirits shall be deemed and taken to be of the degree of strength at which the said hydrometers called Clarke's hydrometers shall, upon trial by any officer or officers of excise, denote any such spirits to be." The meditated investigation of this business, however, did not take place; and this clause was, after being once or twice continued in contemplation of such a measure, at length made perpetual by 41 Geo. III. c. xcvii. § 8.

Now, if the instrument mentioned in the act had agreed with itself, it seems as if the proof as denoted by this hydrometer would have become the legal standard; the subsequent act being, of course, paramount to the antecedent one with respect to those points in which they might be found to clash with each other. Unfortunately, however, it had long been, in all respects, the worst instrument which had been in use in the spirit trade for these purposes, its indications differing every where with each other. Mr. Clarke's one to ten, for example, (by which is meant 10 per cent. over proof,) is only between 8 and 9 per cent. over his own proof, and some points actually differ in like manner to the enormous extent of 7 or 8 per cent. from the truth; and, to complete the confusion, there were always two of those instruments made, one for import and the other for export spirit, each of which differed in every graduation from the other. How this came to be the case we are not sufficiently informed: we are told, however, by the pamphlet delivered with these instruments, that "the export hydrometer is used in trying British spirits in the different divisions of distillery, and by distillers in general. The import is used at the port of London and all the out-ports in trying imported foreign spirits, and by brandy merchants in general; and is a small matter lighter than the export, and in favour of the importer. This notice is thought necessary, as some gentlemen have tried instruments with one of a different kind, not knowing there was any difference in Clarke's hydrometer!!!"

§ 19. Vague and indefinite as the term *proof* has hitherto been in England, it has always been still more so in Ireland, where they have, for about forty years past, used an hydrometer for the purposes of the revenue which has no correction whatever for temperature. The Irish proof, therefore, is altogether incapable of being defined with any tolerable accuracy. It appears, however, to be upon the average somewhere about 9 per cent. over proof by our standard, or to have a specific gravity of about 909 at 60°. It was probably established at first of that strength, as being the highest which a foreign spirituous liquor could possess, without paying the augmentation duty on its subsequent importation into this country.

§ 20. It appears, from what has been before said, that the first step towards the permanent arrangement of this important business must necessarily be to fix and ascertain the real strength of proof spirit by an act of parliament, which shall define its specific gravity at a given temperature. If it were enacted, for example, "that every spirituous liquor whose specific gravity, when free from adulteration, should, at the temperature of 60° of Fahrenheit's thermometer, be to that of distilled water at the same temperature as 920 to 1000 (or in any other ratio which might be thought proper), should be deemed and taken to be proof spirit; and that every spirituous liquor of different strength should be estimated by the quantity of such proof spirit at the said temperature of 60°, which, according to the tables contained in the schedule to such act annexed, (annexing a set of tables for that purpose, calculated from those of Mr. Gilpin, in the manner hereafter mentioned in § 32, &c.) would produce or be producible from every such other spirit by the addition of water to the stronger of the two till they were reduced to the same degree of strength," all the present uncertainty respecting this matter would instantly vanish. The hydrometer would still continue to be the most convenient instrument; and its accuracy would be ensured by the facility with which its errors would be detected, as any man who could use a pair of scales would then be able to examine the truth of its indications.

[To be continued.]

XXXIV. *Of the Variations of the Temperature of the Summer and Winter Seasons that take place in different Years. By* RICHARD KIRWAN, *Esq. LL.D. F.R.S. and P.R.I.A.*[*]

To reason with precision on this subject, we must at first abstract from all sublunary physical causes, and indicate the temperature appropriated to different latitudes from mere astronomical considerations.

Halley has ingeniously resolved this problem so far as the mere ratios of heat in the different seasons are concerned. (Phil. Trans. Abr. ii. p. 165; and Lambert in his Pyrometrie, § 596.

Halley, calculating the ratios of heat communicated by the sun to the earth (which he considers merely as a planet, abstracting from all distinction of land and water,) in the different seasons in the northern hemisphere, reduces these seasons to three, the equinoxes, the summer solstice, and the winter solstice; and attending only to the sines of incidence of the sun's rays, and the duration of their action, he sets the heat communicated at latitude 0, on the days of the vernal and autumnal equinoxes, at 20,000; and on the tropical days in the same latitude at 18,341: and then adds the ratios which the heat in every 10th degree of north latitude bears to these at the same periods. Lambert adds the ratios of lat. 49° and 66° 33′, stating the equatorial heat on the equinoxial day at 999.

But to express these ratios in thermometrical measures we must endeavour to find the greatest heat of the equinoxial day, taking a mean of the heat of the morning at two o'clock, and the evening under the equator, or very near it; and this I find to be 88° or 89° of Fahr. (see Ulloa, Mem. Philosoph. p. 61,) in the northern hemisphere, on the 20th of March, on the ocean; to which, indeed, we must confine ourselves in this inquiry, and particularly the Atlantic, for no uniformity can be expected on land.

It is uncertain what thermometer of Reaumur Ulloa employed, whether the true or the false; and hence I place the heat at 88° of Fahr.

This correspondence being found, the thermometrical degrees corresponding with all the other ratios are easily found by the rule of proportion, and the degrees thus found

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[*] From his paper entitled "Of the Variations of the Atmosphere. 1801."

I call the *mathematical temperature*. But in most cases this temperature is far from agreeing with the temperature really observed, and which I therefore call the *real temperature*; this I take at a *mean*, and not at its *maximum*, which I could not always discover, and is more fugitive and contingent. These temperatures I exhibit in two separate tables, the first indicating those of the vernal equinox and of the northern tropic or midsummer, and the second those of the autumnal equinox and the southern tropic or midwinter, over the Atlantic or standard ocean in our hemisphere.

Table the First.

Vernal Equinox.			Midsummer.	
Latitude.	Mathem.	Real Mean.	Mathem.	Real Mean.
0	88°	84°	80·7°	83°
10	86·5	82	89·3	84·3
20	82·69	77	95·64	80·5
30	76·21	69·5	99·66	73·5
40	67·4	60	101·41	70·5
49	61	51	101·7	62 Lambert
50	56·5	50·5	101·86	61
60	44	40	100·2	56
66° 33′	39·81	34	101·21	55 Lambert
70	30·99	32	101·41	54
80	15·28	27	108·56	51
90	9·6 Lambert	....	109·93	....

In this table we see, 1. That during the vernal equinox the heat differs but little from the mean heat really observed in all latitudes, and perhaps still less from the maximum of real heat; yet, except in latitude 80°, it is always higher, both from the quantity of rays lost in passing through the air, and from the quantity reflected by water and the frequent interposition of clouds, &c.

2. We see that the astronomical heat constantly increases with the height of the latitudes, as the duration of the solar rays more than compensates for their obliquity when the sun is in the northern tropic; but the real heat decreases as the latitudes increase, because this theoretic compensation does not take place from the interposition of clouds and the access of cooler winds, and the increased reflection from the surface of the water.

The different temperatures of different summers are ultimately resolvable into the different direction of the winds during those seasons and the different electrical states of the atmosphere, the south or south-east producing not only clouds which intercept the sun's rays, but also copious rains or hail, which, descending from great heights and occasioning a copious evaporation, cool the air to a great degree. The north and north-east, on the contrary, unless immediately succeeding great rains (for then they increase the evaporation), disperse the clouds, and, proceeding from countries then somewhat heated, allow the sun's rays their natural calefactive effect. But why winds from opposite points should prevail in different years cannot be known until the contemporaneous states of the atmosphere between the northern tropic and the equator are known. It is possible that frequent hurricanes and tornadoes, during which a quantity of air may be destroyed and converted into water, may demand an annual supply from the north, and thus occasion our north and east winds; and the absence of these phænomena may occasion an influx from the south, if the north and east are summoned to a different quarter, by similar causes.

Table the Second.

	Autumnal Equinox.		Midwinter.	
Latitude.	Mathem.	Real Mean	Mathem.	Real Mean.
0°	88°	84°	80·7°	83°
10	86·5	84·6	69·66	78·5
20	82·69	80	58	72·5
30	76·21	73·5	44·54	64·5
40	67·41	70·5	30·55	54
49	61	59	18·23	45 Lambert
50	56·56	58·5	16·71	44
60	44	48	4·73	34
66·33	39·81	42	1·32	30 Lambert
70	30·99	39	0	27
80	15·28	33·5	……..	22
90	9·6	……..	……..	…………

On this table we may remark, 1. That though the mathematical temperature of the autumnal equinox be exactly the same as that of the vernal, yet the real is much higher, as the northern hemisphere being cooled during the winter is slowly heated, and being heated during the summer is slowly cooled.

2. That in consequence of this circumstance the real temperature of the autumnal equinox approaches much nearer to the astronomical than does that of the vernal, until we arrive at latitude 70° and the higher latitudes.

3. In all latitudes above the equator a cold approaching to the astronomical is scarce ever felt at sea in winter: to what can this be attributed but to the equatorial effluence? For other causes, viz. evaporation and the frequent intervention of clouds, or at least haze, intercept the sun's rays, and consequently should cool the air even below the astronomical ratio which supposes the incidence of all the rays: in latitudes above 20° the difference is enormous.

4. At the distance of some hundred miles from the coasts of the Atlantic, in latitudes above 40°, the cold is much more moderate than the mathematical ratios indicate in most years; owing to the above-mentioned cause, and to the reign of westerly and southerly winds, which convey their heat to a considerable distance before they are cooled.

But in latitudes between 55° and 36° a degree of cold far superior to the astronomical has often been observed, and particularly of late, and in countries not very distant, or even bordering on the Atlantic. These extraordinary seasons may be attributed partly to the absence of the superior effluence, or its refrigeration in communicating with air replete with vapours, and partly to the prevalence of east-north-east winds which proceed from the interior and coldest parts of our continent; and hence the cold of the year 1776, so well described by Van Swinden, seems rather to have followed the order of the longitudes than of the latitudes. Wargentin, secretary to the Royal Academy of Stockholm, informs him that the cold observed that winter in Stockholm was not at all extraordinary, and expresses his surprise that it should have been so rigorous in Germany, France, and England. (Mem. Paris, 1776, p. 129.) Nay, it appears that the north-east wind which raged so furiously in Holland and at Montmorenci, latitude 49°, on the 27th (see Van Swinden, p. 40), had not been at Petersburgh on the 18th, nor any day after; for a perfect calm reigned on that day, and the high winds of the remainder of the month were from the north-west. (Act. Petropol. p. 382.) It is therefore probable that this wind proceeded obliquely from the eastern and southern parts of Russia, and may have been derived from latitude 55° and longitude 40°, and originated on the Atlantic, latitude 40°; and hence the utmost rigour of this cold was sooner perceived in the south of France, as Thoulouse, Marseilles, &c. than in the more northern latitudes, as may be seen in Van Swinden's general table, for it reached these towns on the 18th or 19th of January. In the more northern latitudes it was felt only on the 27th; it is true its date at St. Jean de Luz, latitude 43°, is January 28th in the general table; but this is a mistake, as may be seen p. 181, for January 19th is there said to be the true date, and, p. 179, it is said that the 18th or 19th of January are the days on which the greatest cold was observed in all places south of the Garonne; which fully confirms my former statement, that the wind which produced this cold originated in the south-west, and thence was gradually propagated northwards and eastwards. All the minuter modifications of this cold, in places not very distant from each other, may be ascribed either to recent falls of snow, the proximity to which must affect more or less the thermometers, the greater or lesser abundance of vapours in the atmosphere, and other circumstances too tedious and minute for insertion in this general view.

Snow falling from some height in the atmosphere is generally for some time surrounded with an atmosphere much colder than the air some feet above it, as Mr. Wilson observed, though it did not occur to him that the cold was communicated to the air by the snow; for he thought it highly remarkable that a thermometer hung 24 feet above the snow was four degrees less cold than one suspended 2½ feet above it. (Phil. Trans. 1780, p. 462.) Yet Mr. Boyle has long since noticed a similar fact (Boyle Abridg. i. p. 629), as related to him by some navigators; and Foster expressly mentions, that being to the leeward of an icy mountain, probably many feet distant, the thermometer sunk four degrees, and rose to its former height when he had passed that mountain (Observat. p. 73); but when there is not a recent fall of snow, the air several feet above the surface of the earth is generally colder (when no great evaporation takes place) than that nearer to its surface. Thus during the intense cold of January 1776, there having been no fall of snow since the 24th, Van Swinden found the degree of cold on the morning of the 27th to be 8° 25′, while Camper, in the same street, whose thermometer was some feet nearer to the earth, found it only 6° 5′. (See Van Swinden, sur le Froid de l'Année 1776, p. 24, 25, 28, and 176.)

XXXV. *Ac-*

XXXV. *Account and Description of a Stone which fell from the Clouds in the Commune of Sales, near Ville-Franche, in the Department of the Rhone. From a Memoir of M.* De Dree, *read in the National Institute April* 11, 1803*.

We have more than once entertained our readers with phænomena analogous to that which is to be the subject of the present memoir; and since the attention of philosophers has been directed to these singular facts, one might say that they have been more frequent. We cannot, and indeed ought not, to explain them until their existence has been fully proved, and until all the circumstances by which they are characterized are well known. The account which we are about to transcribe is extracted from a pretty long memoir on these phænomena read some time ago before the National Institute, by M. de Dree, brother-in-law of Dolomieu, a very enlightened amateur of mineralogy, and possessor of one of the richest collections of this kind in France. Among the facts which he collected, the one about to be mentioned has a striking character of authenticity; and it acquires a particular degree of interest from a comparison which proved to us that we observed the same meteor, without doubting, any more than others who saw it either at Geneva or in the western part of Swisserland as far as Berne, that it was one of those stones the origin of which it is so difficult to determine.

" In the month of February 1802 I was at Lyons with Dr. Pétetin, president of the Medical Society of that city, member of several learned societies, and author of a work entitled *Nouveau Mécanisme de l'Electricité:* we were conversing on scientific subjects, when he asked me whether I had heard any thing of a meteor which appeared at Lyons some years ago; and on my answering in the negative, he related the fact, of which he was a witness, in nearly the following manner:

" About four years ago (said he) during the evening twilight in the month of March, the weather being serene, and not at all cloudy, there passed over Lyons, nearly in a direction from east to west, a luminous ball, which, as it attracted attention by the strong light it emitted in its passage, was almost generally observed. He added, that he learned a few days after that this luminous globe had been seen by some travellers on Mount-Cenis; and he was informed at the same time that it had fallen in the environs of Ville-Franche under the form of an incandescent stone, a small fragment of which was sent to him.

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* From *Bibliothèque Britannique*, vol. xxii. no. 4, April 1803.

"He assured me also, that a comparison he then made of the periods at which the meteor had been observed on Mount-Cenis, at Lyons, and at Ville-Franche, positively announced that it was the same ball which had traversed that line and shown itself in these three points.

"I expressed to Dr. Pétetin a desire of seeing the fragment of this stone which he had in his possession; and the doctor, judging, no doubt, from the anxiety I showed to obtain information respecting this phænomenon, how much I was interested in it, was so kind as to offer me this fragment in case he should find it.

"I was the more desirous, indeed, to see the specimen of this mineral mass, as I had it in my power to compare it with analogous specimens, one of which fell near Wold Cottage, in Yorkshire, on the 13th of December 1795, and another near Benares, in Bengal, on the 19th of September 1798, a fragment which I brought with me a few years ago from London, where I received it from count de Bournon, F. R. S., a very celebrated mineralogist.

"Some time after Dr. Pétetin sent me the fragment in question, and I was much surprised to find that it had a perfect similarity to specimens of those which fell at Benares and at Wold Cottage; a similarity manifest not only in regard to the genus of stones but to the mineralogical species which enter into their composition, and also in regard to the effects resulting from their movement in the atmospheric fluid.

"As the details which Dr. Pétetin was so kind as to transmit to me along with this valuable present gave me reason to hope that I should be able to discover the exact spot where this globe fell, and the circumstances attending its fall, I made researches in the neighbourhood of Ville-Franche, which were not fruitless; for by successively acquiring more satisfactory information I was at length directed towards the commune of Sales, at about the distance of a league and a half to the north-west of Ville-Franche, in the department of the Rhone, where I learned that most of the inhabitants had been witnesses of, and much frightened by, the arrival of this luminous body, which had fallen in a vineyard within three hundred paces of the village, and near the house of a vintager named Pierre Crepier.

"I addressed myself to two of the inhabitants best acquainted with the fact, and who appeared most capable of relating all the circumstancees of it with simplicity, and free from that air of the marvellous from which minds not directed by a knowledge of the principles of philosophy can scarcely be preserved. I proceeded with them towards the house of Crepier, and it was there on the spot where the stone buried itself in the earth that I received every information respecting it, and even obtained the last specimen of this stone which Crepier had remaining.

" The following are all the circumstances I collected in regard to this singular phænomenon, omitting the useless reasoning in regard to its authenticity:

" On the 12th of March 1798, about six in the evening, the weather being calm and serene, a luminous globe of an extraordinary appearance attracted towards the east the eyes of the inhabitants of the commune of Sales and of the neighbouring villages, as they were returning from their labour; and its rapid approach and horrid humming noise, like that produced by an irregular and hollow body traversing the atmosphere with rapidity, threw all the inhabitants of that commune into the greatest terror, especially when they saw it pass over their heads at a very little elevation. According to their report this ball left behind it a long train of light, and emitted, with an almost continual crackling noise, small blue sparks of fire similar to small stars.

" Its fall was then observed by three workmen who were not more than fifty paces from it, One of them, named Montillard, a young man who was nearest to it, was struck with terror, and dropped his coat and a billet of wood which he was carrying, in order that he might escape as fast as he could. The other two, named Chardon and Lapous, were no less frightened, and fled to Sales, where a general alarm prevailed. These three witnesses agree in stating that this body moved with astonishing rapidity, and that after its fall they still heard a kind of hissing noise proceeding from the place where it buried itself.

" In regard to Crepier, he was at home; where he was so much frightened with the hissing of the body in the air, and the noise of its fall, which took place within less than twenty paces of his habitation, that at first he shut himself up with his family in the cellar, and then in his bed-chamber; where fear prevailing over curiosity, he spent the night without daring to go out to examine what had happened.

" Next morning he was called out by Chardon and Lapous, who had carried with them M. Blondel, adjunct of the commune of Sales, and several other persons, and they all repaired together to the place where the luminous body had been seen to bury itself. There, at the bottom of a hollow, eighteen inches in depth, that is to say, of the whole thickness of the vegetable earth, they found a large, black, irregular, ovoid mass, according to their expression like a calf's head. It was entirely covered with a blackish crust; it was no longer warm, and had the smell of gunpowder. They observed also that it was split in several places, so that Chardon by thrusting his bill into one of the fissures made it fall to pieces. I was not able to learn properly whether this fissure was lined by a crust similar to that on the surface; they only thought they remembered that it was partly black. This mass, having been transported to Crepier's house, their first care was to examine the nature of so unexpected an object, and what it contained. The stone therefore was weighed, and immediately broken; but finding only a stone, from avarice, which did not fail to succeed their emotions of fear, they proceeded to a sentiment of indifference for this mass, while the phænomenon was imputed to the most whimsical and supernatural causes, according to the kind of impression which had been communicated to the spectators.

" The weight of this stone was about twenty pounds.

" The noise of this event was soon spread; and the commissioner of the executive power to the administration of Ville-Franche being informed of it, he sent to request the stone, with information respecting its fall. A fragment of it, weighing about seven pounds, was brought to him, a part of which, with an account of the phænomenon, he transmitted to one of the members of the conventional assembly. I do not know what attention was paid to it, and what effect it produced, at a time when every mind was absorbed in politics.

" M. Place, a merchant of Ville-Franche, who was at Sales at the same time I was there, assured me that he was a witness, as well as many inhabitants of Ville-Franche, to the passage of this luminous globe over the town; that he heard its humming noise; that its elevation could not exceed 500 toises; and that its direction was from east by south to west by north.

" I must add, that the simplicity of most of the reports made to me, their perfect agreement in all the important points, and the great number of persons who saw this phænomenon, which took place at that time of day most favourable for its being generally observed, leave no doubt with me in regard to the veracity of the account which I have

I have here given, and of the certainty of the fact in question.

"Some time ago, when conversing on the subject of this phænomenon with professor Pictet, he recollected that at the same period he and a number of the inhabitants of Geneva and the neighbouring towns, as far as Berne, had observed a luminous body which suddenly appeared in the southern regions, proceeding rapidly from east to west. This phænomenon at that time was considered to be a meteor; but he is so fully persuaded that this body is the same which fell at Sales, that he has given me permission to quote his testimony*.

"To these circumstances I shall join a description of the characters of this mineral substance, in regard to the point of view in which they ought to be considered by a mineralogist; and I shall give the result of the analysis of it made by Vauquelin, member of the Institute.

"Its colour in the inside is an ash-gray formed by a mixture of whitish parts interspersed with black metallic points.

"Its texture is gravelly, and the substances, of which I am about to speak, are disseminated in it, as octaëdral iron and sulphuret of iron are in the amphibolic steatites of Corsica, &c.

"Humidity does not communicate to it an argillaceous odour.

"The following are the substances found disseminated throughout the mass of this stone. The first is malleable iron in grains which readily rust, and which vary in size from an almost imperceptible point to a line in diameter, and even to the weight of twenty-four grains.

"These grains of iron are malleable, soluble, and susceptible of attraction by the magnet. They, however, differ from forged iron in having a whiter aspect and less ductility; in being a little oxidated, and containing nickel, as is found by analysis. Those placed in the centre of the mass are in the same state as those found at the surface.

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* I have a perfect recollection of the appearance of this meteor. Its light was exceedingly bright, and its motion so rapid, that it was seen only for a few seconds, during which it diffused throughout the whole town an alarming light, though it passed at the distance of more than twenty leagues to the south. Its direction, according to estimation, was precisely towards the quarter in which it fell. My learned colleague, professor Prevost, embraced this opportunity of presenting to the Physical Society established at Geneva a very interesting memoir on the *bolides* or fire-balls, in which he collected considerations respecting the supposed magnitude, distance, and velocity of these meteors, which will excite less surprise at present than they did at that period.—*Note of professor Pictet.*

" The second substance is lamellated pyrites (sulphuret of iron) of a white colour inclining a little to that of nickel: it is sometimes dispersed throughout the mass in grains, and sometimes it lines the fissures interspersed in the stone. This pyrites experiences only a very slight, partial, and transient effervescence in acids: it is not susceptible of attraction by the magnet; by the blowpipe it readily gives a frit interspersed with small globules like other pyrites. That in a thin stratum on the sides of the fissures has a gray and less brilliant colour. It contains less sulphur, and approaches near to malleable iron in grains.

" The third, which is very rare, is under the form of spherical or irregular globules of a dark gray colour, brittle, and having a smooth compact fracture. It produces no effervescence in acids; is not susceptible of attraction by the magnet; is refractory to the blowpipe, and assumes in it only a red and black tint. When scratched with a graver this substance assumes a metallic colour like trap.

" Besides the gray globules I found also other globular and irregular bodies of an olive-green ground inclining sometimes to yellow, the fracture of which has a fat shining aspect like the steatites of Briançon, and which are not very hard. As several of these bodies lose their distinguishing character, approaching more or less to the appearance of paste; and besides, as they are in very small quantity, I did not think it proper to include them among the number of the contained substances.

" The surface of this stone is a vitrified black crust, slightly puffed up, which strikes fire with steel; of a quarter of a line in thickness at most, and in the surface of which are observed a few grains of iron and some gray globules, which being more refractory than the rest of the paste have resisted the effects of the heat.

" It is to these grains and these globules, which in all probability have been detached from the mass by the fusion of the neighbouring parts or by the intensity of the heat, that we ought to ascribe those blue sparks which escaped from the mass during its course.

" It is also to be remarked, that in a fragment of this stone the vitrification has been effected in a part of an interior fissure lined with pyrites, with this difference, that this vitrification is much more puffed up.

" The interior of the stone even in the parts nearest to the vitrified crust does not seem to have undergone any change by the action of the fire, only its granulated tissue has that lax aspect observed in all stones which with this kind of aggregation have undergone a certain degree of heat; an aspect which may be better distinguished than described. In regard to the globular bodies, they cannot be the result of a kind of fusion, since they have none of the characters of glass; and being the most refractory of all those substances of which the stone is composed, they could not pass to the state of vitrification near pyrites, which, though the least alterable substance, has nevertheless retained its lamellated texture.

"The paste or earthy part even in small fragments experiences in nitric acid a partial solution without losing its cohesion, and being before susceptible of attraction by the magnet is not so afterwards; which announces that the iron only has been attacked by the acid. It however still retains some metallic grains, which are pyrites. Treated by the blowpipe it gives black frit, assumes the metallic aspect, and continues to be attractable.

"I shall not give the specific gravity of this stone: as it is not homogeneous, and as its gravity depends on the greater or less quantity of metallic particles, and especially on the iron found in it, no positive character can be deduced from this property.

"To conclude: I can assert that the earthy part of the interior of this mass, as well as each of the contained substances, is in its original state, and that the only changes which the fire has effected are the relaxation in the tissue which I have already mentioned; and perhaps the change of colour, in case it has a particular one. I have also reason to think that in certain respects, and particularly in regard to texture, it approaches some kinds of pot-stone or siliceous serpentine containing metals.

"I shall conclude this description by transcribing the letter in which Vauquelin communicated to me the result of the analysis he made of this stone:—'The stone which you gave me to analyse (says he) consists of

Silex - - - - -	46
Oxidated iron - - -	38
Magnesia - - - -	15
Nickel - - - - -	2
Lime - - - - -	2
	103

"'This stone is directly attacked by acids. By the effect of this action sulphurated hydrogen gas is constantly developed. The liquors resulting from the solution of this stone in acids have a more intense green colour than that which, as appears, ought to arise from the iron it contains.'

"On pulverizing this stone there were detached globules of iron, one of which weighed about 24 grains. Its colour is whiter than that of ordinary iron, and, though ductile, it is harder than bar iron. It contains sulphur and nickel. Whence it appears that the sulphurated hydrogen gas and nickel found in it arise from the iron it contains.

"The three parts which are in excess in the result of the analysis arise from the oxygen absorbed by the iron. It ought even to have acquired more of it, and consequently there must be a loss.

"The experiments of Mr. Howard on the same subject have been published in the *Annales de Chimie*, and you will see that my analysis has a great relation to his. You ought to place the more confidence in it, as I obtained the same results before I was made acquainted with those of Mr. Howard *.

XXXVI. *Account of a Fire-ball which fell in the Neighbourhood of Laigle: in a Letter to the French Minister of the Interior.* By C. Biot, *Member of the National Institute, dated July* 20, 1803.

I read yesterday to the Institute an account of the journey I lately undertook by your desire in consequence of the meteor of Laigle. It has been ordered to be printed. An extract from this account may be interesting to you who have contributed to place this astonishing phænomenon beyond a doubt, and perhaps to the Chief Consul, who amidst so many labours still finds means to devote some moments to the sciences.

When I set out from Paris on the 26th of June, I did not proceed directly to Laigle. Had the explosion been as sudden as was announced, it must have been heard at a great distance. It was therefore agreeable to the rules of criticism, first to collect scattered testimonies, and to suffer myself to be gradually conducted by them to the place where the meteor was said to have burst; for in regard to all the circumstances of the explosion the accounts could not but agree, in whatever part they might be collected.

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* In the next Number we shall give an account of the stones which fell in the neighbourhood of Enshisheim and of Agen, taken from the same source.

I went first to Alençon, fifteen leagues west-south-west of Laigle, and in going thither I learned that a globe of fire had been seen proceeding towards the north. The appearance of this globe had been followed by a violent explosion. This took place on the 26th of April 1802, at one in the afternoon. By the direction of this phænomenon, the day, and particularly the hour, I judged that this had been the commencement of the meteor of Laigle.

At Alençon nothing had been heard—in consequence, no doubt, of the usual noise which prevails in a large town; but if I received only vague reports, I acquired a very important certainty, by the mineralogical collections of the country, that nothing exists in the neighbourhood of Laigle which has any resemblance to the meteoric stones.

From Alençon I proceeded to Laigle, traversing the villages, conducted by the accounts given me by the inhabitants. All of them had heard the meteor on the day and at the hour mentioned. In this manner I reached Laigle, and proceeded to the house of our colleague Le Blond; and I was happy to find in him the intelligence of a philosopher and the kindness of a friend.

The meteor did not burst at Laigle, but at the distance of half a league from it. I saw the awful traces of this phænomenon; I traversed all the places where it had been heard; I collected and compared the accounts of the inhabitants: at last I found some of the stones themselves on the spot, and they exhibited to me physical characters which admit no doubt of the reality of their fall.

If we first consider the physical testimonies, no meteoric stones had been found in the hands of the inhabitants before the explosion of the 26th of April. The mineralogical collections, formed on the spot with the greatest care for several years, contained nothing of the kind.

The founderies, iron works, and mines, in the neighbourhood which I visited, exhibited nothing in their productions or in their scoriæ which had the least affinity to these substances. No traces of a volcano are found in the country.

All of a sudden, and only since the time of the meteor, these stones have been found on the ground and in the hands of the inhabitants, who are better acquainted with them than any other person.

These stones are found only in a certain extent, in ground foreign to the substances they contain, and in places where, on account of their size and their number, it is impossible they could have escaped notice.

The largest of these stones when broken still exhale a strong sulphureous odour from their interior parts. That of their surface has vanished, and the smallest exhale no sensible odour, so that the odour of the former seems also from its nature likely to be dissipated in the course of time.

These are so many physical proofs which attest that the meteoric stones of the neighbourhood of Laigle are foreign to the places where they have been found; that they were conveyed thither exactly at the time of the explosion, and by a cause which has modified the principles they contain.

If we now consult the moral testimonies, what do we find? Twenty hamlets, dispersed in an extent of more than two leagues square, almost all the inhabitants of which declare themselves to have been eye-witnesses of the meteor, attest that a dreadful shower of stones was projected from it. Among the number there are men, women, and children. They are simple and unlettered peasants, labourers possessed of strong natural sense and reason; respectable ecclesiastics, and young people who having been military men are free from the illusions of fear. All these persons, of professions, manners, and opinions so different, who had very little or no intercourse with each other, agree in attesting the same fact, which they had no interest to invent: they all refer it to the same day, the same hour, and the same moment, making use of the same comparisons; and this fact, so strongly and so generally attested, is only a consequence of the physical proofs previously collected; which is, that stones of a peculiar nature fell in that country immediately after the explosion of April 26.

Besides, traces which strongly attest the fall of these masses, never mentioned without terror, are still shown. The inhabitants say that they saw them descend along the roofs of the houses like hail, break the branches of the trees, and rebound after they fell on the pavement. They say they saw the earth smoke around the largest of them, and that they still burnt after they were in their hands. These accounts are given and the traces shown only in a certain extent. It is there only that meteoric stones are found on the ground. Not a fragment is found beyond that district, and there is not a single person who pretends to have seen any of them fall beyond it.

A third kind of proof results from certain physical peculiarities uniformly related by the inhabitants of the country, who are too little enlightened to have foreseen the consequences. I here allude to the successive changes observed in the hardness of the stones and in the odour exhaled;

changes which, according to the report of eye-witnesses, among whom we must reckon our colleague Le Blond, took place in the interval of a few days after the explosion of the meteor, and of which I myself observed very sensible traces on breaking fragments of different dimensions; and this new comparison of testimonies and facts serves only to show a new agreement between them.

All the physical and moral proofs which it has been possible to collect are therefore concentrated, and converge towards one point: and if we consider the manner in which I was led, by a comparison of the testimonies, to the place of the explosion; the number of particulars which I collected on the spot; their coincidence with those which I brought from the distance of ten leagues; the multitude of the witnesses; their moral character; the resemblance of their accounts, and perfect agreement from whatever part obtained, without its being possible to discover a single exception in that respect; it may be concluded, without the smallest doubt, that the fact to which these proofs refer actually took place, and that stones really fell in the neighbourhood of Laigle on the 26th of April 1802.

From the aggregate of the testimonies we have deduced the following description of the phænomenon:

On Tuesday, April 26, 1802, about one in the afternoon, the weather being serene, there was observed from Caen, Pont-Audemer, and the environs of Alençon, Falaise, and Verneuil, a fiery globe of a very brilliant splendour, which moved in the atmosphere with great rapidity.

Some moments after there was heard at Laigle, and in the environs of that city to the extent of more than thirty leagues in every direction, a violent explosion, which lasted five or six minutes.

At first there were three or four reports like those of a cannon, followed by a kind of discharge which resembled a firing of musketry; after which there was heard a dreadful rumbling like the beating of a drum. The air was calm and the sky serene, except a few clouds, such as are frequently observed.

This noise proceeded from a small cloud which had a rectangular form, the largest side being in a direction from east to west. It appeared motionless all the time that the phænomenon lasted. But the vapour of which it was composed was projected momentarily from the different sides by the effect of the successive explosions. This cloud was about half a league to the north-north-east of the town of Laigle: it was at a great elevation in the atmosphere, for the inhabitants of two hamlets a league distant from each other saw it at the same time above their heads. In the whole canton over which this cloud hovered, a hissing noise like that of a stone discharged from a sling was heard, and a multitude of mineral masses exactly similar to those distinguished by the name of *meteoric stones* were seen to fall at the same time.

The district in which the stones fell forms an elliptical extent of about two leagues and a half in length and nearly one in breadth, the greatest dimension being in a direction from south-east to north-west, forming a declination of about 22°. This direction which the meteor must have followed is exactly that of the magnetic meridian; which is a remarkable result.

The largest of these stones fell at the south-east extremity of the large axis of the ellipse; the middle-sized ones fell in the centre, and the smallest at the other extremity. It thereby appears that the largest fell first, as might naturally be supposed.

The largest of all those which fell weigh 17½ pounds. The smallest I saw weigh about two gros, which is the thousandth part of the former. The number that fell is certainly *above* two or three thousand.

In this account I have confined myself to a simple relation of facts; I have endeavoured to view them as any other person would have done, and I have employed every care to present them with exactness. I leave to the sagacity of philosophers the numerous consequences that may be deduced from them; and I shall consider myself happy if they find that I have succeeded in placing beyond a doubt the most astonishing phænomenon ever observed by man.

XXXVII. *Experiments to ascertain the Value of different Steeps in curing the Smut in Wheat, and promoting its Growth.* By Mr. B. Bevan.

To the Editor of the Philosophical Magazine.

Leighton, Bedfordshire,
July 19, 1803.

SIR,

I take the liberty of sending you a copy of a table of results in a set of experiments made principally with a view to ascertain the *value of different steeps* in curing the smut in wheat, and promoting its growth; with twelve samples of good wheat A, and twelve samples of very smutty B;

each sort steeped in twelve different solutions of substances most easily to be procured.

Solutions in which the Wheat was steeped 24 Hours.	Specific Gravity of Steep.	Bushels sown per Acre.	Number of smutty Ears in three Sheaves.		Bushels of good Wheat per Acre of Produce.		Cwt. of Straw per Acre.	
February 27, 1802.			A.	B.	A.	B.	A.	B.
1. Solu. of potash - - -	1·357	3·51	1	81	21·6	13·6	36·6	29·1
2. —— of muriate of potash	1·097	3·51	3	218	20·2	10·1	36·0	21·1
3. —— of nitrate of potash -	1·080	3·51	7	115	23·8	14·3	36·9	31·9
4. —— of soda - - - -	1·056	3·51	9	159	20·2	11·7	35·6	26·7
5. —— of muriate of soda -	1·089	3·51	0	290	24·0	14·5	41·5	33·3
6. —— of sulphate of soda -	1·047	3·51	12	241	21·6	12·3	38·5	27·8
7. —— of muriate of ammonia	1·026	3·51	1	150	19·8	17·6	35·4	30·2
8. —— of common soot - -	1·025	3·51	0	123	20·8	11·4	34·8	25·3
9. —— of lime saturated - -	1·003	3·51	0	2	21·9	12·4	38·7	25·9
10. —— of nitric acid - -	1·016	3·51	none	grew	- -	- -	- -	- -
11. —— of muriatic acid - -	1·011	3·51	0	136	20·7	16·1	35·7	34·1
12. —— of sulphuric acid -	1·050	3·51	0	0	20·4	17·8	35·4	37·1
13. Dry in its natural state -	- -	3·51	6	323	20·3	14·7	35·7	31·0
14. Washed in common water	- -	3·51	none sown	107	- -	18·3	- -	38·5

The wheat was sown in rows in Leighton field on a sandy soil mixed with little or no calcareous matter, and is but indifferent land for bearing wheat.

Neither

Neither of the samples that were steeped in solution of nitric acid came up, except one or two single corns; and which, whether by having more room, or receiving but a less degree of stimulus, grew extremely luxurious. I tried the same steeps with barley, and found the same effect from the nitric acid, as not a single one came up.

The very powerful effect of this solution will induce me to try it again in different degrees of strength; and should the result be important, I shall make it public.

I am, sir,
Your most humble servant,
B. Bevan.

XXXVIII. *New Method of preparing corrosive Sublimate (hyperoxidated Muriate of Mercury) in the humid Way.* *By* M. L. Von Schmidt Phiseldeck*.

It is well known how much apothecaries desirous of preparing their own medicines are indebted to Mr. Westrumb for having furnished them with a method of preparing corrosive sublimate without being exposed to the dangerous vapour it emits during the sublimation. For some time past I have employed myself, merely from scientific views, in preparing corrosive sublimate according to this method. But however much I may be sensible of the advantages of this process, I cannot help regretting the loss sustained in the nitric and muriatic acids, which in general cost so much trouble and expense before they can be obtained pure. I reflected a long time on the means of avoiding this loss, and at length discovered a process much more œconomical than that of the chemist Hameln. The question was, to dissolve the mercury in the cheapest concentrated acid (this acid, without doubt, was the sulphuric acid), and to present to the oxide of mercury the muriatic acid without having separated it from its alkaline base. I resolved then to prepare a solution of mercury in sulphuric acid, and to decompose the sulphate of mercury by muriate of soda. I then hoped that I could easily separate the two salts that were formed by crystallization, as the sulphate of soda for its solution took only eight parts of cold water, whereas corrosive sublimate takes 162; but I found that after the first crystallization the two salts mixed, and that no other means of separating them remained but by alcohol. I shall pass over in silence the operations, which were attended with more or less success in this point of view, and describe only the process which I definitively adopted.

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* From *Journal de Chimie et de Physique*, par J. B. Van Mons, Pluviose 15, an. 11.

I introduced into a tubulated retort two ounces of mercury and three ounces three gros of concentrated sulphuric acid; I then adapted to the retort a receiver, without luting it, and made a pretty strong fire. During the solution there was disengaged a very considerable quantity of sulphureous gas. When nothing remained in the retort but a white mass, I added a solution of 5½ ounces of marine salt in six ounces of water, and exposed the mixture to strong ebullition for half an hour. A complete solution took place. I filtered the liquid while still in a state of ebullition, put it into a retort, and distilled it to dryness. On the remaining mass I poured 16 ounces of alcohol, and caused it to digest for some hours. I then decanted the liquid from off the residuum, filtered it again warm, and put it once more into the retort after I had washed it, taking care not to spread any of it in the neck of the retort, and distilled it to dryness. I must here remark, that the distilled liquid, which at first had the colour of Malaga wine, assumed, after the solution was concentrated, the colour of water de Rabel; and the saline mass, after the complete evaporation of the alcohol, was exceedingly white. Lime water made no change in the colour of this liquid.

I poured over the mass in the retort 12 ounces of water; I boiled it to solution, and, having filtered the liquor, exposed it to crystallize. Very beautiful crystals in the form of elongated prisms were deposited. I then poured over the residuum eight ounces of new spirit of wine, and again obtained a considerable quantity of corrosive sublimate. The distilled liquid, after being rectified on half an ounce of potash, was perfectly pure.

By employing this method, corrosive sublimate, in my opinion, will cost one-half less than by Westrumb's process. Sulphuric acid costs only one-third of what the nitric acid does; and there is no comparison between the price of pure muriatic acid and that of marine salt. I therefore flatter myself that this method will meet with a favourable reception.

XXXIX. *Observations on the Plant called St. John's Wort. By* C. BAUNACH *.

ST. JOHN'S WORT is a very common plant, which grows in great abundance in the fields, in the woods, and in uncultivated places. Botanists have described its distinguishing characters under the name of *hypericum perforatum:* it is employed in medicine as an excellent vulnerary and balsamic remedy; but unfortunately its juice is of little utility, since it is unknown to the greater part of dyers, in whose art, however, it may be applied with success. St. John's wort is a resinous plant, the flowers and summits of which, filled with seeds, contain a juice soluble in water, in alcohol, and vinegar: it diffuses throughout the first two liquids a red colour like that of blood, and in the latter a most splendid and beautiful crimson: when combined with acids or metallic solutions it presents a beautiful yellow colour; which proves that it contains two colouring matters, one more soluble than the other, that is the red.

To dye cloth, wool, silk, and cotton, yellow, it is sufficient to immerse them in water properly impregnated with the juice of this plant and a certain quantity of mordant. The salt best for being used as a mordant with this colour is sulphate of alumine, combined with a proper proportion of potash (carbonate of potash), in which the stuffs are suffered to remain some time; for it is on the length of the time, the quantity of the mordant, and the heat employed, that the fixity of the colour, and the shades resulting from it, depend. When little mordant is used, the dye is of a yellow colour; by increasing the mordant it inclines to green; and by adding solution of tin in nitro-muriatic acid it assumes rose, cherry, snd crimson shades, all very beautiful. The alum, generally employed for all extractive dyes, does not succeed well in the process here alluded to: the addition of potash is essentially necessary, because it decomposes this salt, precipitates its earth, dissolves a considerable portion of it; and it is this alkaline salt with an earthy base which in this operation becomes the true mordant, especially as the colouring principle resides in à matter almost purely resinous.

The juice of St. John's wort united to the mordant here mentioned gives to paper a beautiful yellow colour; and as it produces the same effect on skins, leather-dressers may employ it with advantage for dyeing white sheep- and other skins.

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* From the *Annales de Chimie*, No. 137.

The plant in question contains a great deal of tannin. A solution of common glue in water, and several experiments made in this respect, have proved it to me in a convincing manner.

Having poured into the juice of St. John's wort a little solution of the sulphate of iron, there was formed a precipitate of a blackish brown colour which had the property of absorbing oxygen, of becoming at length insoluble in water, and of assuming the characters of a concrete resin.

St. John's wort contains no essential oil. Having subjected a certain quantity of this plant to distillation with water, the product of this liquid had a strong and agreeable odour, in which I could discover no trace of volatile oil.

The juice of St. John's wort does not dissolve in fat nor in volatile oils, but it combines very well with resins. For this purpose, having the juice from the plant, it was poured into flat dishes to be desiccated. This operation may be performed by placing the dishes in an oven some time after the bread has been taken out: it is then reduced to powder, in which state it may be united to turpentine. This solution is easily effected in a copper mortar which has been well heated. The resin saturated in this manner may be mixed with fat and volatile oils. If combined with olive oil it forms a medicine known in pharmacy under the name of *oil of hypericum*, which when prepared in this manner has decisive properties, and may be employed with success. When incorporated with linseed oil it produces a beautiful red varnish, which may be used with advantage for furniture.

It is certain that the juice of St. John's wort is a resino-extractive substance, in which the resin is considerably predominant. The phænomena of its solution in water, alcohol, and resin, and in particular its inflammability, sufficiently prove it. The last property is so great, that when exposed on a burning coal it burns like incense, and emits very little smoke. It has the property of absorbing the oxygen of the atmosphere; it is no way altered in the air; its taste is somewhat bitter, and weakly astringent.

XL. *Account of a new Kind of American Crocodile. By* E. Geoffroy*.

The captain-general Leclerc, being informed by some officers of his staff who had served in Egypt that the crocodile of Saint Domingo had a great resemblance to that of the Nile, thought it a matter of importance to furnish naturalists with the means of confirming this circumstance: he therefore was desirous of making a sacrifice to us of two crocodiles which had been presented to him. The arrival of the younger, which was brought over alive, was announced at the time in the public journals; but it died just when about to be landed at Havre. The second reached us in Nivose: it was much larger; it had been properly prepared at Saint Domingo, and served as an original for delineating the figure which accompanies this memoir. (See Plate V.)

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* From *Annales du Museum d'Histoire Naturelle*, No. 7.

The fact mentioned by the officers attached to general Leclerc's staff, was not yet known: on the contrary, naturalists were persuaded that America contained only one kind of crocodile *, the principal characters of which were an obtuse muzzle, a cavity in the upper jaw for receiving the fourth lower tooth, and the hind-feet half-webbed. They were therefore much surprised to see arrive from Saint Domingo a crocodile similar to those of the old continent, having, like them, the muzzle oblong, an indentation in the side of the upper jaw to afford a passage to the fourth lower tooth, and the hind-feet entirely webbed. Our first suspicion on receiving this animal was, that the identity of species was proved, and that thus the real crocodile existed in the warm countries of both hemispheres.

This, however, was a result so contrary to one of the finest laws established by Buffon, a law of the greatest importance in zoology as well as in the history of the revolutions of the globe, that I did not think proper to admit this first idea without a more accurate examination.

This law, founded on an observation Buffon had made, that no species of the torrid zone had been primitively placed in both continents, had either never been contradicted, or had been so only by objections the weak foundation of which had been soon discovered.

I therefore compared the crocodile of Saint Domingo with that which I had brought from Egypt, and it gave me pleasure to find that there was a difference between these animals sufficient to make them be considered as two distinct species; for I do not think there is any reason for observing, in opposition, that their differences ought to be ascribed to age. They are both nearly of the same size;

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* See the excellent Dissertation of my colleague Cuvier, read in the National Institute, and which he published in Wiedemann's Annals of Zoölogy and Zoötomy.

and I was even enabled to confirm, that age in the crocodile gives rise to other differences than those of which I am about to speak. I can mention, by way of proof, the two individuals for which we are indebted to the enlightened zeal of general Leclerc: though different in age and size, they still appeared to me to be perfectly similar.

The crocodile of Saint Domingo resembles that of the Nile in regard to all those characters which serve to distinguish the latter from the caïman: it, however, has the jaws narrower and longer; the breadth of them is to the length as three to six. In the crocodile of the Nile the ratio is that of four to six. The body of the crocodile of Saint Domingo is also proportionably longer, and the tail consists of three bands more, twenty in the one, and seventeen in the other. The first two of the lower teeth are so long that they pierce the upper jaw from one side to the other; whereas they are smaller in that of the Nile, and form for themselves only two small cavities in which they are received. The fourth tooth of the lower jaw of the former can scarcely be distinguished from the two neighbouring ones, while in the other crocodile these fourth teeth are much larger. The plates which cover the back are much fewer in number, and more unequally distributed in the crocodile of Saint Domingo; the ridges of each are only really prominent in the exterior row, all those of the middle are almost entirely effaced: on the other hand, in the crocodile of the Nile every plate and ridge has the same form, the same prominency, and the same respective arrangement. In a word, all the scales, even those which cover the extremities, are perfectly square in the crocodile of Saint Domingo, and round or hexagonal in that of the Nile.

All these differences appear to me to furnish so many inductions proper for making us believe that the crocodile of Saint Domingo forms a species distinct from that of the Nile. But if this fact cannot be established at present in an incontestable manner, there is at least no reason to consider the law established by Buffon as invalidated by the discovery at Saint Domingo of a crocodile with elongated jaws. To give a decisive opinion in regard to this question, it would be necessary to have a more accurate knowledge of the changes which crocodiles may undergo at the different ages; that is to say, whether they are not subject to local influences which produce accidental variations, and to obtain some information respecting their habits.

XLI. *Pre-*

XLI. *Preparation of a new Luting proper to be used in all chemical Operations. By* C. Paysse, *Professor of Chemistry*[*].

It is generally admitted that the rapid progress which chemistry has made during the last twenty years, is in part owing to the different kinds of apparatus invented by the immortal Lavoisier, and the precautions employed in the art of luting. In this point of view lutings have been of essential service to chemists, since by facilitating the condensation of many aëriform products they have afforded us the means of determining their nature, and appreciating their volume as well as their gravity. This truth did not escape the sagacity of the celebrated chemist Chaptal, who in his Elements of Chemistry says: "On the art of luting an apparatus properly the whole success of an operation depends."

Among the substances most used for this purpose, are reckoned the fat luting paste of almonds or of linseed, the oil of which is extracted, and mixed with strong glue, and that of the white of eggs, and new cheese united to lime. These different kinds of luting are attended with inconveniences which render them improper for being employed under all circumstances. Fat luting for example, composed of dry clay and oil, combined with an oxide of lead, cannot be applied but on parts which receive a weak impression from the heat; for they liquefy at a low temperature, soon run, and consequently become unfit for the proposed end: that of linseed and almonds, mixed with glue or gelatine, is often too porous, easy to be destroyed by acids, and by ammonia, when in a gaseous state: those prepared with the white of eggs, and cheese, mixed with lime, are only attended with the inconveniency of becoming too soon solid, and immediately after mixture; so that it is exceedingly difficult to apply them.

The necessity I was under, in preparing oxygenated muriatic acid on a large scale, to find a luting which to the advantage of being cheap should unite that of being soon prepared, and of resisting the destructive action of the vapours of that acid, and that strong action of heat which the luted part is often obliged to sustain; of being easily applied, and in an uniform manner, without too speedily becoming hard, induced me to make some researches, which furnished me with the most satisfactory result.

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[*] From *Annales de Chimie*, No. 137.

After making a great number of mixtures with different substances, I thought it my duty to adopt the following, which gave me a homogeneous compound, which drying as slowly as could be desired, acquired very great hardness and became very compact, so as to have all the properties I wished for:

Take the whites of eggs with their yolks, carbonate of lime in powder, or lime strongly slaked in the air, equal in weight to about one half that of the eggs; and having put the whole on a piece of linen, apply it in the usual manner.

This luting, the composition of which is simple, possesses when dry a certain degree of elasticity. I have formed of it vessels impermeable to water, and susceptible of being polished by the lathe. In a word, this mixture resembles that substance called sea scum, of which tobacco pipes are made.

XLII. *Account of the Travels of* M. A. DE HUMBOLDT *in South America, extracted from some of his Letters*[*].

M. HUMBOLDT's brother, who is now at Rome, received from him lately three letters: one dated Quito, June 3, 1802; another, Cuença, July 13, 1802; and the third, Lima, the capital of Peru, November 25, 1802. They announce that M. Humboldt will soon return, and that he expected to land in the month of August or September at Cadiz, or Corunna; but the last of his letters in particular is the most interesting. In the following extract from it care has been taken to insert every thing worthy of attention in the other two:

MY DEAR BROTHER, Lima, Nov. 25, 1802.

You must have learned by my preceding letters that I had reached Quito, at which we arrived by traversing the snow of Quiridian and Tolima; for as the cordillera of the Andes forms three separate branches, and as we were at Santa Fé de Bogota, on that which is the most eastern, it was necessary to cross the highest to approach the coasts of the South Sea. Oxen are the only animals which can be employed in this passage for transporting baggage. Travellers in general are carried by men called *largeros*. They have a chair, in which the traveller is seated, tied to their back; they travel about four hours journey every day, and in five or six weeks earn only fourteen piastres. We preferred travelling on foot; and the weather being exceedingly fine, we spent only seventeen days in these solitudes, which exhibit no trace of their having ever been inhabited: we slept in huts constructed of the leaves of the heliconia, which travellers carry with them on purpose. On the western side of the Andes there are marshes in which we sunk up to the knees. The weather had changed, and during the last days of our journey there fell such torrents of rain that our boots rotted on our legs; and we arrived at Carthago with our legs naked and covered with bruises, but enriched with a beautiful collection of new plants, of which I have a great number of drawings.

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[*] From *Magazin Encyclopédique*, Thermidor, an. 11.

From Carthago we went to Popayan by Buga, crossing the beautiful valley of the river Cauca, and having always at our sides the mountain of Choca, and the platina mines which it contains.

During the month of November 1801 we remained at Popayan, and went to visit the basaltic mountains of Julusuito; the mouths of the volcano of Puracé, which with a horrid noise throw out vapours of hydro-sulphurous water; and the porphyritic granites of Pisché, which form columns of from five to seven planes, similar to those which I remember to have seen in the Fuganean mountains of Italy, and which are described by Strange.

The greatest difficulty still remained; which was, to go from Popayan to Quito. It was necessary to cross the Paramos from Pasto, and even in the rainy season, which had already commenced. The name of Paramo is given in the Andes to every place at the height of 1700 or 2000 toises, where vegetation ceases, and where a cold which penetrates to the bones is experienced. To avoid the heats of the valley of Patia, where people in the course of one night are seized with fevers which continue three or four months, and which are known under the name of *calcuturas de Patia* (fevers of Patia), we passed the summit of the cordillera by horrid precipices, in order to proceed from Popayan to Almager, and thence to Pasto, situated at the bottom of a terrible volcano.

Nothing can be more frightful than the entrance and outlet of this valley, in which we spent the Christmas holidays, and where the inhabitants received us with the utmost hospitality. They were covered with thick forests, situated among marshes where the mules sunk half up to the backs, and we passed ravines so deep and so narrow that we thought we were entering the galleries of a mine. The roads therefore are paved with the bones of mules which have perished here of cold and fatigue. The whole province of Pasto, comprehending the environs of Guachucal and Tuqueres, is a cold plain, almost above that point at which vegetation can take place, and surrounded by volcanoes and soufrieres which continually throw up clouds of smoke. The unfortunate inhabitants of these deserts have no other food but potatoes; and when these fail, as they did last year, they go into the mountains to eat the trunk of a small tree called *achupalla* (*pourretia pitcarnia*). As this tree however is the food also of the bears of the Andes, the latter often dispute with them the only nourishment which these elevated regions afford. To the north of the volcano of Pasto I discovered in the small Indian village of Voisaco, at the height of 1370 toises above the level of the sea, a red porphyry with an argillaceous base inclosing vitreous feldspar and hornstone, which has all the properties of the serpentine of the Fichtel-Gebirge. This porphyry has very evident poles, and shows no attractive force. After having been wet day and night for two months, and exposed to the danger of drowning by a very sudden rise of the waters, accompanied with earthquakes, we arrived on the 6th of January 1802 at Quito, where the marquis of Salvaalegre was so kind as to prepare for us a house, which after so many fatigues afforded us all the conveniences that we could have wished for at Paris, or at London.

Quito is a beautiful town, but the sky is dismal and cloudy. The neighbouring mountains exhibit little verdure, and the cold is very considerable. The great earthquake of February 4th 1797, which agitated the whole province, and destroyed in a moment from thity-five to forty thousand people, has also been fatal to the survivors. It has so changed the temperature of the air, that the thermometer generally stands at from 4° to 10° of Reaumur, and rarely ascends to 16° or 17°; while Bouguer always observed it at 15° or 16°. Since that catastrophe there have been continual earthquakes: and what shocks! It is probable that the whole elevated part is only one volcano. What are called the mountains of Cotopaxi and Pinchincha are only small summits, the craters of which form different apertures all terminating in the same hollow. The earthquake of 1797 unfortunately proved this hypothesis, for the earth everywhere opened and vomited up sulphur, water, &c. Notwithstanding these horrors and dangers with which nature has surrounded the inhabitants of Quito, they are cheerful, lively, and agreeable. Their town breathes nothing but pleasure; and no where does there appear so decided a taste for amusements.

ments. It is in this manner that man is accustomed to sleep soundly on the brink of a precipice.

We resided nearly eight months in the province of Quito; that is to say, from the beginning of January to the month of August, and employed that time in visiting the different volcanoes. We examined in succession the summits of Pinchincha, Cotopaxi, Antisana, and Iliniça, spending from a fortnight to three weeks on each of them; and always returning in the intervals to Quito, which we left on the 9th of June 1802 to proceed to the environs of Chimboraço, which is situated in the southern part of the province.

I twice ascended, viz. on the 26th and 28th of May 1802, to the edge of the crater of Pinchincha, a mountain which overlooks the town of Quito. Before, no person, as far as I know, except Condamine, ever saw it; and Condamine himself arrived there only after five or six days of fruitless researches, and without instruments; and, on account of the excessive cold, could remain on it only twelve or fifteen minutes. I succeeded in carrying thither my instruments; I made important measurements, and collected some of the air to analyse it. The first time I ascended I was accompanied only by an Indian. As La Condamine approached the crater at the lower part of its edge covered with snow, I made my first attempt by following his traces; but we were in danger of perishing. The Indian fell into a fissure up to the breast; and we observed with horror that we had walked on a bridge of frozen snow, for at the distance of some paces from us there were holes through which daylight appeared. We then found ourselves on arches which adhere to the very crater. Alarmed, but not discouraged, I changed my project. From the circumference of the crater there arise, projecting themselves as I may say over the abyss, three peaks or rocks not covered with snow; because the vapours exhaled from the mouth of the volcano continually dissolve it. I climbed up one of these rocks, and found at its summit a stone, which being supported at one end only, and hollow below, projected over the precipice in the form of a balcony. Here I stationed myself to make experiments. But this stone was only about twelve feet in length and six in breadth, and was strongly agitated by frequent shocks of an earthquake, of which I counted eighteen in less than thirty minutes. That we might examine the bottom of the crater better, we lay down on our bellies; and I do not think that the imagination can conceive any thing more gloomy and frightful than what we then saw. The mouth of the volcano forms a circular hole of nearly a league in circumference, the edges of which, cut perpendicularly, are covered with snow at the top. The inside is very black, but the gulph is so immense, that the summits of several mountains placed there can be distinguished. These summits seemed to be 300 toises below us: you may judge then where their bases must be. I have no doubt that the bottom of the crater is on a level with the town of Quito. La Condamine found this crater extinct, and even covered with snow; but we had melancholy news to carry to the inhabitants of Quito, that the volcano in their neighbourhood was now burning. We were convinced of this beyond all doubt by the most evident signs. When we approached the mouth of it we were almost suffocated by sulphureous vapours. We even saw blue flames moving about here and there, and every two or three minutes we experienced strong shocks of an earthquake, with which the edges of the crater were agitated, and of which nothing was perceived at the distance of 100 toises. I suppose that the great catastrophe of February 7th 1797 kindled up the flames also of Pinchincha. After visiting this mountain alone I returned two days after, accompanied by my friend Bonpland, and Charles de Montufar the son of the marquis de Selvaalegre. We were furnished with more instruments than the preceding time, and measured the diameter of the crater and the height of the mountain. We found the former to be 754 toises*, and the latter 2477. In the interval of two days which took place between our excursions to Pinchincha we had a very violent earthquake at Quito. The Indians ascribed it to some powder which I must have thrown into the volcano.

During our journey to the volcano of Antisana, the weather was so favourable that we ascended to the height of 2773 toises. The barometer fell in that elevated region to 14 inches 7 lines; and in consequence of the rarity of the air the blood flowed from our lips, gums, and even eyes. We experienced extreme weakness, and one of the persons who accompanied us fainted. It was before thought impossible to ascend higher than the summit called Corazon, which is 2470 toises in height, and which La Condamine reached. On analysing the air brought from the highest point to which we ascended, it gave 0·008 of carbonic acid for 0·218 of oxygen gas.

We paid a visit also to the volcano of Cotopaxi, but it was impossible for us to reach the mouth of the crater. It is not true that this mountain has become lower since the earthquake of 1797.

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* The crater of Veſuvius is only 312 toises in diameter.

On the 9th of June 1802 we left Quito to proceed to the southern part of the province, where we wished to examine and measure Chimboraço and Tunguragua; and to take a plan of the whole country convulsed by the grand catastrophe of 1797. We succeeded in approaching to within about 250 toises of the summit of the immense colossus of Chimboraço. A ridge of volcanic rocks, destitute of snow, facilitated our ascent. We ascended to the height of 3031 toises, and felt ourselves incommoded in the same manner as we had been on the summit of Antisana. Two or three days even after our return to the plain we were still subject to an indisposition, which we could ascribe only to the effect of the air in these elevated regions, which by analysis gave us 20 hundreds of oxygen. The Indians by whom we were accompanied deserted us before we reached that height, saying that we intended to kill them. Bonpland, Charles Montufar, and one of my domestics, who carried a part of my instruments, were the only persons who remained with me: nevertheless, we could have continued our journey to the top had we not been prevented by a fissure too deep to be crossed. We therefore thought proper to descend. Being ill secured from the cold of these elevated regions, we suffered very much; and I in particular had the misfortune to lacerate my foot by a fall a few days before, which subjected me to great pain in a road where we every moment struck our toes against sharp stones, and were obliged to examine the ground at every step. La Condamine found the height of Chimboraço to be nearly 3217 toises. Trigonometrical measurement, which I made at two different times, gave 3267 toises, and I have reason to place some confidence in my operations. All this enormous colossus, as well as all the high mountains of the Andes, consists not of granite, but of porphyry, from the base to the summit; and the porphyry is 1900 toises in thickness. The short stay which we made at that enormous height was dismal and melancholy: we were enveloped by a thick fog, which only suffered us from time to time to have a glimpse of the horrid abysses by which we were surrounded. No living creature, not even the condour, which on Antisana continually hovered over our heads, was to be seen. Small kinds of moss were the only organized beings which reminded us that we were still in the neighbourhood of the earth.

It is almost probable that Chimboraço, like Pinchincha and Antisana, is of a volcanic nature. The ridge on which we ascended consists of burnt and scorified rock, mixed with pumice stone. It resembles all the currents of lava in this country; and continues beyond that point where I was obliged to set bounds to my researches, towards the summit of the mountain. It is possible that this summit may be the crater of an extinguished volcano; and this is even probable. The idea of this possibility, however, makes one shudder—and with reason; for, if the volcano should be rekindled, this colossus would destroy the whole province.

The mountain of Tunguragua sunk down at the period of the earthquake of 1797. Bouguer makes its height to be 2620 toises; I found it to be only 2531: it has lost therefore nearly 100 toises of its height. The inhabitants of the neighbouring country assert that they have seen its summit crumble down before their eyes.

During our stay at Riobamba, where we spent some weeks with the brother of Charles Montufar, who is corregidor there, we by chance made a very curious discovery. The state of the province of Quito before the conquest of the inca Tupayupagi * is absolutely unknown. But the king of the Indians, Leandro Zapla, who resides at Lican, and whose mind is highly cultivated, has in his possession manuscripts written by one of his ancestors in the sixteenth century, which contain the history of that period. These manuscripts are written in the language of Paraguay, which formerly was the general language of Quito; but in the course of time it gave place to that of the Incas, or the Anichna, and is now lost. Fortunately, another of Zapla's ancestors amused himself in translating these manuscripts into Spanish. We made extracts from these valuable documents, and particularly in regard to the memorable period of the eruption of the mountain called *Nevado del Attas,* which must have been the highest in the universe, superior even to Chimboraço, and which the Indians called *Capa-Urcu,* 'the chief of mountains.' Ouainia Abomatha, the last independent *cochocando* (king of the country), reigned at that time at Lican. The priests informed him that this catastrophe was a sinister presage of his destruction. "The face of the universe," said they to him, "is changing: other gods will expel ours. Let us not oppose what has been ordained by fate." The Peruvians indeed introduced into the country the worship of the sun. The eruption of the mountain continued seven years, and Zapla's manuscript asserts that the shower of ashes at Lican was so abundant that continual night prevailed during that period. When the quantity of volcanic matters which are found in the plain of Tapia around the enormous mountain which then crumbled to pieces, is considered, and when we reflect that Cotopaxi has often involved Quito in darkness for fifteen or eighteen hours, we may believe that the exaggeration of this account is not very great. This manuscript, the traditions which I collected at Parime, and the hieroglyphics I saw in the desert of Casiquiare, where at present no vestiges of mankind remain, added to the ideas offered by Clavigero respecting the emigration of the Mexicans towards the southern part of America, have given rise to some conjectures on the origin of these people, which I purpose to explain as soon as I can find leisure.

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* Quito was conquered by the Peruvians in 1470.

I have applied also with great assiduity to the study of the American languages, and I have seen how much what La Condamine says of their poverty is false. The Carib language is rich, beautiful, energetic, and polished: it is in no want of expressions for abstract ideas. It speaks of posterity, eternity, existence, &c.; and the numerical signs are sufficient to express all the possible combinations of figures. I applied in particular to the Inca language: it is generally spoken in company; and is so rich in delicate and varied phrases, that the young men, in order to say soft things to the ladies, when they have exhausted all the resources of the Castillan, begin to speak Inca. These two languages, and others equally rich, are sufficient to prove that America formerly possessed a greater degree of culture than the Spaniards found there in 1492. But I have collected still further proofs, not only at Mexico and in Peru, but even at the court of the king of Bogota, a country the history of which is absolutely unknown in Europe, and whose mythology even and fabulous traditions are highly interesting. The priests were acquainted with the art of drawing a meridian line, and observing the moment of the solstice. They reduced the lunar year to a solar by intercalations; and I have in my possession a heptagon stone, found near Santa Fé, which they employed for calculating these intercalary days. But what is still more, even at Erevato, in the interior of Parime, the savages believe that the moon is inhabited by men; and know by tradition from their ancestors, that it derives its light from the sun.

From Riobamba I proceeded by the famous paramo of Assuay towards Cuença, after having visited the large sulphur mines of Tirrau. It was this mountain of sulphur which the negroes who revolted in 1797, after the earthquake, attempted to set on fire. This no doubt was the most desperate project ever attempted, for they hoped by these means to form a volcano which would swallow up the whole province of Alaussy. At the height of the paramo of Assuay, an elevation of 2300 toises, are the magnificent ruins of the Inca's highway. It conducted almost to Cuzco, was entirely constructed of cut stone, and very straight, and resembled the most beautiful of the Roman roads. In the same neighbourhood are found also the ruins of the palace of the inca Tupayupangi, of which La Condamine gave a description in the Memoirs of the Academy of Berlin. In the quarry which furnished the stones there are still seen several half cut. I do not know whether Condamine spoke also of the so-called Inca's billiard-table. The Indians name this place in the Quichua language, *Inca-chungana*, the Inca's game. But I much doubt whether it was ever destined for that purpose. It is a seat cut out in the rock, with ornaments in the arabesc form, in which it is believed that the ball ran. There is nothing more elegant in our gardens in the English style; and every thing proves the good taste of the inca, for the seat is so situated as to command a delightful view. In a wood not far from this place is found a round spot of yellow iron in freestone: the Peruvians have ornamented it with figures, supposing it to be the image of the sun. I made a drawing of it.

We remained only ten days at Cuença, and proceeded thence to Lima through the province of Jaen, where we spent a month in the neighbourhood of the river of the Amazons. We arrived at Lima on the 23d of October 1802.

In the month of December I purpose proceeding from this place to Acapulco, and thence to Mexico, that in the month of May 1803 I may reach the Havanna, where I shall embark without delay for Spain. I have given up, as you may see, the idea of returning by the Philippines. I should have crossed an immense tract of the ocean without seeing any thing but Manilla and the Cape; or, if I had attempted to proceed to the East Indies, I should have wanted the necessaries for that voyage, and which it was impossible for me to procure here.

We have had forty or fifty young crocodiles, on the respiration of which I have made very curious experiments. Other animals diminish the volume of the air in which they live, but the crocodile increases it. A crocodile immersed in 1000 parts of atmospheric air, which contain 274 of oxygen gas, 15 of carbonic acid gas, and 711 of azot, increases this mass in one hour and forty-three minutes 124 parts;

and these 1124 parts contain then, as I found by exact analysis, 106·8 of oxygen, 79 of carbonic acid, and 938·2 of azotic gas, mixed with other unknown gaseous substances. The crocodile then in one hour and three quarters produces 64 parts of carbonic acid, and absorbs 167·2 of oxygen; but as 46 parts are found in the 64 parts of carbonic acid, it appropriates to itself only 121 parts of oxygen; which is very little, considering the colour of its blood. It produces 227 parts of azote, or other gaseous substances, on which the acidifiable bases exercise no action.

I made these experiments in the town of Munpox with lime water and nitrous gas prepared with great care. The crocodile is so sensible to carbonic acid gas and to its own exhalations, that it dies when put into air corrupted by one of its own species. It however can live two or three hours without breathing at all. I made these experiments on crocodiles seven or eight inches in length: notwithstanding this smallness of size, they are capable of cutting off a finger with their teeth, and they have the courage to attack a dog. These experiments are troublesome, and require great circumspection. We have made very minute descriptions of the caïman or crocodile of South America; but as the descriptions of that of Egypt which I saw before my departure from Europe were not equally circumstantial, I cannot venture to determine whether they are of the same species. The Institute of Egypt must undoubtedly have obtained details which will remove all doubt in regard to this point. This much however is certain, that there are three different species of crocodile in the tropical regions of the new continent, to which the inhabitants give the names of *bava*, *caïman*, and *crocodile*. No naturalist has yet sufficiently distinguished these species. These monsters, as at New Barcelona, are sometimes of so peaceable a nature that people bathe before them; and sometimes, as at New Guiana, they are so mischievous and ferocious, that during the time we were there they devoured an Indian on the quay in the middle of the street. At Oratuen we saw an Indian girl, eighteen years of age, whom a crocodile seized by the arm. She had the courage to put her other hand into her pocket to pull out her knife, with which she gave the monster so many wounds in the eyes that he let her go, but cut off the arm near the shoulder. This girl's presence of mind was as astonishing as the skill displayed by the Indians in speedily curing so dangerous a wound: one might have said that the arm was amputated and dressed at Paris.

Near Santa-Fé there are found in the Campo de Gigante, at the height of 1370 toises, an immense number of fossil elephants' bones, both of the African species and of the carnivorous kind discovered near the Ohio. We caused several to be dug up, and have sent some specimens of them to the National Institute. I much doubt whether any of these bones were ever before found at such a great height: since that time I have received two from a place of the Andes situated about two degrees of latitude from Quito and Chili, so that I can prove the existence and destruction of these gigantic elephants from the Ohio to the country of the Patagonians. I shall bring with me a fine collection of these bones for M. Cuvier. About fifteen years ago the entire petrified skeleton of a crocodile was discovered in a calcareous rock in the valley of the Magdalen: it was broken through ignorance, and it was impossible for me to procure the head, which existed not long ago.

XLIII. *The Art of moulding Carving in Wood. By* LENORMAND, *Professor of Natural Philosophy in the Central School of the Department of Tarn*[*].

INGENIOUS or curious men are often thwarted in the execution of their projects by the difficulty of finding in the places where they reside workmen capable of assisting them in the articles for which they may have occasion. Small towns in particular furnish only indifferent workmen; and besides, they do not contain artists of every kind. Good carvers, for example, reside only in large towns; and these even are not very common. I had seen plasterers supply the want of good modellers by incrusting in their decorations plaster moulded on excellent models. I therefore conceived that it might be possible to mould carving in wood, to be afterwards applied to cabinet-makers' work. This idea I did not at first carry into execution; but two or three years after, having occasion for some pieces of carving, I invented a new art[†], as will be seen by what follows. Necessity rendered me industrious, and I at length accomplished my object.

Wishing to obtain a case for a pendulum clock I had constructed, I drew a plan of it; and presented it to an excellent cabinet-maker in the small town in which I resided.

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[*] From *Bibliothèque Physico-Economique*, June 1803.

[†] This art is not new; but the experiments of the author may furnish useful hints to artists.—EDIT.

He would undertake only the plain work, and referred me for the execution of the carving to Toulouse or Bourdeaux. I was sensible how difficult it would be to get the carving of the different pieces executed at a distance, and particularly within the required time; and how expensive it would be to transport such a case, which might also be damaged by the way. I told him that I would myself undertake the carving of the laurel and oak foliage which I had placed in the plan, provided he would undertake the remaining part. Fearing, however, that my carving would not correspond to his work, and might tend to degrade it, he was unwilling to undertake any thing till I had shown him a specimen of my labour—a proposal to which I consented.

I was well aware that very hard wood, such as box, might be moulded by putting it under a press in copper moulds, after having subjected it to certain preparations: but for this purpose very expensive moulds, an excellent press, &c. are required, which occasions considerable expense, and by this method bas-reliefs only can be executed. But the art I am about to describe requires only cheap materials with very little practice, and affords the means of making not only figures in relief, but even the most difficult objects of sculpture.

In the town where I resided I found one of those Italians who employ themselves in moulding plaster figures. I caused him to make such moulds as I had occasion for, and which were copies from the best masters. I succeeded perfectly in moulding my garlands in walnut-tree wood; and I showed them to my cabinet-maker, who took me for an able sculptor. He constructed the case, applied to it the foliage I had made, and neither he nor any person who saw it had the least suspicion of the method I had employed. All believed that the ornaments had proceeded from the chisel of an able carver. Since that time I have moulded for my friends bas-reliefs, trophies, &c. with wood of every kind. I shall now describe my

Process.

I made very clear glue with five parts of Flanders glue and one part of fish glue or isinglass. I dissolved these two kinds of glue separately in a large quantity of water, and mixed them together after they had been strained through a piece of fine linen to separate the filth and heterogeneous parts which could not be dissolved. The quantity of water cannot be fixed, because all kinds of glue are not homogeneous, so that some require more and some less. The proper degree of liquidity may be known by suffering the mixed glue to become perfectly cold: it must then form a jelly, or rather a commencement of jelly. If it happens that it is still liquid when cold, a little of the water must be evaporated by exposing the vessel in which it is contained to heat. On the other hand, if it has too much consistence, a little warm water must be added. In a word, the proper degree will be ascertained by a few trials.

The glue thus prepared is to be heated till you can scarcely endure your finger in it: by this operation a little water is evaporated, and the glue acquires more consistence. Then take fine raspings of wood or sawdust, sifted through a fine hair-sieve, and form it into a paste, which must be put into moulds of plaster or sulphur after they have been well rubbed over with linseed- or nut-oil, in the same manner as when plaster is to be moulded. Care must be taken to press the paste in the mould with your hand, in order that it may acquire all the forms of the mould: then cover it with an oiled board, and, placing over it a weight, suffer it in that manner to dry. The desiccation may be hastened and rendered more complete by a stove. When the impression is dry remove the rough parts, and if any inequalities remain behind they must be smoothed; after which the impression may be affixed with glue to the article for which it is intended. Then cover it with a few strata of spirit of wine varnish, as is done in general in regard to carved work, or with wax in the encaustic manner. It requires much attention to discover that such ornaments are not carved in the usual manner. Gilding may be applied to them with great facility. This operation is exceedingly easy; nothing is necessary but moulds; and with a little art the ornaments may be infinitely varied.

I tried also to mould figures, and completely succeeded. These, however, require more care. I first make a paste, similar to the former, with very fine sawdust, and place a stratum, of about two lines in thickness, on every part of the mould; after which it is left to dry almost entirely. In the mean time I prepare a coarse paste with coarse sawdust which has not been made to pass through a fine but a coarse sieve, and instead of Flanders glue I employ common glue, which is less expensive, adding to it a sixth of fish glue. I first put together two parts of the mould, after introducing into the joints a slight stratum of the fine paste, which I make very clear and apply with a small brush. I fill up the vacuity between the two pieces with coarse paste. I then apply the third piece as I did the second, and so on until the whole are adjusted, always filling up the vacuities with coarse paste. I suffer the whole to dry in the mould, and obtain a figure in relief of solid wood executed with all the delicacy of plaster figures. Care must be taken to remove with a sharp knife, or small file, the prominences formed by the joinings. If the figure be not suffered to dry too much, these prominences may be easily removed with the point of a sharp penknife. It will be necessary to learn the art of determining the proper degree of desiccation; for if the figure be taken from the mould before it is properly dried it will become warped, and if it be too dry it cannot be corrected but with a file, which is tedious and laborious, whereas if the proper moment be seized the paste may be cut like wax; especially if the sawdust has been fine which is necessary for the exterior strata. The figures may then be completely dried in a stove, by which means they will acquire a degree of desiccation and solidity hardly to be conceived. Figures thus moulded may be bronzed or varnished: they will then be unalterable by the effects of moisture or dryness.

I have already said that Flanders and not common glue ought to be employed for the exterior strata, because this glue is almost colourless*; whereas the other, being dark-coloured, gives too obscure a tint even to walnut-tree wood. Being desirous to try whether my moulded figures would be unalterable by the effects of moisture or dryness, I made the following experiments:

Experiment I.

I exposed in a large bell-glass filled with atmospheric air two figures, one of which was varnished and the other not. I placed under the bell Saussure's hygrometer and a capsule filled with water, after having moistened the sides of the bell. The air was soon saturated with water, and the hygrometer marked 100 degrees. I observed no alteration whatever in the varnished figure, and the other exhibited no other sensible alteration than a commencement of solution in the glue, so that on applying my finger to its surface it was found to be somewhat viscid; in a word, the figure was not in the least warped.

Experiment II.

I then introduced my two figures and the hygrometer into another very dry bell, under which I had placed a capsule filled with calcined potash. The moisture of the air by which the figures were surrounded was soon absorbed, and the hygrometer indicated zero. In order to ascertain whether the whole moisture imbibed by the unvarnished figure was entirely dissipated, I left every thing *in statu quo* for four hours, the hygrometer still indicating zero. I then took out the two figures, neither of which had experienced the least alteration.

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* When this cannot be had, a glue fit for the purpose may be made by boiling shreds of parchment in common water till dissolved.—Edit.

Experiment III.

I repeated the first experiment with a view to cause the two figures to absorb as much moisture as possible; and when the hygrometer marked 100° I took them from the bell, and suddenly introduced them into a stove the heat of which was 50° of Reaumur. The unvarnished one became dry without cracking, and the other showed a little softening in the varnish. This effect I ascribed to the imperfect desiccation before the experiment, for the softening was more considerable than is generally the case when a varnished body is exposed to heat.

These experiments appeared to me sufficient to induce me to conclude, that sculpture in moulded wood, according to the process here described, is unalterable by moisture or drought, for in our climates the thermometer never rises to 50°. Such sculptured figures have the solidity of wood, and are even preferable to it; for a slight blow given to wood, if cut across the fibres, will detach some of the parts; whereas figures formed of artificial wood, if I may be allowed the expression, are homogeneous in all their parts, and are not so easily broken.

Besides the advantages which this invention on the first view exhibits, it offers others which may be of great utility to our arts and manufactures.

1st, In the large manufactories of mirrors the ornaments in general are in a very bad taste and miserably executed, because the carvers are very ill paid. If this new method be adopted, sculptors would pay more attention to their first work: they would mould their ornaments in plaster or in sulphur, then take a multitude of copies with the greatest facility, and these ornaments would add to the value of our furniture.

2d, Inlayers would make much more elegant works by employing pastes of different coloured woods, which might be managed with greater ease than the thin pieces of coloured boards which they employ. I am now engaged with some true that this mountain has become lower since the earthquake of 1797.

On the 9th of June 1802 we left Quito to proceed to the southern part of the province, where we wished to examine and measure Chimboraço and Tunguragua; and to take a plan of the whole country convulsed by the grand catastrophe of 1797. We succeeded in approaching to within about 250 toises of the summit of the immense colossus of Chimboraço. A ridge of volcanic rocks, destitute of snow, facilitated our ascent. We ascended to the height of 3031 toises, and felt ourselves incommoded in the same manner as we had been on the summit of Antisana. Two or three days even after our return to the plain we were still subject to an indisposition, which we could ascribe only to the effect of the air in these elevated regions, which by analysis gave us 20 hundreds of oxygen. The Indians by whom we were accompanied deserted us before we reached that height, saying that we intended to kill them. Bonpland, Charles Montufar, and one of my domestics, who carried a part of my instruments, were the only persons who remained with me: nevertheless, we could have continued our journey to the top had we not been prevented by a fissure too deep to be crossed. We therefore thought proper to descend. Being ill secured from the cold of these elevated regions, we suffered very much; and I in particular had the misfortune to lacerate my foot by a fall a few days before, which subjected me to great pain in a road where we every moment struck our toes against sharp stones, and were obliged to examine the ground at every step. La Condamine found the height of Chimboraço to be nearly 3217 toises. Trigonometrical measurement, which I made at two different times, gave 3267 toises, and I have reason to place some confidence in my operations. All this enormous colossus, as well as all the high mountains of the Andes, consists not of granite, but of porphyry, from the base to the summit; and the porphyry is 1900 toises in thickness. The short stay which we made at that enormous height was dismal and melancholy: we were enveloped by a thick fog, which only suffered us from time to time to have a glimpse of the horrid abysses by which we were surrounded. No living creature, not even the condour, which on Antisana continually hovered over our heads, was to be seen. Small kinds of moss were the only organized beings which reminded us that we were still in the neighbourhood of the earth.

It is almost probable that Chimboraço, like Pinchincha and

dren, four of whom, with three of my domestics, passed regularly through the distemper; and they soon after went into the licensed small-pox hospital in this neighbourhood, and all seven of them were inoculated by Dr. Aspinwall with the matter of the small-pox, without the least trait of infection." Page 5.

The error concerning the inoculation of the cow-pock in America would not have happened if Dr. Chichester's account had not failed in getting to Europe; nor would the first introduction have been imputed to the Vaccine Institution, as was supposed from a passage in Dr. Lettsom's book on the cow-pock, viz. "The vaccine matter which first succeeded with professor Waterhouse was transmitted from England in a bottle with a glass stopper*." Page 24.

London,
July 24, 1803.

XLV. *Account of the Life and Labours of the late Mr.* RAMSDEN, *in a Letter from Professor* PIAZZI, *of Palermo, to M.* DE LALANDE†.

WHEN I had the pleasure of seeing you lately at London, you admired, as I did, the genius and works of the celebrated Ramsden, which has induced me to address to you such circumstances as I have been able to collect respecting the life and labours of this incomparable artist. No one has contributed more to the progress of astronomy than you have done by your zeal, and by your works on the principles and calculations of that science; and Mr. Ramsden is certainly the first for inventing and constructing instruments: but as he is not so well known in France, perhaps, as he deserves to be, my letter may serve to give your countrymen a just idea of his merit.

Jesse Ramsden was born at Halifax, in Yorkshire, on the 6th of October 1730. At an early period he conceived a strong desire of devoting himself to literature, and especially to history and antiquities: the mathematics and chemistry engaged his attention also in their turn: but his father was anxious that he should pursue some occupation which might be useful to him; and as he was a clothier, young Ramsden applied to the same employment till he had

attained to the age of twenty-one. He then went to London, to seek for some occupation more suited to his genius. Besides other things, he applied to engraving under Burton*: and a fortunate circumstance conducted him to that object for which nature seemed to have destined him, which was to be the reviver and father of the instrumental part of astronomy. Mathematical instruments were often brought to him to be engraven: the more he examined them the more he was sensible of their defects, and a secret instinct made him desirous of constructing better ones. He therefore resolved to make an attempt in this line: he soon acquired the use of the file, and made himself acquainted with the method of turning brass, and even of grinding glasses. In the year 1763 he constructed instruments for Sisson, Dollond, Nairne, Adams, and other mathematical instrument makers. He then established a shop on his own account, in the Hay-market, about the year 1768; from which he removed to Piccadilly in 1775. Having formed a design of examining all the astronomical instruments, he resolved to correct those which being founded on good principles were defective only in the construction, and to set aside those which were wrong in both these respects.

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* This mode of transmitting matter was peculiar for a time to the Vaccine Institution, now at No. 44, Broad-street, Golden-square.

† From the *Journal des Sçavans*, Nov. 1788.

Hadley's sextant, which is so much employed in the British navy, appeared to him one of the most useful, but it was then very imperfect; the essential parts were not of sufficient strength; the centre was subject to too much friction; and the index could be moved several minutes without any change being produced in the position of the mirror; the divisions in general were very coarse; and Mr. Ramsden found that the abbé de la Caille was right, when he estimated at five minutes the error which might take place in the observed distances of the moon and stars, and which

might occasion in the longitude an error of fifty nautical leagues. Mr. Ramsden therefore changed the construction in regard to the centre, and made these instruments so correct as to give never more than half a minute of uncertainty. At present he warrants sextants of fifteen inches radius to within six seconds. Since the time when he improved these instruments he has constructed 983; and several of them having been carried to India and America, the error has been found to correspond with what he determined it to be before their departure. He has since made sextants from fifteen inches to an inch and a half radius, and in the latter the minutes can be clearly distinguished; but in general he prefers those of ten inches, as being easier managed and susceptible of the same exactness.

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* Mr. Burton was a thermometer and barometer maker, and divider of instruments. Instruments at this period were divided by means of a plate applied to them, and the divisions were in this manner marked off. Mr. Burton was one of the best workmen of his time, and worked for Short, Bird, and other eminent artists. Mr. Ramsden bound himself apprentice to Mr. Burton for four years; and after his time was expired entered into partnership with Mr. Fairbone, who lived afterwards in New-street, Shoe-lane. This partnership, however, did not long continue. Mr. Ramsden opened a shop on his own account in the Strand, and, having married miss Dollond, became possessed of a part of Mr. Dollond's patent for achromatic telescopes. Mr. Ramsden in the course of a few years removed to the Hay-market, and then to Piccadilly, where he died in the year 1800. Mr. Ramsden had seven children; but none of them are now alive, except one son, captain John Ramsden of the East India company's service, and late commander of the Dorchester.—EDITOR.

The invention of a dividing machine having now become necessary, he employed himself in constructing one, which he did with the greatest success. The dividing machines before used were far from being exact. Graham and Bird employed beam compasses. The latter kept his method a secret till it was purchased from him by the board of longitude, in order to be published. Mr. Ramsden had already discovered a method of his own, which in exactness surpassed that of Bird. For large works he still uses the beam compasses; but as it is necessary in the greater number of common instruments to save time, he has employed himself for ten years in improving his dividing machine, in which ease and expedition are united. You have seen that admirable machine with which a sextant could be divided in the course of twenty minutes, and which is sufficient to give an idea of the inventive genius and superior talents of Mr. Ramsden. It was your friend Dr. Shepherd who made this excellent machine known to the board of longitude, who gave the inventor a premium of 600l. sterling, and caused an engraving to be made of it in 1777: but the edition was accidentally burned, and you are right in wishing to cause it to be engraved at Paris. The machine is still in the hands of Mr. Ramsden, and he has undertaken to divide sextants for three shillings. The board of longitude has often given greater premiums for objects of less utility; but the greatest men do not always obtain the greatest rewards. Newton, indeed, got a place in the mint; but he was not indebted for it to his merit alone.

Mr. Ramsden has constructed an instrument also for dividing straight lines, a description of which has been printed; and I am sorry that you have no longer at Paris that invented by M. Megnie, in order that they might be compared.

While Mr. Ramsden was employed on his dividing machine, he improved at the same time other instruments. The theodolite before consisted merely of a telescope, turning on a circle divided at every three minutes, by means of a vernier; but in the hands of Mr. Ramsden it has become a new and perfect instrument, which serves for measuring heights and distances as well as for taking angles. I saw in his possession the largest and most wonderful of all theodolites, employed by general Roy for measuring the triangles which at present join those of France, and by which there cannot be an error of a second, though it is only eighteen inches radius. It is furnished with two telescopes, which each turn on a horizontal axis, and by which the angles between objects more or less elevated are reduced to the horizon, and measured. General Roy has lately measured the angle between the pole star and the sides of his triangles, in order to have the convergency of the meridians such as it is in our oblate spheroid. These operations have already shown that the difference between the meridians of the observatories of Paris and Greenwich is 9′ 20″.

The barometer destined for measuring the heights of mountains has been much improved by Mr. Ramsden. His method of marking at the bottom the line of the level, and of looking at the top to the contact of the index with the summit of the mercury, renders it possible to distinguish the hundredth part of a line, and to measure heights within a foot. He showed M. de Luc that it is the summit of the column, and not the part which touches the glass, that ought to be observed; and he caused to be engraved a table, which accompanies his barometers, and which, without calculation, gives the heights of places according to the height of the barometer, and even for different degrees of heat. He has simplified also, in the most ingenious manner, the apparatus for carrying and supporting this portable barometer.

Various other philosophical machines have been made by Mr. Ramsden, and always with new improvements: such as an electric machine; a manometer for measuring the density of the air; an instrument for measuring inaccessible distances, and which renders it unnecessary to measure a base; assaying balances which turn with a ten-thousandth part of the weight; levels exceedingly sensible; the optic rectangle, prismatic eye-glasses where much fewer rays are lost than by the reflection of an inclined mirror, when it is necessary to look on one side; the dynameter, with which he measures the magnifying power of a telescope, by applying before the eye-glass a small scale divided into hundredths

of a line to measure the pencil or image of the object-glass. This was the original invention, but it was afterwards much improved.

The pyrometer, destined to measure the dilatation of bodies by heat, afforded exercise also for the talents of Mr. Ramsden; and with the happiest success, as may be seen in the Philosophical Transactions for 1785. Mr. Ramsden, on examining the pyrometer then in use, had observed the radical defect of that instrument, in which the bodies subjected to experiment were not sufficiently separated. But with his microscopic pyrometer he found means to compare the natural state of a body with the same body exposed to any degree of heat or of cold, and by a micrometer adapted to the microscope he measured these variations with an exactness before unknown, and which furnished the measurement of a base with a precision ten times greater than in any of those ever before measured. On this occasion, as on all others, Mr. Ramsden showed a natural sagacity in discovering the essential faults of every instrument, and in inventing the most simple and most exact methods of correcting them.

Optics are no less indebted to him. He found means to correct the aberration of sphericity and refrangibility in compound eye-glasses applied to all astronomical instruments, and in a new and perfect manner. Opticians had imagined that this could be accomplished by making the image of the object-glass fall between the two eye-glasses; which was attended with this great inconvenience, that the eye-glass could not be touched without deranging the line of collimation, and the value of the parts of the micrometer. To remedy this inconvenience Mr. Ramsden set out from a very simple experiment, namely, that the edges of an image observed through a prism are less coloured according as the image is nearer the prism; and, in consequence of this truth, he sought for the means of placing the two eye-glasses between the image of the object-glass and the eye, without failing to correct the two aberrations, which he did by changing the radii of the curves, and placing the glasses in a manner altogether different from that commonly employed.

He invented also a reflecting object-glass micrometer, a description of which may be seen in the Transactions of the Royal Society of London for 1779. In his paper on this subject he points out the defects and inconveniences of that of Bouguer, first invented in 1748, in which the different positions of the eye, in regard to the pencil of light, cause the two images to appear sometimes to touch each other, sometimes to be separated, and sometimes alternately by a until the whole are adjusted, always filling up the vacuities with coarse paste. I suffer the whole to dry in the mould, and obtain a figure in relief of solid wood executed with all the delicacy of plaster figures. Care must be taken to remove with a sharp knife, or small file, the prominences formed by the joinings. If the figure be not suffered to dry too much, these prominences may be easily removed with the point of a sharp penknife. It will be necessary to learn the art of determining the proper degree of desiccation; for if the figure be taken from the mould before it is properly dried it will become warped, and if it be too dry it cannot be corrected but with a file, which is tedious and laborious, whereas if the proper moment be seized the paste may be cut like wax; especially if the sawdust has been fine which is necessary for the exterior strata. The figures may then be completely dried in a stove, by which means they will acquire a degree of desiccation and solidity hardly to be conceived. Figures thus moulded may be bronzed or varnished: they will then be unalterable by the effects of moisture or dryness.

I have already said that Flanders and not common glue ought to be employed for the exterior strata, because this glue is almost colourless*; whereas the other, being dark-coloured, gives too obscure a tint even to walnut-tree wood. Being desirous to try whether my moulded figures would be unalterable by the effects of moisture or dryness, I made the following experiments:

Experiment I.

I exposed in a large bell-glass filled with atmospheric air two figures, one of which was varnished and the other not. I placed under the bell Saussure's hygrometer and a capsule filled with water, after having moistened the sides of the bell. The air was soon saturated with water, and the hygrometer marked 100 degrees. I observed no alteration whatever in the varnished figure, and the other exhibited no other sensible alteration than a commencement of solution in the glue, so that on applying my finger to its surface it was found to be somewhat viscid; in a word, the figure was not in the least warped.

Experiment II.

I then introduced my two figures and the hygrometer into another very dry bell, under which I had placed a capsule filled with calcined potash. The moisture of the air by which the figures were surrounded was soon absorbed, and the hygrometer indicated zero. In order to ascertain whether the whole moisture imbibed by the unvarnished figure was entirely dissipated, I left every thing *in statu quo* for four hours, the hygrometer still indicating zero. I then took out the two figures, neither of which had experienced the least alteration.

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* When this cannot be had, a glue fit for the purpose may be made by boiling shreds of parchment in common water till dissolved.—Edit.

Experiment III.

I repeated the first experiment with a view to cause the two figures to absorb as much moisture as possible; and when the hygrometer marked 100° I took them from the bell, and suddenly introduced them into a stove the heat of which was 50° of Reaumur. The unvarnished one became dry without cracking, and the other showed a little softening in the varnish. This effect I ascribed to the imperfect desiccation before the experiment, for the softening was more considerable than is generally the case when a varnished body is exposed to heat.

These experiments appeared to me sufficient to induce me to conclude, that sculpture in moulded wood, according to the process here described, is unalterable by moisture or drought, for in our climates the thermometer never rises to 50°. Such sculptured figures have the solidity of wood, and are even preferable to it; for a slight blow given to wood, if cut across the fibres, will detach some of the parts; whereas figures formed of artificial wood, if I may be allowed the expression, are homogeneous in all their parts, and are not so easily broken.

Besides the advantages which this invention on the first view exhibits, it offers others which may be of great utility to our arts and manufactures.

1st, In the large manufactories of mirrors the ornaments in general are in a very bad taste and miserably executed, because the carvers are very ill paid. If this new method be adopted, sculptors would pay more attention to their first work: they would mould their ornaments in plaster or in sulphur, then take a multitude of copies with the greatest facility, and these ornaments would add to the value of our furniture.

2d, Inlayers would make much more elegant works by employing pastes of different coloured woods, which might be managed with greater ease than the thin pieces of coloured boards which they employ. I am now engaged with some experiments on this subject. My intention is to make small tablets to imitate mosaic. I shall communicate the result to the public as soon as my experiments are terminated.

XLIV. *Evidence of the precise Date of the Cow-Pock in America.*

To Mr. Tilloch.

SIR,

FINDING that the honour of the first instances of the vaccine inoculation in America is not bestowed upon the gentleman to whom it is due, and that there have been even some little contradiction and mistakes among the English practitioners with regard to which of them first introduced the new practice into that country by furnishing matter, I trust the following statement of facts may establish the historical truth in question.

In the winter of the year 1799 Dr. John Chichester, a practitioner of the first distinction in Charleston, South Carolina, and to whom I have been pupil, received vaccine matter from his learned friend and former teacher Dr. Pearson, accompanying the first publications written on the cow-pock by Dr. Jenner and himself. With this matter several persons were inoculated, but the disease was produced in one case only. This was a mulatto boy named Robert, about seven or eight years old, the property of Thomas Tunno, esq. merchant. The small-pox matter was subsequently inserted, in the most careful manner, without effect. It was some time after the occurrence of the above case, before those which have been published as the first instances in America really happened.

It may be proper to notice that my late worthy master Dr. Chichester was not supported by the approbation of his brethren in his introduction of the vaccine inoculation in America, notwithstanding the high authorities above mentioned who first proposed it to the public.

I remain, sir, your humble servant,

NATH. H. RHODES.

P. S. Since my arrival in London I have seen doctor Waterhouse's latest Treatise on the Variola Vaccina, 8vo. Cambridge 1802: hence I am enabled to fix the precise date of *his first* inoculation from his own words, viz.:—“I commenced the experiment July 8, 1800, on my own children,

dren, four of whom, with three of my domestics, passed regularly through the distemper; and they soon after went into the licensed small-pox hospital in this neighbourhood, and all seven of them were inoculated by Dr. Aspinwall with the matter of the small-pox, without the least trait of infection." Page 5.

The error concerning the inoculation of the cow-pock in America would not have happened if Dr. Chichester's account had not failed in getting to Europe; nor would the first introduction have been imputed to the Vaccine Institution, as was supposed from a passage in Dr. Lettsom's book on the cow-pock, viz. "The vaccine matter which first succeeded with professor Waterhouse was transmitted from England in a bottle with a glass stopper*." Page 24.

London,
July 24, 1803.

XLV. *Account of the Life and Labours of the late Mr.* RAMSDEN, *in a Letter from Professor* PIAZZI, *of Palermo, to M.* DE LALANDE†.

WHEN I had the pleasure of seeing you lately at London, you admired, as I did, the genius and works of the celebrated Ramsden, which has induced me to address to you such circumstances as I have been able to collect respecting the life and labours of this incomparable artist. No one has contributed more to the progress of astronomy than you have done by your zeal, and by your works on the principles and calculations of that science; and Mr. Ramsden is certainly the first for inventing and constructing instruments: but as he is not so well known in France, perhaps, as he deserves to be, my letter may serve to give your countrymen a just idea of his merit.

Jesse Ramsden was born at Halifax, in Yorkshire, on the 6th of October 1730. At an early period he conceived a strong desire of devoting himself to literature, and especially to history and antiquities: the mathematics and chemistry engaged his attention also in their turn: but his father was anxious that he should pursue some occupation which might be useful to him; and as he was a clothier, young Ramsden applied to the same employment till he had

attained to the age of twenty-one. He then went to London, to seek for some occupation more suited to his genius. Besides other things, he applied to engraving under Burton*: and a fortunate circumstance conducted him to that object for which nature seemed to have destined him, which was to be the reviver and father of the instrumental part of astronomy. Mathematical instruments were often brought to him to be engraven: the more he examined them the more he was sensible of their defects, and a secret instinct made him desirous of constructing better ones. He therefore resolved to make an attempt in this line: he soon acquired the use of the file, and made himself acquainted with the method of turning brass, and even of grinding glasses. In the year 1763 he constructed instruments for Sisson, Dollond, Nairne, Adams, and other mathematical instrument makers. He then established a shop on his own account, in the Hay-market, about the year 1768; from which he removed to Piccadilly in 1775. Having formed a design of examining all the astronomical instruments, he resolved to correct those which being founded on good principles were defective only in the construction, and to set aside those which were wrong in both these respects.

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* This mode of transmitting matter was peculiar for a time to the Vaccine Institution, now at No. 44, Broad-street, Golden-square.

† From the *Journal des Sçavans*, Nov. 1788.

Hadley's sextant, which is so much employed in the British navy, appeared to him one of the most useful, but it was then very imperfect; the essential parts were not of sufficient strength; the centre was subject to too much friction; and the index could be moved several minutes without any change being produced in the position of the mirror; the divisions in general were very coarse; and Mr. Ramsden found that the abbé de la Caille was right, when he estimated at five minutes the error which might take place in the observed distances of the moon and stars, and which

might occasion in the longitude an error of fifty nautical leagues. Mr. Ramsden therefore changed the construction in regard to the centre, and made these instruments so correct as to give never more than half a minute of uncertainty. At present he warrants sextants of fifteen inches radius to within six seconds. Since the time when he improved these instruments he has constructed 983; and several of them having been carried to India and America, the error has been found to correspond with what he determined it to be before their departure. He has since made sextants from fifteen inches to an inch and a half radius, and in the latter the minutes can be clearly distinguished; but in general he prefers those of ten inches, as being easier managed and susceptible of the same exactness.

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* Mr. Burton was a thermometer and barometer maker, and divider of instruments. Instruments at this period were divided by means of a plate applied to them, and the divisions were in this manner marked off. Mr. Burton was one of the best workmen of his time, and worked for Short, Bird, and other eminent artists. Mr. Ramsden bound himself apprentice to Mr. Burton for four years; and after his time was expired entered into partnership with Mr. Fairbone, who lived afterwards in New-street, Shoe-lane. This partnership, however, did not long continue. Mr. Ramsden opened a shop on his own account in the Strand, and, having married miss Dollond, became possessed of a part of Mr. Dollond's patent for achromatic telescopes. Mr. Ramsden in the course of a few years removed to the Hay-market, and then to Piccadilly, where he died in the year 1800. Mr. Ramsden had seven children; but none of them are now alive, except one son, captain John Ramsden of the East India company's service, and late commander of the Dorchester.—EDITOR.

The invention of a dividing machine having now become necessary, he employed himself in constructing one, which he did with the greatest success. The dividing machines before used were far from being exact. Graham and Bird employed beam compasses. The latter kept his method a secret till it was purchased from him by the board of longitude, in order to be published. Mr. Ramsden had already discovered a method of his own, which in exactness surpassed that of Bird. For large works he still uses the beam compasses; but as it is necessary in the greater number of common instruments to save time, he has employed himself for ten years in improving his dividing machine, in which ease and expedition are united. You have seen that admirable machine with which a sextant could be divided in the course of twenty minutes, and which is sufficient to give an idea of the inventive genius and superior talents of Mr. Ramsden. It was your friend Dr. Shepherd who made this excellent machine known to the board of longitude, who gave the inventor a premium of 600l. sterling, and caused an engraving to be made of it in 1777: but the edition was accidentally burned, and you are right in wishing to cause it to be engraved at Paris. The machine is still in the hands of Mr. Ramsden, and he has undertaken to divide sextants for three shillings. The board of longitude has often given greater premiums for objects of less utility; but the greatest men do not always obtain the greatest rewards. Newton, indeed, got a place in the mint; but he was not indebted for it to his merit alone.

Mr. Ramsden has constructed an instrument also for dividing straight lines, a description of which has been printed; and I am sorry that you have no longer at Paris that invented by M. Megnie, in order that they might be compared.

While Mr. Ramsden was employed on his dividing machine, he improved at the same time other instruments. The theodolite before consisted merely of a telescope, turning on a circle divided at every three minutes, by means of a vernier; but in the hands of Mr. Ramsden it has become a new and perfect instrument, which serves for measuring heights and distances as well as for taking angles. I saw in his possession the largest and most wonderful of all theodolites, employed by general Roy for measuring the triangles which at present join those of France, and by which there cannot be an error of a second, though it is only eighteen inches radius. It is furnished with two telescopes, which each turn on a horizontal axis, and by which the angles between objects more or less elevated are reduced to the horizon, and measured. General Roy has lately measured the angle between the pole star and the sides of his triangles, in order to have the convergency of the meridians such as it is in our oblate spheroid. These operations have already shown that the difference between the meridians of the observatories of Paris and Greenwich is 9′ 20″.

The barometer destined for measuring the heights of mountains has been much improved by Mr. Ramsden. His method of marking at the bottom the line of the level, and of looking at the top to the contact of the index with the summit of the mercury, renders it possible to distinguish the hundredth part of a line, and to measure heights within a foot. He showed M. de Luc that it is the summit of the column, and not the part which touches the glass, that ought to be observed; and he caused to be engraved a table, which accompanies his barometers, and which, without calculation, gives the heights of places according to the height of the barometer, and even for different degrees of heat. He has simplified also, in the most ingenious manner, the apparatus for carrying and supporting this portable barometer.

Various other philosophical machines have been made by Mr. Ramsden, and always with new improvements: such as an electric machine; a manometer for measuring the density of the air; an instrument for measuring inaccessible distances, and which renders it unnecessary to measure a base; assaying balances which turn with a ten-thousandth part of the weight; levels exceedingly sensible; the optic rectangle, prismatic eye-glasses where much fewer rays are lost than by the reflection of an inclined mirror, when it is necessary to look on one side; the dynameter, with which he measures the magnifying power of a telescope, by applying before the eye-glass a small scale divided into hundredths

of a line to measure the pencil or image of the object-glass. This was the original invention, but it was afterwards much improved.

The pyrometer, destined to measure the dilatation of bodies by heat, afforded exercise also for the talents of Mr. Ramsden; and with the happiest success, as may be seen in the Philosophical Transactions for 1785. Mr. Ramsden, on examining the pyrometer then in use, had observed the radical defect of that instrument, in which the bodies subjected to experiment were not sufficiently separated. But with his microscopic pyrometer he found means to compare the natural state of a body with the same body exposed to any degree of heat or of cold, and by a micrometer adapted to the microscope he measured these variations with an exactness before unknown, and which furnished the measurement of a base with a precision ten times greater than in any of those ever before measured. On this occasion, as on all others, Mr. Ramsden showed a natural sagacity in discovering the essential faults of every instrument, and in inventing the most simple and most exact methods of correcting them.

Optics are no less indebted to him. He found means to correct the aberration of sphericity and refrangibility in compound eye-glasses applied to all astronomical instruments, and in a new and perfect manner. Opticians had imagined that this could be accomplished by making the image of the object-glass fall between the two eye-glasses; which was attended with this great inconvenience, that the eye-glass could not be touched without deranging the line of collimation, and the value of the parts of the micrometer. To remedy this inconvenience Mr. Ramsden set out from a very simple experiment, namely, that the edges of an image observed through a prism are less coloured according as the image is nearer the prism; and, in consequence of this truth, he sought for the means of placing the two eye-glasses between the image of the object-glass and the eye, without failing to correct the two aberrations, which he did by changing the radii of the curves, and placing the glasses in a manner altogether different from that commonly employed.

He invented also a reflecting object-glass micrometer, a description of which may be seen in the Transactions of the Royal Society of London for 1779. In his paper on this subject he points out the defects and inconveniences of that of Bouguer, first invented in 1748, in which the different positions of the eye, in regard to the pencil of light, cause the two images to appear sometimes to touch each other, sometimes to be separated, and sometimes alternately by a

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and these 1124 parts contain then, as I found by exact analysis, 106·8 of oxygen, 79 of carbonic acid, and 938·2 of azotic gas, mixed with other unknown gaseous substances. The crocodile then in one hour and three quarters produces 64 parts of carbonic acid, and absorbs 167·2 of oxygen; but as 46 parts are found in the 64 parts of carbonic acid, it appropriates to itself only 121 parts of oxygen; which is very little, considering the colour of its blood. It produces 227 parts of azote, or other gaseous substances, on which the acidifiable bases exercise no action.

I made these experiments in the town of Munpox with lime water and nitrous gas prepared with great care. The crocodile is so sensible to carbonic acid gas and to its own exhalations, that it dies when put into air corrupted by one of its own species. It however can live two or three hours without breathing at all. I made these experiments on crocodiles seven or eight inches in length: notwithstanding this smallness of size, they are capable of cutting off a finger with their teeth, and they have the courage to attack a dog. These experiments are troublesome, and require great circumspection. We have made very minute descriptions of the caïman or crocodile of South America; but as the descriptions of that of Egypt which I saw before my departure from Europe were not equally circumstantial, I cannot venture to determine whether they are of the same species. The Institute of Egypt must undoubtedly have obtained details which will remove all doubt in regard to this point. This much however is certain, that there are three different species of crocodile in the tropical regions of the new continent, to which the inhabitants give the names of *bava*, *caïman*, and *crocodile*. No naturalist has yet sufficiently distinguished these species. These monsters, as at New Barcelona, are sometimes of so peaceable a nature that people bathe before them; and sometimes, as at New Guiana, they are so mischievous and ferocious, that during the time we were there they devoured an Indian on the quay in the middle of the street. At Oratuen we saw an Indian girl, eighteen years of age, whom a crocodile seized by the arm. She had the courage to put her other hand into her pocket to pull out her knife, with which she gave the monster so many wounds in the eyes that he let her go, but cut off the arm near the shoulder. This girl's presence of mind was as astonishing as the skill displayed by the Indians in speedily curing so dangerous a wound: one might have said that the arm was amputated and dressed at Paris.

Near Santa-Fé there are found in the Campo de Gigante, much further improved by Ramsden. He first suppressed the endless screw, which by pressing on the centre destroyed its precision. He placed the centre of gravity on the centre of the base, and caused all the movements to take place in every direction. He pointed out the means of rectifying the instrument in all its parts; and he applied to it a very ingenious small machine for measuring or correcting the effect of refraction. This invention is much anterior to that given by Mr. Dollond in the Philosophical Transactions. Mr. Ramsden had a patent for this kind of equatorial. The honourable Stewart Mackenzie, brother of the earl of Bute, the friend and patron of Mr. Ramsden, wrote a description of this machine, which has been printed. But Mr. Ramsden does not always strictly adhere to the description: his inventive genius rarely allows him to construct the same instrument many times in the same manner; and it often happens that he breaks to pieces instruments which have cost a great deal of labour, if they are not as correct as he wishes.

The largest equatorial instrument ever constructed is that destined for Sir George Shuckburgh, on which Mr. Ramsden has been employed for nine years. The circle of inclination is four feet in diameter, so that observations can be made nearly within a second. The telescope is placed between six pillars, which form the axis of the machine; and the whole turns around two pivots resting on supporters of masonwork.

The transit instrument is employed in all the large observatories of Europe; but Mr. Ramsden has added to it several improvements. He invented a method of illuminating the wires, by making the light pass along the axis of the machine. The reflector is placed in the inside, and obliquely in the middle. He did not lessen the aperture of the object-glass: and as the light passes through a coloured prism, which may be moved at pleasure, the light may be increased or diminished. For adjusting this essential instrument, Mr. Ramsden invented a method which supersedes the use of a spirit-of-wine level, on which he sets no value, because it does not give that exactness which it is always his aim to obtain. He suspends a thread and plummet before the telescope placed vertically: this thread passes over two points, which are marked on two pieces fixed one above and the other below the telescope, and one of which has a small motion. The thread is absolutely detached from the telescope; and when it corresponds on the same points in the two different situations of the telescope, the observer is certain that the axis is horizontal, as you have remarked in your Astronomy. What is newer and more ingenious in Mr. Ramsden's method is, that the thread and plummet sometimes pass over the images only of the points which are formed in the focus of a lens, because he is sometimes obliged to remove the thread to a considerable distance from the instrument and from the points; but the exactness is not lessened, and there is no parallax.

Ramsden's meridian telescopes, such as those at Blenheim, at Manheim, at Dublin, and such as those made for the observatories of Paris and Gotha, are also remarkable for the excellence of the object-glasses. Mr. Usher, in a letter from Dublin, says that he can see in the open day stars of the fourth magnitude, and those of the third very near their conjunction with the sun. These telescopes are eight feet in length. I had the satisfaction to obtain one of five feet for my observatory of Palermo; but it is so good, and the sky is so serene, that I hope to have the same advantage in my observations.

The mural quadrant is the most important of all the astronomical instruments, and Mr. Ramsden has distinguished himself here also by the exactness of his divisions: he places the thread and plummet behind the instrument, in order that it may not be necessary to remove it when observations are made near the zenith. His method of illuminating the object-glass and at the same time the divisions, and of suspending the telescope, is also new, and consequently more perfect. In those of eight feet which he made for the observatories of Padua and Vilna, and which Dr. Maskelyne examined, the greatest error does not exceed two seconds and a half. One for Milan is in a great state of forwardness, and is of the same size.

The mural quadrant of the duke of Marlborough at Blenheim, which is six feet, is one of the instruments which you and I admired. It is fixed to four pillars which turn on two pivots, so that the instrument may be placed north and south in a minute. This instrument is as beautiful as perfect, and no one deserves more to possess it than his grace the duke of Marlborough: professed astronomers are not more zealous, assiduous, or correct. It was for this noble instrument that Mr. Ramsden invented a method of rectifying the arc of 90 degrees, respecting which an able astronomer had started some difficulties; but with a horizontal thread, and a thread and plummet forming a kind of cross which does not touch the quadrant, he showed him that there was not a second of error in 90 degrees; and that the difference arose from a mural quadrant of Bird, where the arc of 90 degrees contained several seconds too much, and which had not been verified by so exact a method as his.

But the quadrant is not the instrument which Mr. Ramsden values most. It is the whole circle; and he has proved that to attain to the utmost degree of precision of which observation is susceptible, we must renounce the quadrant entirely. His principal reasons are, 1st, The least variation in the centre is perceived by the two points diametrically opposite. 2d, As the circle is turned the plane is always rigorously exact; which cannot possibly be the case in the quadrant. 3d, Two measurements can always be had of the same arc; which serves for verifying the accuracy of the observation. 4th, The first point of the division can be verified every day with the greatest ease. 5th, The dilatation of the metal is uniform, and can produce no error. 6th, This instrument is a meridian telescope as well as a mural. 7th, It becomes a moveable azimuth circle by adding a horizontal circle below the axis, and then gives the refractions independently of the measure of time. You therefore approved the resolution I had formed of confining myself to this instrument, and of not quitting London till I could carry with me a circle of five feet, which Mr. Ramsden was constructing for the observatory of Palermo: as soon as mine is finished he promises to put in hand that destined for Paris. He then hopes to finish that for Dublin, which is twelve feet, and which you saw in an advanced state; but a circle of 7 or 8 feet is sufficient to give precision within half a second, as in the zenith sector, which is employed for the most rigorous observations in regard to the figure of the earth.

You remarked with the greatest satisfaction the ingenious manner in which the axis is supported, that it may have no friction on the pivots, and especially Mr. Ramsden's new invention for rendering the axis perfectly horizontal, by means of a thread and plummet, which is however without the machine; and you had the pleasure of seeing that inventive genius exercise his talents on this new problem, and solve it in the completest manner. As his talents have been exercised in a wide field, he has collected in his shops workmen in all professions which relate to the construction of mathematical and philosophical instruments, that he might have every thing made under his own inspection. The same workman is always confined to the same branch, and by these means acquires the utmost degree of correctness. But notwithstanding this perfection, which ought to enable Mr. Ramsden to make a fortune, he sells his instruments cheaper than any other artist in the same line at London: the difference is sometimes a third. Though he has nearly 60 workmen in his employment he is not able to execute all the orders which he has received from every part of the world, and you yourself have experienced how difficult it is to obtain instruments from him.

No person can be more reasonable, more attentive to business, or more indifferent for pleasure or for riches than Mr. Ramsden: he is exceedingly frugal in his manner of life; and, unless provoked, no one can be more polite, milder, or more complaisant. I hope, sir, you will publish with pleasure this tribute of my gratitude to a man of uncommon talents, whom you esteem as much as I do, and who, in his turn, has conceived for you a real attachment.

XLVI. *Some Details respecting the Voyage of the two French Corvettes, Le Geographe and Le Naturaliste, sent out under the Command of Captain* Baudin *for the Purpose of making Discoveries.*

These two corvettes sailed from the north-west port of the Isle of France on the 25th of April 1801, and on the 27th of May discovered the land of New Holland in lat. 34° 36′, long. 111° 44′: the land they saw was that called Leeuwin's Land, which forms the south-west extremity of New Holland, and which the charts indicate as little known.

Preparations were then made for exploring the country and the two corvettes were employed in this service from the 27th of May to the 14th of June. As these were the first geographical operations made on board these vessels, it appears that they are not very correct; and captain Baudin has sent home no account of them, nor any of the charts which he caused to be constructed. He announces that on his return from Port Jackson he proposes to revist Leeuwin's Land, and to repeat the operations undertaken, in order to construct a chart of Bai du Geographe, which he found in that part.

Among the charts brought home by the Naturaliste is one of the same coast of Leeuwin's Land, executed on board the Naturaliste by C. Faure: it contains an extent of coast from lat. 34° 23′ to 32° 13′, but coarsely delineated and without any details.

After these operations the two corvettes were separated, and did not again meet till they arrived at the Island of Timor; but both of them sailed along the coast of the Land of Endracht, which lies to the north of Leeuwin's Land.

Captain Baudin entered the Bay of Seals on the 27th of June, and remained there till the 13th of July. Here Bernier, the astronomer, made some observations, which fix the longitude of the northern point of Barren Island at 109° 13′ 46″. The course of the Geographe in this bay and on the coast to the north of it is traced out on a chart constructed by C. Boulanger, one of the engineers belonging to the expedition. This chart, however, is only a copy of a Dutch chart given to captain Baudin before his departure, and which C. Boulanger corrected by the results of astronomical observations.

Captain Baudin observes in his memoir, that his first operations in regard to this coast, and that of De Witt's and of Leeuwin's Land, were not satisfactory; and he purposes to repeat them on his return from Port Jackson. He has therefore sent home none of the charts of the early part of his voyage along these coasts. Among the charts which the Naturaliste has brought home there is only one small chart of a part of the coast of New Holland, where the Geographe landed; and which is supposed to be on the coast of De Witt's Land, or of the north-west. This chart, constructed by Rousard, an officer of the marine engineers, is merely a sketch taken at sight, and may give some idea of the observations made on board the Geographe; though, as captain Baudin announces, it contains only fragments of the coasts. He says he transmitted to the minister of the marine, in a letter dated Timor, October 6, 1801, some details respecting his navigation on the western coasts of New Holland from Leeuwin's Land, and that he then announced that they were by no means satisfactory. He arrived at Timor, and entered Coupang Bay on the 22d of August 1801.

The Naturaliste being separated from the Geographe on the coast of Leeuwin's Land proceeded to the Island of Rottenest, which had been agreed on as the first place of rendezvous in case of separation. The captain explored that island as well as another in the neighbourhood, and which is not marked in the charts: he called it Isle-aux-Ours. He examined at the same time Swan's River, on the coast of New Holland, opposite to the Isle of Rottenest: some boats were ordered to proceed up that river as far as they possibly could without danger; and a chart of it was constructed, some experiments on this subject. My intention is to make small tablets to imitate mosaic. I shall communicate the result to the public as soon as my experiments are terminated.

XLIV. *Evidence of the precise Date of the Cow-Pock in America.*

To Mr. Tilloch.

SIR,

FINDING that the honour of the first instances of the vaccine inoculation in America is not bestowed upon the gentleman to whom it is due, and that there have been even some little contradiction and mistakes among the English practitioners with regard to which of them first introduced the new practice into that country by furnishing matter, I trust the following statement of facts may establish the historical truth in question.

In the winter of the year 1799 Dr. John Chichester, a practitioner of the first distinction in Charleston, South Carolina, and to whom I have been pupil, received vaccine matter from his learned friend and former teacher Dr. Pearson, accompanying the first publications written on the cow-pock by Dr. Jenner and himself. With this matter several persons were inoculated, but the disease was produced in one case only. This was a mulatto boy named Robert, about seven or eight years old, the property of Thomas Tunno, esq. merchant. The small-pox matter was subsequently inserted, in the most careful manner, without effect. It was some time after the occurrence of the above case, before those which have been published as the first instances in America really happened.

It may be proper to notice that my late worthy master Dr. Chichester was not supported by the approbation of his brethren in his introduction of the vaccine inoculation in America, notwithstanding the high authorities above mentioned who first proposed it to the public.

I remain, sir, your humble servant,

NATH. H. RHODES.

P. S. Since my arrival in London I have seen doctor Waterhouse's latest Treatise on the Variola Vaccina, 8vo. Cambridge 1802: hence I am enabled to fix the precise date of *his first* inoculation from his own words, viz.:—" I commenced the experiment July 8, 1800, on my own children,

dren, four of whom, with three of my domestics, passed regularly through the distemper; and they soon after went into the licensed small-pox hospital in this neighbourhood, and all seven of them were inoculated by Dr. Aspinwall with the matter of the small-pox, without the least trait of infection." Page 5.

The error concerning the inoculation of the cow-pock in America would not have happened if Dr. Chichester's account had not failed in getting to Europe; nor would the first introduction have been imputed to the Vaccine Institution, as was supposed from a passage in Dr. Lettsom's book on the cow-pock, viz. "The vaccine matter which first succeeded with professor Waterhouse was transmitted from England in a bottle with a glass stopper*." Page 24.

London,
July 24, 1803.

XLV. *Account of the Life and Labours of the late Mr.* RAMSDEN, *in a Letter from Professor* PIAZZI, *of Palermo, to M.* DE LALANDE†.

WHEN I had the pleasure of seeing you lately at London, you admired, as I did, the genius and works of the celebrated Ramsden, which has induced me to address to you such circumstances as I have been able to collect respecting the life and labours of this incomparable artist. No one has contributed more to the progress of astronomy than you have done by your zeal, and by your works on the principles and calculations of that science; and Mr. Ramsden is certainly the first for inventing and constructing instruments: but as he is not so well known in France, perhaps, as he deserves to be, my letter may serve to give your countrymen a just idea of his merit.

Jesse Ramsden was born at Halifax, in Yorkshire, on the 6th of October 1730. At an early period he conceived a strong desire of devoting himself to literature, and especially to history and antiquities: the mathematics and chemistry engaged his attention also in their turn: but his father was anxious that he should pursue some occupation which might be useful to him; and as he was a clothier, young Ramsden applied to the same employment till he had

attained to the age of twenty-one. He then went to London, to seek for some occupation more suited to his genius. Besides other things, he applied to engraving under Burton*: and a fortunate circumstance conducted him to that object for which nature seemed to have destined him, which was to be the reviver and father of the instrumental part of astronomy. Mathematical instruments were often brought to him to be engraven: the more he examined them the more he was sensible of their defects, and a secret instinct made him desirous of constructing better ones. He therefore resolved to make an attempt in this line: he soon acquired the use of the file, and made himself acquainted with the method of turning brass, and even of grinding glasses. In the year 1763 he constructed instruments for Sisson, Dollond, Nairne, Adams, and other mathematical instrument makers. He then established a shop on his own account, in the Hay-market, about the year 1768; from which he removed to Piccadilly in 1775. Having formed a design of examining all the astronomical instruments, he resolved to correct those which being founded on good principles were defective only in the construction, and to set aside those which were wrong in both these respects.

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* This mode of transmitting matter was peculiar for a time to the Vaccine Institution, now at No. 44, Broad-street, Golden-square.

† From the *Journal des Sçavans*, Nov. 1788.

Hadley's sextant, which is so much employed in the British navy, appeared to him one of the most useful, but it was then very imperfect; the essential parts were not of sufficient strength; the centre was subject to too much friction; and the index could be moved several minutes without any change being produced in the position of the mirror; the divisions in general were very coarse; and Mr. Ramsden found that the abbé de la Caille was right, when he estimated at five minutes the error which might take place in the observed distances of the moon and stars, and which

might occasion in the longitude an error of fifty nautical leagues. Mr. Ramsden therefore changed the construction in regard to the centre, and made these instruments so correct as to give never more than half a minute of uncertainty. At present he warrants sextants of fifteen inches radius to within six seconds. Since the time when he improved these instruments he has constructed 983; and several of them having been carried to India and America, the error has been found to correspond with what he determined it to be before their departure. He has since made sextants from fifteen inches to an inch and a half radius, and in the latter the minutes can be clearly distinguished; but in general he prefers those of ten inches, as being easier managed and susceptible of the same exactness.

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* Mr. Burton was a thermometer and barometer maker, and divider of instruments. Instruments at this period were divided by means of a plate applied to them, and the divisions were in this manner marked off. Mr. Burton was one of the best workmen of his time, and worked for Short, Bird, and other eminent artists. Mr. Ramsden bound himself apprentice to Mr. Burton for four years; and after his time was expired entered into partnership with Mr. Fairbone, who lived afterwards in New-street, Shoe-lane. This partnership, however, did not long continue. Mr. Ramsden opened a shop on his own account in the Strand, and, having married miss Dollond, became possessed of a part of Mr. Dollond's patent for achromatic telescopes. Mr. Ramsden in the course of a few years removed to the Hay-market, and then to Piccadilly, where he died in the year 1800. Mr. Ramsden had seven children; but none of them are now alive, except one son, captain John Ramsden of the East India company's service, and late commander of the Dorchester.—EDITOR.

The invention of a dividing machine having now become necessary, he employed himself in constructing one, which he did with the greatest success. The dividing machines before used were far from being exact. Graham and Bird employed beam compasses. The latter kept his method a secret till it was purchased from him by the board of longitude, in order to be published. Mr. Ramsden had already discovered a method of his own, which in exactness surpassed that of Bird. For large works he still uses the beam compasses; but as it is necessary in the greater number of common instruments to save time, he has employed himself for ten years in improving his dividing machine, in which ease and expedition are united. You have seen that admirable machine with which a sextant could be divided in the course of twenty minutes, and which is sufficient to give an idea of the inventive genius and superior talents of Mr. Ramsden. It was your friend Dr. Shepherd who made this excellent machine known to the board of longitude, who gave the inventor a premium of 600l. sterling, and caused an engraving to be made of it in 1777: but the edition was accidentally burned, and you are right in wishing to cause it to be engraved at Paris. The machine is still in the hands of Mr. Ramsden, and he has undertaken to divide sextants for three shillings. The board of longitude has often given greater premiums for objects of less utility; but the greatest men do not always obtain the greatest rewards. Newton, indeed, got a place in the mint; but he was not indebted for it to his merit alone.

Mr. Ramsden has constructed an instrument also for dividing straight lines, a description of which has been printed; and I am sorry that you have no longer at Paris that invented by M. Megnie, in order that they might be compared.

While Mr. Ramsden was employed on his dividing machine, he improved at the same time other instruments. The theodolite before consisted merely of a telescope, turning on a circle divided at every three minutes, by means of a vernier; but in the hands of Mr. Ramsden it has become a new and perfect instrument, which serves for measuring heights and distances as well as for taking angles. I saw in his possession the largest and most wonderful of all theodolites, employed by general Roy for measuring the triangles which at present join those of France, and by which there cannot be an error of a second, though it is only eighteen inches radius. It is furnished with two telescopes, which each turn on a horizontal axis, and by which the angles between objects more or less elevated are reduced to the horizon, and measured. General Roy has lately measured the angle between the pole star and the sides of his triangles, in order to have the convergency of the meridians such as it is in our oblate spheroid. These operations have already shown that the difference between the meridians of the observatories of Paris and Greenwich is 9′ 20″.

The barometer destined for measuring the heights of mountains has been much improved by Mr. Ramsden. His method of marking at the bottom the line of the level, and of looking at the top to the contact of the index with the summit of the mercury, renders it possible to distinguish the hundredth part of a line, and to measure heights within a foot. He showed M. de Luc that it is the summit of the column, and not the part which touches the glass, that ought to be observed; and he caused to be engraved a table, which accompanies his barometers, and which, without calculation, gives the heights of places according to the height of the barometer, and even for different degrees of heat. He has simplified also, in the most ingenious manner, the apparatus for carrying and supporting this portable barometer.

Various other philosophical machines have been made by Mr. Ramsden, and always with new improvements: such as an electric machine; a manometer for measuring the density of the air; an instrument for measuring inaccessible distances, and which renders it unnecessary to measure a base; assaying balances which turn with a ten-thousandth part of the weight; levels exceedingly sensible; the optic rectangle, prismatic eye-glasses where much fewer rays are lost than by the reflection of an inclined mirror, when it is necessary to look on one side; the dynameter, with which he measures the magnifying power of a telescope, by applying before the eye-glass a small scale divided into hundredths

of a line to measure the pencil or image of the object-glass. This was the original invention, but it was afterwards much improved.

The pyrometer, destined to measure the dilatation of bodies by heat, afforded exercise also for the talents of Mr. Ramsden; and with the happiest success, as may be seen in the Philosophical Transactions for 1785. Mr. Ramsden, on examining the pyrometer then in use, had observed the radical defect of that instrument, in which the bodies subjected to experiment were not sufficiently separated. But with his microscopic pyrometer he found means to compare the natural state of a body with the same body exposed to any degree of heat or of cold, and by a micrometer adapted to the microscope he measured these variations with an exactness before unknown, and which furnished the measurement of a base with a precision ten times greater than in any of those ever before measured. On this occasion, as on all others, Mr. Ramsden showed a natural sagacity in discovering the essential faults of every instrument, and in inventing the most simple and most exact methods of correcting them.

Optics are no less indebted to him. He found means to correct the aberration of sphericity and refrangibility in compound eye-glasses applied to all astronomical instruments, and in a new and perfect manner. Opticians had imagined that this could be accomplished by making the image of the object-glass fall between the two eye-glasses; which was attended with this great inconvenience, that the eye-glass could not be touched without deranging the line of collimation, and the value of the parts of the micrometer. To remedy this inconvenience Mr. Ramsden set out from a very simple experiment, namely, that the edges of an image observed through a prism are less coloured according as the image is nearer the prism; and, in consequence of this truth, he sought for the means of placing the two eye-glasses between the image of the object-glass and the eye, without failing to correct the two aberrations, which he did by changing the radii of the curves, and placing the glasses in a manner altogether different from that commonly employed.

He invented also a reflecting object-glass micrometer, a description of which may be seen in the Transactions of the Royal Society of London for 1779. In his paper on this subject he points out the defects and inconveniences of that of Bouguer, first invented in 1748, in which the different positions of the eye, in regard to the pencil of light, cause the two images to appear sometimes to touch each other, sometimes to be separated, and sometimes alternately by a sort of oscillation. He found also that the aberration of the rays, which renders the object badly defined, increased the inconvenience of that instrument. He thought it would therefore be necessary to abandon the principle of refraction, and to substitute that of reflection. This instrument, as simple as ingenious, contains no more mirrors or glasses than what are necessary for the telescope; and the separation of the two images depends only on the inclination of the mirrors, and not on the focus.

He however employed himself in improving the refracting micrometer, and conceived the happy idea of placing this micrometer not towards the object-glass, but exactly in the conjugate focus of the first eye-glass. This micrometer is composed of two plano-convex lenses, which can be moved and form two images, as in the object-glass micrometers, but with this difference, that the rays before they fall on the plano-convex lenses pass through a lens convex on both sides, at a certain distance towards the object-glass. By these means the contrary refraction of the two plano-convex lenses, and the convex lens, corrects the error which takes place in object-glass micrometers, where the image depends only on the focus of the two plano-convex lenses. The image being already considerably magnified before it falls on the refracting micrometer, the imperfection of the glasses can occasion only an insensible error in the measurement of angles. It is true, indeed, that by this position the field of the micrometer will be smaller than what it would be were the micrometer near the object-glass. Mr. Ramsden devised means also for making the images to be uniformly illuminated in every part of the field. With this micrometer the diameter of the planets may be measured in every direction; it may be adapted to achromatic telescopes of every kind; it may be brought near to or removed from the object-glass at pleasure, to render vision distinct; and it may be taken from the tube of the eye-glasses, that the telescope may be employed without a micrometer. All these advantages have given great reputation to Ramsden's micrometers, and the astronomer who can obtain one of them is fortunate.

In consequence of these and other inventions Mr. Ramsden was elected a fellow of the Royal Society in 1786.

The objects hitherto mentioned, however, are not the most important of the works of Mr. Ramsden. The equatorial, the transit instrument, and quadrant, received in his hands new improvements. The equatorial first constructed by Sisson, and which was somewhat improved by Short, was much further improved by Ramsden. He first suppressed the endless screw, which by pressing on the centre destroyed its precision. He placed the centre of gravity on the centre of the base, and caused all the movements to take place in every direction. He pointed out the means of rectifying the instrument in all its parts; and he applied to it a very ingenious small machine for measuring or correcting the effect of refraction. This invention is much anterior to that given by Mr. Dollond in the Philosophical Transactions. Mr. Ramsden had a patent for this kind of equatorial. The honourable Stewart Mackenzie, brother of the earl of Bute, the friend and patron of Mr. Ramsden, wrote a description of this machine, which has been printed. But Mr. Ramsden does not always strictly adhere to the description: his inventive genius rarely allows him to construct the same instrument many times in the same manner; and it often happens that he breaks to pieces instruments which have cost a great deal of labour, if they are not as correct as he wishes.

The largest equatorial instrument ever constructed is that destined for Sir George Shuckburgh, on which Mr. Ramsden has been employed for nine years. The circle of inclination is four feet in diameter, so that observations can be made nearly within a second. The telescope is placed between six pillars, which form the axis of the machine; and the whole turns around two pivots resting on supporters of masonwork.

The transit instrument is employed in all the large observatories of Europe; but Mr. Ramsden has added to it several improvements. He invented a method of illuminating the wires, by making the light pass along the axis of the machine. The reflector is placed in the inside, and obliquely in the middle. He did not lessen the aperture of the object-glass: and as the light passes through a coloured prism, which may be moved at pleasure, the light may be increased or diminished. For adjusting this essential instrument, Mr. Ramsden invented a method which supersedes the use of a spirit-of-wine level, on which he sets no value, because it does not give that exactness which it is always his aim to obtain. He suspends a thread and plummet before the telescope placed vertically: this thread passes over two points, which are marked on two pieces fixed one above and the other below the telescope, and one of which has a small motion. The thread is absolutely detached from the telescope; and when it corresponds on the same points in the two different situations of the telescope, the observer is certain that the axis is horizontal, as you have remarked in your Astronomy. What is newer and more ingenious in Mr. Ramsden's method is, that the thread and plummet sometimes pass over the images only of the points which are formed in the focus of a lens, because he is sometimes obliged to remove the thread to a considerable distance from the instrument and from the points; but the exactness is not lessened, and there is no parallax.

Ramsden's meridian telescopes, such as those at Blenheim, at Manheim, at Dublin, and such as those made for the observatories of Paris and Gotha, are also remarkable for the excellence of the object-glasses. Mr. Usher, in a letter from Dublin, says that he can see in the open day stars of the fourth magnitude, and those of the third very near their conjunction with the sun. These telescopes are eight feet in length. I had the satisfaction to obtain one of five feet for my observatory of Palermo; but it is so good, and the sky is so serene, that I hope to have the same advantage in my observations.

The mural quadrant is the most important of all the astronomical instruments, and Mr. Ramsden has distinguished himself here also by the exactness of his divisions: he places the thread and plummet behind the instrument, in order that it may not be necessary to remove it when observations are made near the zenith. His method of illuminating the object-glass and at the same time the divisions, and of suspending the telescope, is also new, and consequently more perfect. In those of eight feet which he made for the observatories of Padua and Vilna, and which Dr. Maskelyne examined, the greatest error does not exceed two seconds and a half. One for Milan is in a great state of forwardness, and is of the same size.

The mural quadrant of the duke of Marlborough at Blenheim, which is six feet, is one of the instruments which you and I admired. It is fixed to four pillars which turn on two pivots, so that the instrument may be placed north and south in a minute. This instrument is as beautiful as perfect, and no one deserves more to possess it than his grace the duke of Marlborough: professed astronomers are not more zealous, assiduous, or correct. It was for this noble instrument that Mr. Ramsden invented a method of rectifying the arc of 90 degrees, respecting which an able astronomer had started some difficulties; but with a horizontal thread, and a thread and plummet forming a kind of cross which does not touch the quadrant, he showed him that there was not a second of error in 90 degrees; and that the difference arose from a mural quadrant of Bird, where the arc of 90 degrees contained several seconds too much, and which had not been verified by so exact a method as his.

But the quadrant is not the instrument which Mr. Ramsden values most. It is the whole circle; and he has proved that to attain to the utmost degree of precision of which observation is susceptible, we must renounce the quadrant entirely. His principal reasons are, 1st, The least variation in the centre is perceived by the two points diametrically opposite. 2d, As the circle is turned the plane is always rigorously exact; which cannot possibly be the case in the quadrant. 3d, Two measurements can always be had of the same arc; which serves for verifying the accuracy of the observation. 4th, The first point of the division can be verified every day with the greatest ease. 5th, The dilatation of the metal is uniform, and can produce no error. 6th, This instrument is a meridian telescope as well as a mural. 7th, It becomes a moveable azimuth circle by adding a horizontal circle below the axis, and then gives the refractions independently of the measure of time. You therefore approved the resolution I had formed of confining myself to this instrument, and of not quitting London till I could carry with me a circle of five feet, which Mr. Ramsden was constructing for the observatory of Palermo: as soon as mine is finished he promises to put in hand that destined for Paris. He then hopes to finish that for Dublin, which is twelve feet, and which you saw in an advanced state; but a circle of 7 or 8 feet is sufficient to give precision within half a second, as in the zenith sector, which is employed for the most rigorous observations in regard to the figure of the earth.

You remarked with the greatest satisfaction the ingenious manner in which the axis is supported, that it may have no friction on the pivots, and especially Mr. Ramsden's new invention for rendering the axis perfectly horizontal, by means of a thread and plummet, which is however without the machine; and you had the pleasure of seeing that inventive genius exercise his talents on this new problem, and solve it in the completest manner. As his talents have been exercised in a wide field, he has collected in his shops workmen in all professions which relate to the construction of mathematical and philosophical instruments, that he might have every thing made under his own inspection. The same workman is always confined to the same branch, and by these means acquires the utmost degree of correctness. But notwithstanding this perfection, which ought to enable Mr. Ramsden to make a fortune, he sells his instruments cheaper than any other artist in the same line at London: the difference is sometimes a third. Though he has nearly 60 workmen in his employment he is not able to execute all the orders which he has received from every part of the world, and you yourself have experienced how difficult it is to obtain instruments from him.

No person can be more reasonable, more attentive to business, or more indifferent for pleasure or for riches than Mr. Ramsden: he is exceedingly frugal in his manner of life; and, unless provoked, no one can be more polite, milder, or more complaisant. I hope, sir, you will publish with pleasure this tribute of my gratitude to a man of uncommon talents, whom you esteem as much as I do, and who, in his turn, has conceived for you a real attachment.

XLVI. *Some Details respecting the Voyage of the two French Corvettes, Le Geographe and Le Naturaliste, sent out under the Command of Captain* BAUDIN *for the Purpose of making Discoveries.*

THESE two corvettes sailed from the north-west port of the Isle of France on the 25th of April 1801, and on the 27th of May discovered the land of New Holland in lat. 34° 36′, long. 111° 44′; the land they saw was that called Leeuwin's Land, which forms the south-west extremity of New Holland, and which the charts indicate as little known.

Preparations were then made for exploring the country and the two corvettes were employed in this service from the 27th of May to the 14th of June. As these were the first geographical operations made on board these vessels, it appears that they are not very correct; and captain Baudin has sent home no account of them, nor any of the charts which he caused to be constructed. He announces that on his return from Port Jackson he proposes to revist Leeuwin's Land, and to repeat the operations undertaken, in order to construct a chart of Bai du Geographe, which he found in that part.

Among the charts brought home by the Naturaliste is one of the same coast of Leeuwin's Land, executed on board the Naturaliste by C. Faure: it contains an extent of coast from lat. 34° 23′ to 32° 13′, but coarsely delineated and without any details.

After these operations the two corvettes were separated, and did not again meet till they arrived at the Island of Timor; but both of them sailed along the coast of the Land of Endracht, which lies to the north of Leeuwin's Land.

Captain Baudin entered the Bay of Seals on the 27th of June, and remained there till the 13th of July. Here Bernier, the astronomer, made some observations, which fix the longitude of the northern point of Barren Island at 109° 13′ 46″. The course of the Geographe in this bay and on the coast to the north of it is traced out on a chart constructed by C. Boulanger, one of the engineers belonging to the expedition. This chart, however, is only a copy of a Dutch chart given to captain Baudin before his departure, and which C. Boulanger corrected by the results of astronomical observations.

Captain Baudin observes in his memoir, that his first operations in regard to this coast, and that of De Witt's and of Leeuwin's Land, were not satisfactory; and he purposes to repeat them on his return from Port Jackson. He has therefore sent home none of the charts of the early part of his voyage along these coasts. Among the charts which the Naturaliste has brought home there is only one small chart of a part of the coast of New Holland, where the Geographe landed; and which is supposed to be on the coast of De Witt's Land, or of the north-west. This chart, constructed by Rousard, an officer of the marine engineers, is merely a sketch taken at sight, and may give some idea of the observations made on board the Geographe; though, as captain Baudin announces, it contains only fragments of the coasts. He says he transmitted to the minister of the marine, in a letter dated Timor, October 6, 1801, some details respecting his navigation on the western coasts of New Holland from Leeuwin's Land, and that he then announced that they were by no means satisfactory. He arrived at Timor, and entered Coupang Bay on the 22d of August 1801.

The Naturaliste being separated from the Geographe on the coast of Leeuwin's Land proceeded to the Island of Rottenest, which had been agreed on as the first place of rendezvous in case of separation. The captain explored that island as well as another in the neighbourhood, and which is not marked in the charts: he called it Isle-aux-Ours. He examined at the same time Swan's River, on the coast of New Holland, opposite to the Isle of Rottenest: some boats were ordered to proceed up that river as far as they possibly could without danger; and a chart of it was constructed, which is inserted in the journal of C. Hamelin, the commander of the Naturaliste.

The same corvette sailed along the western coast of New Holland from lat. 32° south, which is that of the Isle of Rottenest as far as the Bay of Seals, or of Dirk Hartoge; and a chart of that coast was constructed on five large sheets. This chart, however, is merely a sketch made at sight, and can serve only to give a general idea of the course. It contains none of those details exhibited in the Dutch chart of the same coast.

The Naturaliste having remained some time in the Bay of Seals, waiting for the Geographe, took that opportunity of exploring this bay, and the result is a chart very different from any before published. It deserves consideration, and may be useful to navigators who in future may touch at this bay, in the different gulphs of which they will find resources of which no idea was before entertained.

From the Bay of Seals the Naturaliste steered a direct course for the Island of Timor, without following the coast of Endracht's Land or that of De Witt's, and arrived in Coupang Bay on the 20th of September, having left the Bay of Seals on the 3d. Captain Baudin has added to the charts which he sent home, copies of some Dutch charts of the islands of the Indian Archipelago, which he made during his stay at Timor.

The two corvettes left Timor in company on the 13th of November 1801, and proceeded to D'Entrecasteaux's channel on the south-east coast of Van Diemen's Land, where they arrived on the 13th of January 1802. They explored every part of that channel with the greatest care; and captain Baudin announces that they found nothing in it to be rectified. "It is hardly possible," says he, "to find any work more correct, or better executed, than that of the geographers who have made us acquainted with these places for touching at; and we shall be well satisfied if we hear the navigators who succeed us give the same account of our labours, in regard to the coasts which no one ever visited before us."

Captain Baudin only observes that the land called Tasman's Land, in the chart of D'Entrecasteaux's channel, is not an island; and that it is joined to Van Diemen's Land by an isthmus of about 80 or 100 paces in breadth. It is to be observed that D'Entrecasteaux's boats did not proceed so far in this channel as those of captain Baudin; and the observation he makes in regard to Tasman's Land is correct. He observes also that the Bay of Frederick Hendrick is not in the place where it is laid down in D'Entrecasteaux's chart, and he gives a long dissertation on this subject. D'Entrecasteaux was not in that part, and the Bay of Frederick Hendrick was given from old charts.

After ascertaining the correctness of the chart of D'Entrecasteaux's channel, captain Baudin did not think it necessary to make a new one, as he had nothing to add to it. He confined himself to the construction of one of Frederick Hendrick's Bay, every part of which he explored.

In the chart of Van Diemen's Land, constructed in 1798 and 1799 by captain Flinders, and which was engraved at the depôt in order that captain Baudin might have a sufficient number of copies, the eastern coast of Van Diemen's Land was traced out in a vague manner, as countries little known are in general. Captain Baudin was therefore charged to explore it in a complete and correct manner; and this operation he undertook on leaving the Bay of Frederick Hendrick. Among the charts he has sent home there is one of the Island Maria, the coast of which was explored and correctly traced out; another of the coast of Van Diemen's Land between Maria and Schouten's Islands; and a third of Schouten's Islands and the adjacent coast. It is seen by the last-mentioned chart that there is only one Schouten's Island, instead of the five laid down in captain Flinders' chart. The rest form a long peninsula joined to Van Diemen's Land by an isthmus.

There is a chart also of the remainder of the eastern coast of Van Diemen's Land, from Cape Pelé in lat. 42° 8′ to Swan's Island in lat. 40° 42′. This chart is the result of the course followed by C. Boulanger, who had been sent on shore to explore the country, and who was not able to return on board. Having waited some days for a boat to come and fetch him, he resolved to proceed along the coast which he supposed the corvettes would sail along; and he fortunately arrived at Swan's Island and Banks's Straits, where he found an English ship, the captain of which promised to carry him to Port Jackson; but some days after he fell in with the Naturaliste, which took him on board.

The two corvettes had been separated some time, and were endeavouring to fall in with each other. Both of them proceeded to Van Diemen's Land, but without meeting. They visited Dalrymple River, which is situated about the middle of that coast, and another called North River; both proceeded also to the coast of New Holland, which forms the northern coast of Basse's Strait. The Naturaliste explored the coast from Cape Wilson to Port Western. That port also was visited, and plans were made of both. After this operation the Naturaliste made for Port Jackson, in hopes of finding there the Geographe.

Captain Baudin, after visiting different parts of Basse's Straits in hope of meeting with the Naturaliste, determined to explore the southern coast of New Holland, which was entirely unknown. He first proceeded to Cape Wilson, from which he took his point of departure, and directed his course west, following the coast to the distance of fifteen degrees of longitude. About the middle of his course he fell in with captain Flinders, who had left England eight months after him, and who was charged to make the same researches as captain Baudin on all the coasts of New Holland. He had cruised along the southern coast from Leeuwin's Land to the point where he fell in with captain Baudin, and two days before meeting him had discovered a large and beautiful island, to which he gave the name of Kangaroo Island. This island is situated in lat. 35° 50′ south, and long. 135° 4′, and appears to be about thirty leagues in extent from east to west. Captain Flinders had passed through the channel which separates it from the land, and had seen none of the southern part.

Captain Baudin, continuing his course, fell in with this island, which he found such as it had been described by captain Flinders: like him he passed on the north side, and did not see the southern part; but on the north side he found two gulphs which proceeded a great way inland, and which he entered, to explore the whole extent of them: he however could see only one side, because the other was filled with sand-banks and shoals, which did not permit the vessel to approach the land.

When he came out of these gulphs he continued his course towards the west as far as the Isles of St. Peter and St. Francis, which were nearly the term of the researches of D'Entrecasteaux on that coast; he then proceeded to the south-east to reach Port Jackson, where he found the Naturaliste.

This discovery of captain Baudin is highly important, as it completes the survey of the southern coast of New Holland, which is entirely owing to France. As yet we can form no opinion in regard to the degree of correctness with which it has been explored, because C. Baudin has sent home only a part of the chart which he constructed, and as the chart itself is only a sketch: he has added to it a chart which only indicates his course, with the soundings along that coast; and he promises to send the other part of the coast by the first opportunity.

Captain Baudin has joined to these charts twelve views well executed, and by which he has endeavoured to give an idea of the nature of the country. They relate only to Leeuwin's Land; but he promises others of the same kind in regard to every part of New Holland which he has visited.

On his departure from Port Jackson he purposed,

1st, To explore King's Island lately discovered in Basse's Straits, and situated to the north-west of Hunter's Isles.

2d, The large island called Kangaroo Island, the southern part of which is unknown.

3d, The two large gulphs situated to the north of Kangaroo Island, and which he can examine in every part by means of the Kasuarina, a small vessel which he procured at Port Jackson, and which draws little water.

4th, The northern part of the Islands of St. Peter and St. Francis, and where the discoveries he has made unite with those of D'Entrecasteaux.

5th, Leeuwin's Land, and that of Endracht, which he has already seen, but not in a satisfactory manner.

6th, De Witt's Land, where he knows he shall experience great difficulties, but where he hopes to find also interesting objects.

7th, In the last place, the Gulph of Carpentaria, which will be the boundary of his researches.

The two vessels sailed from Port Jackson, Nov. 18, 1802, twenty-five months after their departure from France, and on the 6th of December anchored in the Bay of Sea Elephants in the eastern part of King's Island in lat. 39° 51', long. 141° 34'.

Two days after, captain Hamelin, having received his ultimate orders, separated from the Geographe and the Kasuarina in order to proceed to France.

When on the point of sailing, an English galliot anchored near them. This vessel had been dispatched for the purpose of exploring Port Philips on the south-west coast of the Bay of Frederick Hendrick, in Van Diemen's Land, and the river in the north of the same land very near D'Entrecasteaux's channel; to construct charts of these places; and to wait at the latter for the arrival of the Porpoise, which was to carry thither the troops necessary for forming a settlement. They learned also by this vessel, that the Lady Nelson brig, which sailed from Port Jackson along with the Investigator, had returned thither, having lost all her anchors, and been obliged to make one of wood. They had separated from captain Flinders on the 2d of October

1802 in lat. 20° north, very near the coast. The latter had also lost three anchors, and had several times struck; as had been the case with the Lady Nelson, which by these means had broken her sliding keel. Captain Flinders continued his voyage to the Gulph of Carpentaria.

XLVII. *Observations on the Chemical Nature of the Humours of the Eye. By* Richard Chenevix, *Esq. F.R.S. and M.R.I.A.*[*]

The functions of the eye, so far as they are physical, have been found subject to the common laws of optics. It cannot be expected that chemistry should clear up such obscure points of physiology as all the operations of vision appear to be; but some acquaintance with the intimate nature of the substances which produce the effects cannot fail to be a useful appendage to a knowledge of the mechanical structure of the organ.

The chemical history of the humours of the eye is not of much extent. The aqueous humour had been examined by Bertrandi; who said that its specific gravity was 975, and therefore less than that of distilled water. Fourcroy, in his *Système des Connoissances Chimiques,* tells us that it has a saltish taste; that it evaporates without leaving a residuum; but that it contains some animal matter, with some alkaline phosphate and muriate. These contradictions only prove that we have no accurate knowledge upon the subject.

The vitreous humour is not better known. Wintringham has given its specific gravity (taking water at 10000) as equal to 10024; but I am not acquainted with any experiments to investigate its chemical nature.

We are told by Chrouet, that the crystalline lens affords, by destructive distillation, fœtid oil, carbonate of ammonia, and water, leaving some carbon in the retort. But destructive distillation, although it has given us much knowledge as to animal matter in general, is too vague a method for investigating particular animal substances.

I shall now proceed to mention the experiments I have made upon all the humours. I shall first relate those which were made upon the eyes of sheep (they being the most easily procured), and shall afterwards speak of those of the human body, and other eyes. I think it right to observe, that all these eyes were as fresh as they could be obtained.

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[*] From the *Transactions of the Royal Society for* 1802.

SHEEP'S

SHEEP'S EYES.

Aqueous Humour.

The aqueous humour is a clear transparent liquid, of the specific gravity of 10090*, at 60° of Fahrenheit. When fresh, it has very little smell or taste.

It causes very little change in the vegetable re-active colours; and this little would not, I believe, be produced immediately after death. I imagine it to be owing to a generation of ammonia, some traces of which I discovered.

When exposed to the air, at a moderate temperature, it evaporates slowly, and becomes slightly putrid.

When made to boil, a coagulum is formed, but so small as hardly to be perceptible. Evaporated to dryness a residuum remains, weighing not more than 8 per cent. of the original liquor.

Tannin causes a precipitate in the fresh aqueous humour both before and after it has been boiled, and consequently shows the presence of gelatine.

Nitrate of silver causes a precipitate, which is muriate of silver. No metallic salts, except those of silver, alter the aqueous humour.

From these and other experiments it appears that the aqueous humour is composed of water, albumen, gelatine, and a muriate, the basis of which I found to be soda.

I have omitted speaking of the action of the acids, of the alkalis, of alcohol, and of other re-agents, upon this humour. It is such as may be expected in a solution of albumen, of gelatine, and of muriate of soda.

Crystalline Humour.

To follow the order of their situation, the next of the humours is the crystalline.

This differs very materially from the others.

Its specific gravity is 11000.

When fresh, it is neither acid nor alkaline. It putrefies very rapidly. It is nearly all soluble in cold water, but is partly coagulated by heat. Tannin gives a very abundant precipitate; but I could not perceive any traces of muriatic acid when I had obtained the crystalline quite free from the other humours. It is composed, therefore, of a smaller proportion of water than the others, but of a much larger proportion of albumen and gelatine.

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* All these specific gravities are mean proportionals of several experiments. The eyes of the same species of animal do not differ much in the specific gravity of their humours.

Vitreous

Vitreous Humour.

I pressed the vitreous humour through a rag, in order to free it from its capsules; and in that state, by all the experiments I could make upon it, I could not perceive any difference between it and the aqueous humour, either in its specific gravity (which I found to be 10090, like that of the other,) or in its chemical nature.

M. Fourcroy mentions a phosphate as contained in these humours; but I could not perceive any precipitation by muriate or nitrate of lime; nor did the alkalis denote the presence of any earth, notwithstanding M. Fourcroy's assertion of that fact.

HUMAN EYE.

I could not procure a sufficient quantity of these, fresh enough to multiply my experiments upon them. However, by the assistance of Mr. Carpue, surgeon to his majesty's forces, I fully convinced myself that the humours of the human eye, chemically considered, did not contain any thing different from the respective humours of the eyes I had examined. The aqueous and vitreous humours contained water, albumen, gelatine, and muriate of soda; and the crystalline humour contained only water, albumen, and gelatine. The specific gravity of the aqueous and vitreous humours I found to be 10053, while that of the crystalline was 10790.

EYES OF OXEN.

I found the eyes of oxen to contain the same substances as the respective humours of other eyes. The specific gravity of the aqueous and vitreous humours is 10088, and that of the crystalline 10765.

What is particularly worthy of notice is, that the difference which appears to exist between the specific gravity of the aqueous or vitreous humour and that of the crystalline, is much greater in the human eye than in that of sheep, and less in the eye of the ox. Hence it would appear that the difference between the density of the aqueous and vitreous humour and that of the crystalline, is in the inverse ratio of the diameter of the eye, taken from the cornea to the optic nerve. Should further experiments show this to be a universal law in nature, it will not be possible to deny that it is in some degree designed for the purpose of promoting distinct vision.

In taking the specific gravity of the aqueous and vitreous humours, no particular precaution is necessary, except that they ought to be as fresh as possible. But the crystalline humour is not of an uniform density throughout; it is therefore essential that attention be given to preserve that humour entire for this operation. I found the weight of a very fresh crystalline of an ox to be 30 grains; and its specific gravity was, as I before stated, 10765. I then pared away all the external part, in every direction, till there remained but 6 grains of the centre; and the specific gravity of these 6 grains I found to be 11940. From this it would seem that the density increases gradually from the circumference to the centre.

It is not surprising that the crystalline humour should be subject to disorders, it being wholly composed of animal matter of the most perishable kind. Fourcroy says that it is sometimes found osseous in advanced age. Albumen is coagulated by many methods; and, if we suppose that the same changes can take place in the living eye as in the dead animal matter of the chemists, it will be easy to account for the formation of the cataract; a disorder which cannot be cured but by the removal of the opake lens. If a sufficient number of observations were made respecting the frequency of the cataract in gouty habits, some important conclusions might be drawn as to the influence of phosphoric acid in causing the disorder, by the common effect of acids in coagulating albumen.

XLVIII. *On a Change observed in the Colour of Prussian Blue by coming in Contact with Iron.* By THOMAS GILL, *Esq.*

To Mr. Tilloch.

SIR,

HAVING frequently discoursed with you on the various colours producible from iron, I make no apology for troubling you with this letter. Happening this morning to be grinding some Chinese blue colour with parchment, size, and water, I noticed that a knife I employed in mixing it became of a greenish tinge: upon this I resolved to prosecute the experiment, when the following singular result was obtained:—After spreading a little of the colour upon the blade of the knife with a camel's hair pencil, I diluted and took off enough of the colour to form a tint upon paper, and as soon as that was laid on proceeded to take off a second, a third, &c. portion, without adding more colour to the knife, until I had obtained thirty-six different tints, each varying in colour from the original blue. The changes were to a greenish blue, a green, an olive green, a yellowish green, a yellow, and so on to a buff colour, where I ceased from further prosecuting the search.

I then tried the same experiment with a cake of Newman's Prussian blue; and, repeating the process quicker, I obtained from the same original quantity of colour first laid on the knife no less than eighty-six different tints, each varying in colour as in the former experiment.

Indigo treated in a similar manner did not change its tint in the least degree under the process.

This discovery may lead to very important consequences in the theory of colour-making; and it furnishes some curious facts, namely, that the Chinese are acquainted with the art of making a colour similar to the Prussian blue; and that indigo should be preferred to the Prussian blue when we wish our colours to be durable, and not subject to change on coming in contact with iron: it should also serve as a caution never to employ the pallet knife or iron in any form in treating Prussian blue.

It would appear from the result of this experiment, either that the Prussic acid, one of the constituent principles of Prussian blue, is not saturated in that combination with iron, but is still able to exercise an action on that metal in its metallic state, or else that the acid itself becomes decomposed, giving up its oxygen to the iron, and producing the ochrey tint which changes gradually the blue to green.

We are possessed of one tinge only of green colour produced from iron, namely, Prussian green, with the process for obtaining which I am unacquainted.

Allow me before concluding to mention another fact with which some of your readers may not be acquainted. The superiority of the Chinese colours over those of Europe is owing chiefly to their being ground much finer, and being mixed with size instead of gum, which prevents their having any gloss.

XLIX. *On the Marine Spencer, invented by* KNIGHT SPENCER, *Esq.*

IN our last Number we gave a short description of this invention. To shew its great utility, we need only to mention a few of the many possible cases in which it may be employed with advantage. It may be proper to premise, that being made of an article at all times to be had in abundance, and of no present value whatever, and being very simple in its construction, it may be made by old people in poor-houses, by Greenwich pensioners, &c. who could not be better employed, and therefore may be afforded at a price within the reach of every foremast man.

It would be a desirable appendage to the life-boat in cases when the whole crew could not be taken in at once. Any number of persons furnished with these might be floated ashore attached to the boat with small cords. In cases of shipwreck where no life-boats are at hand, doubtless many lives might be saved by the marine spencer; and we may presume that seamen will act with more effect in time of danger when they know they have the certain means on board of getting safe to shore when every exertion is found in vain to save the vessel.

In cases of persons falling overboard, any one not acquainted with swimming, if furnished with a marine spencer, might safely leap after them and keep them from sinking until a boat could be launched.

In cases of fire at sea, or in harbours, the most beneficial effects may be expected from its use; and had the Queen Charlotte, which was burnt at sea, been furnished with only one hundred of the marine spencers, many hundreds of lives might have been saved, as in moderate weather one will keep three or four people from drowning.

A corner in every seaman's locker in the royal navy could not be better employed than in containing one of these; and if the men were occasionally exercised in the use of them, so as to render them familiar, the most important consequences might attend it.

Most certainly the life-boat is the best means yet discovered of preserving lives in cases of shipwreck; but as the smaller description of merchant vessels could not afford the expense of one, these marine spencers are certainly the next best means of preserving the lives of the crews in such cases.

L. *List of interesting Curiosities collected by Mr.* Clarke *during his Travels in the East.*

To the Editor of the Philosophical Magazine.

SIR,

As various inaccurate accounts have appeared concerning the acquisitions to literature and science which have been brought home by Mr. Clarke, of Jesus college, Cambridge, from his extensive travels, I take the liberty of transmitting to you a list, though brief, that may however be relied on; and am, sir, Yours, &c.

From Patmos.

1. The works of Plato, most beautifully written upon vellum, in folio. The scholia in minute capitals. The colyphon proves that it was written by John the calligraph for Arethas, deacon of Patræ, for 13 Byzantine nummi, in the 14th year of the indiction and the 6404th of the world (A. C. 896), in the reign of Leo, son of Basilius.

2. Lexicon of St. Cyril of Alexandria.

3. Greek poetry, accompanied by antient Greek musical notes.

4. Ditto, ditto.

5. The works of Gregory of Nazianzum.

From Naxos.

Copies of the Gospels, in capitals, of very antient date.

From Mount Athos.

1. The orations of Demosthenes.

2. The works of ten Athenian orators, some of which not hitherto known.

From Constantinople.

1. The works of Dionysius the areopagite, with a curious and learned commentary, written on vellum, in folio.

2. Complete copy of the Gospels, written in the eighth century.

3.
4. } Various copies of the Gospels, and of the Epistles
5. } and Acts of the Apostles of different dates.
6.

7. The works of Philip the hermit.

8. The dialogues of Theodore the Syracusan.

9. A work on the Greek Grammar.

10. } The writings of commentators on the Gospels;
11. } and the works of the earliest fathers of the
12. } church.

13. Very antient copy of the Evangelistarium of the Greek church.

14. Ditto ditto.

15. A work of Philes on animals.

The Plato which professor Porson calls a *monument of literature* was, on its arrival, supposed to be the oldest Greek manuscript extant with an express date, as the MS.

six years older, noticed by Montfaucon in his Paleographia, p. 42, has disappeared; and that of Euclid, mentioned by Dorville on Chariton, p. 49, 50, was supposed to have been lost. The latter, however, which was written by the same hand as the Plato, the year before it, has been since found, and purchased, with the rest of Dorville's collection, by Dr. Raine and Mr. Banks. Professor Porson, who, in copying the scholia on Plato, has thence discovered passages from Greek plays and from poets that were lost, will, it is hoped, allow the world to profit by the fruits of his industry and unrivalled erudition.

To the above MSS. in the Greek language should be added, in Hebrew, the Bible of the Karæan Jews; in Coptic, the Gospels; in Arabic, many volumes of history, poetry, &c.; in Abyssinian or Æthiopic, the Gospels, &c.; and in Persian, some unpublished works of Sadi and other writers.

The rest of the collections consist of, 1. Antique monuments from Saïs, in Egypt, the ruins of which city were discovered for the first time by Messrs. Clarke and Crips. Various other antiquities from the upper parts of this country.

2. Medals and vases from all parts of Greece.

3. Sculpture and inscriptions from the Cimmerian Bosporus, the Crimea, the shores of the Euxine, the plain of Troy, the Greek islands, and the Grecian continent.

4. Minerals from all the countries passed through between the 69th and 29th degrees of north latitude, including many new substances.

5. Plants, seeds, &c. from the same regions. These include several new species, and a new genus. Also the herbarium of professor Pallas, which comprises all his botanical discoveries in Siberia; and of Dr. Noezen, of Sweden, abounding in arctic plants.

6. Original maps and charts not yet published. Among these are the great chart of islands and seas between Kamschatka and America, the result of Billing's voyage; the map of the countries between the Black and Caspian seas, on a very large scale; the Crimea; charts of the Russian ports; and a map of the plain of Troy, now engraving by Arrowsmith.

7. Models, implements of husbandry, customs of different nations. Animals, insects, &c.

8. A large collection of drawings from nature, for the purpose of illustrating the account of the journey, if it should ever be published.

LI. *Notices respecting New Books.*

Philosophical Transactions of the Royal Society of London for the Year 1803. Part I.

This valuable publication consists of the following papers: —1. The Bakerian Lecture. Observations on the Quantity of horizontal Refraction; with a Method of measuring the Dip at Sea. By William Hyde Woollaston, M.D. F.R.S. —2. A Chemical Analysis of some Calamines. By James Smithson, Esq. F.R.S.—3. Experiments on the Quantity of Gases absorbed by Water at different Temperatures and under different Pressures. By Mr. William Henry.—4. Experiments and Observations on the various Alloys, on the Specific Gravity, and on the comparative Wear of Gold; being the Substance of a Report made to the right honourable the Lords of the Committee of the Privy Council, appointed to take into Consideration the State of the Coins of this Kingdom and the present Establishment and Constitution of His Majesty's Mint. By Charles Hatchett, Esq. F.R.S.—5. Observations on the Chemical Nature of the Humours of the Eye. By Richard Chenevix, Esq. F.R.S. and M.R.I.A.—6. Account of some Stones said to have fallen on the Earth in France; and of a Lump of native Iron said to have fallen in India. By the Right Honourable Charles Greville, F.R.S.—7. Observations on the Structure of the Tongue; illustrated by Cases in which a Portion of that Organ has been removed by Ligature. By Everard Home, Esq. F.R.S.—8. Observations of the Transit of Mercury over the Disc of the Sun; to which is added, an Investigation of the Causes which often prevent the proper Action of Mirrors. By William Herschel, L.L.D. F.R.S. —9. An Account of some Experiments and Observations on the constituent Parts of certain astringent Vegetables, and on their Operation in Tanning. By H. Davy, Esq. Professor of Chemistry in the Royal Institution.—10. Appendix to Mr. Henry's Paper on the Quantity of Gases absorbed by Water at different Temperatures and under different Pressures.—Appendix. Meteorological Journal kept at the Apartments of the Royal Society.

An Essay on the Law of Patents for new Inventions: to which are prefixed two Chapters on the general History of Monopolies, and on their Introduction and Progress in England to the Time of the Inter-regnum: with an Appendix, containing Copies of the Caveat, Petition, Oath, and

and other Formulæ; *with an arranged Catalogue of all the Patents granted from the* 1*st of January* 1800 *to the present Time. By* JOHN DYER COLLIER. 1803. Longman and Rees. Royal 8vo.

It is the principal design of our periodical work to give some account of the progress of science and philosophy; and it will not appear to our readers unconnected with this object if we take notice of a publication which is the first on the subject, and the professed intention of which is to enable men of superior attainments to reap the produce of their talents. In the 21st of James the statute of monopolies was passed, by which the exclusive privilege of the sale of any important discovery for fourteen years was assigned to the ingenious inventor. To the comments on this act of parliament, and the cases that have been tried upon it, the essay to which we are now adverting is devoted; and full instructions are given to enable the candidate for the royal grant to obtain his patent, and to protect himself from the evil consequences of its infringement.

The list of patents alluded to in the title page shows how extensively the private studies of the philosopher are at this day rendered subservient to the purposes of commerce and the arts, on which the wealth and prosperity of the country so essentially depend; and this important application of the pursuits of the scientific student is in a great degree to be attributed to the reward thus assigned for his labours by the wisdom of public institution. Yet he is often disappointed of this honourable remuneration from a variety of causes, of which it is extremely material that he should be apprised; we shall therefore extract a few observations from this view of the Law of Patents, which will inspire him with caution, and tend to show how material it is to him now and then to descend from the elevated sphere of science to the lower world we inhabit, and the vulgar affairs in which it is conversant:—

"It is a melancholy reflection, that of the numerous patents which pass the great seal, and, of course, subject the grantee to considerable expense, very few become sufficiently popular and notorious to reward him for the application of his time, labour, and talents. We mention this as a caution to men of enterprise and ability, that the sanguine temperament to which they are commonly subject may not lead them to colour too highly the remote prospect. A profound observer of human nature has told us, that before we arrive at the age of forty we usually make an anticipation too favourable of the future; and the spirit and energy which supply the inventive talent are peculiarly liable to this species of self-deception.

" It will be seen in the catalogue, that a great variety of patents have been extended to objects connected with those branches of the trade and manufactures of this country which are of the first importance to commercial prosperity. No less than thirteen patents in a very short period have been assigned for improvements on the steam-engine, most of which would have been probably suppressed if the legal discussion on the patent of Messrs. Boulton and Watt, for an improvement on the same invention, had not completely decided the question on the competency and validity of patents for improvements, whenever those improvements can be deemed material and useful.

" The mere inspection of the following list will show how necessary it is that patentees should be informed of the rules laid down in the Law of Patents. It will be seen how great a portion of the grants appear by their titles to be extended not to any *piece of mechanism, utensil, or manufacture,* but to a *process, method, or principle,* for which no patent can be valid. It is, however, some consolation to reflect, that what is stated as such is often the application of a method, process, or principle, in some substantial form, for which a patent can be maintained. It is now understood that where this construction can be given it will be applied in the most beneficial sense for the patentee; and that no advantage will be taken of minute verbal criticisms to render the royal grant nugatory, and to disappoint the inventor of his equitable reward."

Useful Hints to those who are afflicted with Ruptures; on the Nature, Cure, and Consequences of the Disease; and of the empirical Practices of the present Day. By T. SHELDRAKE.

For the present work the public is indebted to the author of a very useful work entitled " A Practical Essay on the Club-foot and other Distortions of the Legs and Feet of Children," which has been well received. The present appears to us to be equally useful, and well deserving of the attention of such as are afflicted with ruptures, or have relations in that situation.

LII. *Pro-*

LII. *Proceedings of Learned Societies.*

ACADEMY OF SCIENCES AT STOCKHOLM.

In the sitting of the 13th of April M. Melanderhjelm read an account of the expedition recommended by his Swedish majesty in the beginning of the year 1801, for the purpose of solving in a decisive manner the problem in regard to the figure of the earth, which is of so much importance to astronomy. The king for the accomplishment of this object appropriated the sum of 5000 rix-dollars, and appointed two members of the academy, Messrs. Ofverbom and Svanberg, to perform the requisite operations. These gentlemen immediately proceeded to the neighbourhood of the polar circle to prepare every thing necessary for the operations: such as observatories for the two remotest stations; signals on every convenient point in the extent of twenty-two Swedish miles; and a series of triangles as convenient as the situation of the country would admit. After making these preparations they returned again to Stockholm to receive one of Borda's circles, constructed at Paris by Lenoir under the inspection of Delambre, together with two metres, and a Peruvian toise of iron which the French National Institute presented to the Swedish Academy of Sciences. These articles, however, did not arrive before the beginning of December at Stockholm, where Messrs. Ofverbom and Svanberg made preparations for their new journey. But as the academy foresaw that two persons would not be sufficient to superintend all the individual parts and details of this operation, which required so much accuracy, Dr. Holmquist of Upsal and Mr. Palander of Abo were appointed to unite themselves to Messrs. Ofverbom and Svanberg, and to assist them in their labour. These gentlemen set out together in the beginning of the year 1802; and immediately on their arrival at Tornea prepared the iron rods which were destined for measuring the base line, and made ready for the measurement. The place on the river Tornea used as a base in 1736 was again made choice of for the same purpose. Eight weeks were employed in this operation, from the middle of February to the middle of April. The centigrade thermometer often stood at 25, 26, and even 30° under the freezing point. It was necessary to measure with the greatest accuracy all the angles belonging to the series of triangles—a labour they were obliged to defer till summer, which in that climate is exceedingly short. The number of these angles amounted to about 80; and it was requisite that the measurement of each should be repeated at least thirty times, in order to render as insignificant as possible the errors in the graduation, from which no instrument is entirely free. This was not only done, but many of the angles were measured sixty and even eighty times; and at the end of August the operations were so far advanced, that observations were made of the pole-star, by which the extent of the arc of the meridian measured could be determined. The last southern point was at Malörn, a small island in the archipelago of Tornea, where the gentlemen employed arrived in the beginning of September, and continued their observations on the latitude of this place till the end of October. The observation of the zenith distance of the pole-star was repeated 400 times.

In order to avoid, in determining the absolute latitude of the first and last stations, the uncertainty which might arise from the declination of the pole-star, its zenith distance at its inferior passage of the meridian was observed 88 times. When all these operations were performed, the expedition was considered to be at an end, and the observers returned home. Messrs. Ofverbom and Svanberg, in order to gratify the curiosity of astronomers, who are interested in the result of this new measurement, immediately on their return to Stockholm undertook a calculation; according to which it appears, that in the latitude of 66° 20′ 11·83″ the length of a degree of the meridian is 57209 toises, or 196 toises less than that given by the measurement of Maupertuis. This without doubt will be an agreeable novelty to those who are acquainted with the importance of this question, and its relation to the grand phænomena of nature. If this result be compared with that of Bouguer's measurement at the equator, we obtain 1-313th for the flattening of the earth at the pole; and the northern degree of the meridian, which hitherto has been so different from all other results deduced from the precession of the equinoxes, experiments with the pendulum, calculations of the parallax compared with the ellipsoid form of Jupiter, no longer forms any exception, but coincides nearly with that which the greatest mathematicians of the last century were obliged to admit. Messrs. Ofverbom and Svanberg, however, do not consider the above result as absolute and definitive, because improvements may still be made in their provisional calculation, which perhaps may amount to 20 toises; and this is the more possible, as one second of variation will occasion a difference of 16 toises.

All that we as yet think ourselves authorised to assert is, that the flattening of the earth cannot at any rate be greater than 1-300dth.

LIII. *Intelligence, and Miscellaneous Articles.*

AËROSTATION.

Garnerin's 33d Ascent.

On the 1st of July Garnerin, accompanied by Mrs. Garnerin, ascended in his balloon at Petersburgh in presence of their imperial majesties, a great number of persons of distinction, and an immense concourse of spectators. This ascent, being Garnerin's 33d, took place in the garden of the first corps of cadets, where a tent had been erected for their majesties, who repaired thither at six o'clock in the evening. After they had examined the balloon, Garnerin presented to the emperor a small one filled with inflammable air, which his majesty let off himself. Other balloons of a similar kind were then presented to their majesties, and to his royal highness the grand duke Constantine, who let them off in like manner, in order to ascertain the direction of the wind. The state of the atmosphere was exceedingly favourable. At half after seven the aërial travellers entered the car. The signal was given, and the balloon ascended in a majestic manner. At eight o'clock it began to descend, and the aëronauts reached the earth in safety on the little Ochta at some distance from Petersburgh.

Mr. Robertson's Balloon.

Mr. Robertson about the middle of July ascended in a balloon at Hamburgh, accompanied by his friend M. Lhoest. The following are the details published by Mr. Robertson himself:

"On the day on which I determined to make my ascent, I resolved to rise to as great an elevation as my health and strength would permit. M. Lhoest, my friend and companion, having determined to accompany me, we took, besides provisions, as large a quantity of ballast as the car could conveniently contain. After hovering some time over the city of Hamburgh, we resolved to ascend higher; and, having thrown out some ballast, we rose to such a height that the elasticity of the air distended our balloon so much, that we were obliged to open the valve and suffer a certain quantity of gas to escape. It issued from the balloon with a loud noise. The tension of the balloon being considerably lessened, we threw out more ballast, and ascended till it was almost impossible for us to endure the great cold which we experienced. My teeth chattered; my pulse beat with violence; my veins swelled; and the blood issued from my nose. The state of my friend was different: his head seemed to be swelled, and he could not keep his hat on. We experienced also a great numbness, which inclined us to sleep. Not being able any longer to endure this state, we resolved to descend; which we did slowly, for half an hour, with our watches in our hands. About half after twelve we arrived over Badenburg near Winsen on the Luhe, where we purposed to get out; but the inhabitants, taking us for spectres, fled with the utmost consternation, carrying with them their cattle, and sought shelter in their houses, the doors of which they shut.

My friend having observed to me that this terror might be injurious to us, and induce these people to fire at us, we threw out a part of our remaining ballast, and, having again ascended, continued our voyage till two in the afternoon. We at length descended near Wichtenbeck, on the road to Zell, where commissioners Weyhe and M. Raven were so kind as to give us every assistance in their power, and caused us to be conducted with our balloon to the first post, about five leagues distant from the place of our descent. We arrived at Wichtenbeck in perfect health, after having passed over twenty-five French leagues in five hours.

By some further particulars which Mr. Robertson has since published at Hamburgh, it appears that he rose to the height of 2600 toises. " When the balloon rose (says he) the thermometer was at 28 inches. At 11 o'clock the machine, which had not been entirely filled, became so dilated that the inflammable air issued with noise from the lower tube. As this aperture was not sufficient, I was obliged to open the upper valve. It remained open nearly a quarter of an hour, during which time the balloon ascended in a perpendicular direction: at intervals we threw out some ballast. The atmosphere below us was serene, but above us it was somewhat cloudy. Though we approached the sun, the heat decreased as we ascended, and we could look at that luminary without being dazzled. When the barometer was at 14 inches it appeared to become stationary. The thermometer was at 4½ degrees below zero. The cold was not excessive; but the singing in my ears increased, and all our faculties seemed to be palsied by a general indisposition.

Having taken a little wine to recruit my strength, I began some experiments, but they were not satisfactory. I proposed to my companion to ascend higher: he consented, though as much indisposed as myself. We successively threw out ballast. The mercury in the barometer fell to 12·4 inches. At that height the cold out of the car was insupportable, though the thermometer was only one degree below the freezing point. We were obliged to respire faster, and our pulse beat with extreme rapidity. We could scarcely resist the strong inclination to sleep with which we were seized. The blood rushed to our head, and M. Lhoest remarked that it had entered my eyes. My head was so swelled that I could not put on my hat. In this region, where the balloon was invisible from the earth, Mr. Robertson made the following experiments:

1st, Having let a drop of ether fall on a piece of glass, it evaporated in four seconds.

2d, He electrified by friction glass and sealing-wax. These substances gave no signs of electricity which could be communicated to other bodies. The Voltaic pile, which when the balloon was set free from the earth acted with its full force, gave only a tenth part of its electricity.

3d, The dipping needle seemed to have lost its magnetic virtue, and could not be brought to that direction which it had at the surface of the earth.

4th, He struck with a hammer oxygenated muriate of potash. The explosion occasioned a sharp noise, which, though not very strong, was insufferable to the ear. It is also to be observed, that though the aëronauts spoke very loud they could with great difficulty hear each other.

5th, At that height Mr. Robertson was not able to extract any electricity from the atmospheric electrometer and condenser.

6th, In consequence of a suggestion from professor Helmbstadt of Berlin, Mr. Robertson carried with him two birds: the rarefaction of the air killed one of them; the other was not able to fly, it lay extended on its back, but fluttered with its wings.

7th, Water began to boil by means of a moderate degree of heat maintained with quicklime.

8th, According to observations made, it appears that the clouds never rise above 2000 toises, and it was only in ascending and descending through clouds that Mr. Robertson was able to obtain positive electricity.

NATURAL HISTORY.

Captain Baudin, commander of the ships sent on a voyage of discovery round the world, by the French government, having collected at New Holland and the Moluccas a quantity of productions from the three kingdoms of nature, sufficient to load one of the ships, dispatched them to France. They arrived lately at Havre in the *Naturaliste*, commanded by captain Hamelin. One of the professors in the Museum of Natural History at Paris was commissioned to receive them, and to take the speediest measures for conveying them to the place of their destination.

This collection consists of more than 140 boxes or half barrels, containing minerals, vegetables, or animals.

The minerals are contained in 14 boxes.

The vegetables, consisting of plants dried and prepared for the herbal, occupy 12: besides these there are 3 casks filled with specimens of different kinds of wood; 2 boxes with seeds, and more than 60 half casks of living plants.

The dead animals, or the remains of them, such as madrepores, shells, insects, preserved birds, and skins of quadrupeds, fill 35 boxes.

The living animals consist of 19 individuals, and are contained in 9 cages.

All these articles, planks of wood proper for cabinet-makers, and some utensils of the Indians, were conveyed from the *Naturaliste* into two small brigs of the state destined to carry them to Paris, and to land them at the gate of the Museum of Natural History.

This cargo is one of the most valuable of the kind ever brought to Europe; but unfortunately the length of the voyage, the want of means, and the multitude of rats with which the vessel was infested, and ignorance in regard to the manner of treating live beings, caused a great many of them to perish on the passage from the South Sea to France. Many of the animals died, and several of those alive are in a languishing condition. A small number only are in good health, such as the Indian hinds and the black swans, male and female. Fortunately they are the most valuable of those embarked.

In regard to the living vegetables, they have suffered still more: of a hundred individuals sent from Port Jackson only thirty give any signs of life, 12 or 15 vegetate feebly, and only half a dozen at most are in a good state. Among the latter there are several kinds of the flax of New Zealand,

land, the most valuable plant of the whole cargo for the purposes of rural and domestic œconomy.

We are assured that captain Baudin will continue his researches around New Holland; that he is employed in collecting geographical observations and natural productions; and that he will return in the month of July next year with a more considerable cargo. As the different parts of this new collection, during the voyage, will be under the care of those who collected them, it is probable they will arrive in a good state, and that they will enable naturalists to become better acquainted with the productions of this fifth part of the world.

RUSSIAN VOYAGE OF DISCOVERY.

The two vessels destined for the new Russian voyage of discovery are the Nadeschda (the Hope), commanded by captain-lieutenant Krusenstern, the chief of the expedition; and the Newa commanded by captain-lieutenant Lisjansky. Both these gentlemen are naval officers of distinguished talents, and animated with the utmost zeal for promoting the objects of the undertaking. Dr. Redowsky of Moscow has been appointed physician to the expedition, with a salary of 2000 roubles, and 800 roubles for his table. As these vessels are destined also to carry an embassy to Japan, this opportunity will be embraced to send back to their native country some Japanese separated from it by an unfortunate accident. In the year 1793 the governor of the province of Sendey, in Japan, dispatched a transport laden with provisions for the maintenance of the troops in the capital. In consequence of tempestuous weather this vessel was driven from its course, and, after wandering about for four months, was at length wrecked on the island of Naazky, which belongs to the Russian American company. The governor received the Japanese who escaped, and who amounted to sixteen persons, with great friendship, and sent them to Ochozk. They were conveyed thence to Irkuzk, where they arrived in the month of September 1794, and afterwards were removed to Petersburgh. At present they amount to twelve, as four of them died and two remained voluntarily at Irkuzk. They have learned to speak the Russian language with tolerable facility, and four of them have embraced the christian religion. The emperor has given them their choice, either to return to their own country or to remain in Russia. Only three have resolved to return to Japan, and one even of these will accompany the embassy back to Russia. Among the presents destined for the emperor of Japan is an ingenious piece of workmanship carried from England to Russia by a German named Schuri, and sold to the empress Catharine II. for 15000 roubles. It represents a peacock of the natural size, which spreads out and folds together its magnificent feathers in a manner which exactly imitates nature. It is accompanied by a great number of birds and small animals, which all move with the greatest ease, and emit the sounds peculiar to them. This beautiful automaton was placed formerly in the Tauride palace, and under Paul I. was removed to the collection of rarities in the Hermitage.

VACCINE INSTITUTION.

The following new Edition of Directions for the Vaccine Inoculation has been lately issued by this Institution:

I. The limpid matter should be taken from a decidedly characterized cow-pock, which is proceeding, apparently, through its respective stages. It is most efficacious in producing the vaccina from a pock before the eleventh or twelfth day; and is most abundant, and is usually taken, about the ninth day. But it may be used at an earlier period, even as early as the fifth day, if it can be collected. However, matter from a pock later than the eleventh or twelfth days is not more liable to produce inflamed arms than that from younger pocks; and if the cow-pock be excited at all, it is as distinct as from any earlier matter. No differences in the effects of the vaccine matter inoculated appear to depend on the presence, extent, or absence of the red areola.

II. The matter is usually taken on glass, thread, or a quill, on which it should be suffered to become dry without applying heat, and when so dried it is scarcely visible. The air should be excluded by keeping the matter between two glass plates, or in a bottle filled with hydrogen gas.

III. As dried matter fails much more frequently to excite the vaccina than recent fluid matter, it will be advisable, in order to insure the effect, or for obtaining a great quantity of matter, that, instead of a single puncture or scratch (which is sufficient and preferable with recent matter), there be matter inserted in two punctured or scratched parts in each arm. The dried matter at the time of inoculation should be softened by warm but not very hot water.

IV. The inoculation must be performed in the same manner as for the small-pox.

V. If the infectious matter produce the required effect in three, four, or five days, there will be seen a red spot like a small gnat-bite; in six or seven days, a small vesicle will appear; in nine days, a circular vesicle (improperly called a pustule) will be found as large as a pea, or from about two-tenths to four-tenths of an inch in diameter, usually surrounded by a red areola. By the eleventh day the vesicle begins to scab or grow dry, and turn black in the middle, and the areola becomes more extensive. By the fifteenth day, but often later, the pock becomes a mere scab, circular, prominent, well defined, of a blackish or mahogany colour, adhering firmly; but the areola disappears. Unless it be separated by violence the scab does not fall off, in general, sooner than the twentieth day. It then leaves a cicatrix permanent for life.

VI. If the eruption or pimple excited by inoculation has not the characters and does not pass through the stages in the course above stated (V), although sometimes anomalous, this cow-pock may render the constitution unsusceptible of the small-pox, yet it cannot be depended upon. In such cases, the inoculation should be re-instituted; for, if the vaccina cannot be again excited, the unsusceptibility desired will have been produced: but if a further proof be wanted, recourse must be had to inoculation with the variolous matter.

VII. In many cases, no constitutional affection or fever can be perceived: when it occurs, it is almost always on the ninth and tenth days; but provided the pock exhibit the distinctive characters of the cow-pock, even without areola, with the usual course of its stages, the susceptibility of the small-pox will be as effectually destroyed as if there had been considerable febrile affection, and extensive areola.

VIII. If erythema, like erysipelas, extend over the arm, with swelling, pain, &c., it has always subsided in a few days of itself, only avoiding irritating applications, or at most on using sedatives.

IX. Eruptions sometimes occur, but they require no particular treatment.

X. The small-pox may break out at any period within twelve days of inoculation for the cow-pock. If they appear earlier than the sixth or seventh, the vaccina is cut off in its progress; if they appear later, the vaccina goes forward in its usual course.

XI. The medical treatment which may be required from unusual or supervening complaints, being analogous to that in the small-pox, must be accordingly.

XII. Measles, chicken-pox, hooping-cough, and other disorders, may intervene during the vaccina, without, in general, varying its progress*.

FULMINATING SILVER.

In our last we gave (from M. Van Mons's Journal) a letter from Brugnatelli, announcing *as new* a preparation of a fulminating silver. Having since compared it with a process described by the honourable Mr. Howard at the end of his interesting paper on Fulminating Mercury, published in the Philosophical Transactions, (also in this work, see vol. vii. p. 126,) we find it to be exactly the same.

ASTRONOMY.

The weather was very favourable for observing the solar eclipse on the morning of the 17th; but owing to an error in the Nautical Ephemeris, many people were disappointed of an opportunity of seeing the commencement of the phænomenon.

Geocentric motion of Pallas for September 1803.

	A. R. of Pallas.	Declin. North.
Sept. 3	17^{h} 56^{m} 47^{s}	13° $23'$
6	17 57 29	12 48
9	17 58 22	12 13
12	17 59 24	11 40
15	18 0 34	11 6
18	18 1 55	10 32
21	18 3 26	10 1
24	18 5 6	9 27
27	18 6 52	8 56
30	18 8 49	8 25

* It has been found proper to require half-a-guinea for arming three lancets, or for matter on thread or glass: but each practitioner may be supplied with matter as often as wanted for his own use only, by paying one guinea annually, the expense of postage and porterage being discharged by those who apply.

The Institution does not warrant any matter but that which has on the package the impression of the seal of the Institution, namely, a cow, with the motto *Feliciores inserit.*

LIV. *On the Stones said to have fallen at Ensisheim, in the Neighbourhood of Agen, and at other Places. From the Memoir of M.* De Drée *.

Being desirous to collect a certain number of these extraordinary facts, that I might compare them with each other, I sought for and obtained some correct ideas respecting two masses of this kind; one of which fell near Ensisheim, and the other in the environs of Agen. These stones, indeed, have been already mentioned; but as the latter was neither analysed nor described mineralogically—as the circumstances of the fall of the former are imperfectly known—and as, at the period at which Mr. Barthold professor of the central school of the Upper Rhine described and analysed it, he could not search for certain distinguishing characters of this kind of stones, which were not then known—I thought it might be of importance to give an account of them here, in order to remove all uncertainty respecting the nature of these bodies.

1. *Stone of Ensisheim.*

The stone known under this name, which fell at Ensisheim in Alsace, made a great noise, especially about the end of the fifteenth century, and is mentioned in several works. Mr. Butenschoen, professor of history in the central school of Colmar, gave some account of it, which was inserted in the *Decade Philosophique*; and to the kindness of that gentleman I am indebted for the following extracts from the chronicles of the time.

Literal Translation of a German Notice respecting the Stone of Ensisheim, which was formerly preserved along with that Stone in the Parish Church of the Place.

"On Wednesday, Nov. 7. the night before St. Martin's day, in the year of our Lord 1492, a singular miracle happened: for between the hours of eleven and twelve a loud clap of thunder took place, with a long-continued noise, which was heard at a great distance; and a stone fell from the heavens in the Ban of Ensisheim which weighed 260 pounds; and the noise was much louder in other places than here. A child then saw it strike on a field situated on the upper Ban, towards the Rhine, and the In, near the canton of Gisgane, which was sown with wheat. It did no hurt, except that it made a hole there. It was afterwards transported thence; and a great many fragments were detached from it, which the land-vogt forbade. It was then deposited in the church, with intention of suspending it as a miracle; and a great many people came hither to see this stone, respecting which there were singular discourses. But the learned said they did not know what it was, for it was something supernatural that so large a stone should fall from the atmosphere; but that it was a miracle of God: because, before that time, nothing of the kind had ever been heard of, seen, or described. When this stone was found, it had entered the earth to a depth equal to the height of a man. What every body asserted was, that it had been the will of God that it should be found. And the noise of it was heard at Lucerne, at Villing, and many other places, so loud, that it was thought the houses were all overturned. And when king Maximilian was here, the Monday after St. Catharine's day of the same year, his royal excellency caused the stone which had fallen to be carried to the castle; and after conversing a long time with his lords, he said the people of Ensisheim should take it: and he gave orders that it should be suspended in the church, and that no person should be permitted to take any part of it. His excellency, however, took two fragments; one of which he kept, and the other he sent to duke Sigismund of Austria. The people talked a great deal of this stone, which was suspended in the choir, where it still is, and many came to see it."—*Arithenicus in Chronico Hirsaugiensi, in Vitâ Blavii Abbatis* XL. *ad Annum* 1492. *Edit. M.S. Galli*, 1690, *Vol. II. p.* 551.

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* See the last Number of the Phil. Mag.

"The same year (1492), on the 7th day of November, a stone, called a thunder-stone, of a prodigious size (for we have it from eye-witnesses that it weighed 255 pounds), fell from the heavens in the village of Suntgaw, near the town of Ensisheim, not far from Bale, a city of Germany. Its fall was so violent that it broke into two pieces. The largest is still to be seen at the door of the church of Ensisheim, suspended by an iron chain, as a proof of the truth of the fact which we announce, and to preserve the remembrance of it." —*Paulus Lang, in Chronico Cirizense, in Vol. III. Scriptor. Rer. Germ. Histor. p.* 1264.

"On the 7th of the ides of November, in the year of our Lord 1492, there arose a storm, during which the heavens appeared to be on fire. While the thunder roared, a stone of a prodigious size fell from the heavens, with a horrible crash, near the town of Ensisheim, on the lands belonging to the emperor Frederick. Its form was that of a delta, with a triangular point. It is still shown at Ensisheim as a wonderful phænomenon."—*Joh. Linturii Appendix ad Fasciculum Temporum Werneri Rollewinck, in Vol. I. Scriptor. Rer. Germ. Histor.* p. 580.

The same year (1492), after the festival of St. Martin, a stone weighing 300 pounds and more, hard, and of different colours, fell in Alsace, with a great noise, from a brilliant and flaming cloud. The rest of the horizon exhibited no clouds. At the same moment, the heavens being still serene, a large red cross was observed around the moon.

In a rescript of king Maximilian, dated Augsbourg, Nov. 12, 1503, that sovereign mentions this stone, which he says fell near him while at the head of his army; and he gives it as a presage of the victory which he had gained over the crown of France*.

Brant also has made this stone the subject of some poems †.

One of the fragments of this stone was suspended and preserved in the church of Ensisheim till a few years back, when it was conveyed to the library of Colmar, and there deposited. It weighs about 150 pounds at least, notwithstanding the specimens which have been detached from it; and professor Butenschoen says that this stone is beyond all doubt that spoken of in the before-mentioned chronicles.

M. Felix Desportes, præfect of the department of the Upper Rhine, having permitted several fragments to be detached from this mass, in order to be sent to me, I am indebted to that zealous protector of the sciences, and to professor Butenschoen, for the advantage of being able to give a comparative description of its characters, and for an analysis of it, which Vauquelin undertook to make.

" The different specimens of this stone which I received did not exhibit that continued black vitrified crust observed on the stone of Sales, and other stones of the same kind; but I discovered this crust in the cavities which had been sheltered from the shock and from friction; and these remains are sufficient evidences to attest that this crust had existed. It exhibits the puffed-up appearance of vitrification: the colour is only rather brown than black; which arises either from the effect of time, or from the greater quantity of iron diffused throughout the mass, as will be seen hereafter.

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* Rescriptum Maximiliani regis de cruciata, &c. die 12 Nov. 1503, in vol. Rer. Germ. nono, de pace publica, autore J. P. Datt. Ulm, 1698, p. 114.

† De fulgetro immani jam nuper, anno 1492, prope Basileam, &c. In variis Sebastiani Brant carminibus. Basileæ, 1498, 4to.

As this stone has a great resemblance to that of Sales, I shall for the sake of brevity give only a comparative description of them.

In this stone, as well as in that of Sales, there are found malleable iron, containing nickel disseminated throughout it in grains; lamellated and whitish sulphuret of iron in the form of lumps and grains; gray sulphuret of iron less sulphurated, in thin scaly strata, which line a multitude of small fissures which traverse the stone in every direction: but it is observed that the white pyrites in it form larger lumps, and that the gray pyrites is more abundant than in that of Sales.

Small amygdaloid globules might perhaps be seen in it; but as they have a more metallic fracture than those of the stone of Sales, as they are almost confounded in regard to appearance with the mass, they do not deserve particular attention. Its interior colour is darker and bluer than that of Sales, and its fracture is somewhat changeable in its colour, in consequence of the metallic splendour of small fissures, which gives it a different appearance by the magnifying glass; and on the transverse branches of these fissures it is observed, that this texture and the constituent elements are the same in both, except in regard to the grain, which is finer, and the tissue, which is more compact, than in that of Ensisheim.

On carefully examining the differences which I observed between this stone and that of Sales, and which distinguish it also from the stones of Agen and of Benares, and from others of the same kind about to be mentioned, it will be seen that they do not relate to the constituent elements, but only to their proportions; it is therefore impossible not to discover that this mineral mass is of the same nature as that of Sales and others of the like kind. This is confirmed by the following note, communicated to me by Vauquelin:

"This stone has a perfect resemblance in the number, nature, and quantity of its constituent principles, to all the stones said to have fallen from the clouds which have hitherto been subjected to chemical analysis.

"It is certainly composed of silex, magnesia, iron, nickel, sulphur, and a small quantity of lime.

"I have ascertained, by particular trials, the presence of sulphur and nickel in the grains of malleable iron, and in the pyrites, but in different proportions. This stone therefore

fore resembles, in every point, all those which have fallen from the atmosphere."

2. *Stones of Agen.*

The stones known under the name of *the stones of Agen* have been mentioned in the *Bibliothèque Britannique* *. Their fall has been confirmed by numerous testimonies and authentic documents, inserted by M. Bertholon in the *Journal des Sciences utiles* †.

According to the above accounts, these stones fell on the 24th of July 1790, between nine and ten in the evening, in the communes of Juliac, Creon, and others adjacent, between Roquefort in the department of Landes, Mezin in the department of Lot and Garonne, and Eause in the department of Gerz, after the apparition of a large fire-ball which passed through the air, accompanied with a loud report.

They fell at different distances; some gently, and others with rapidity and a hissing noise.

They buried themselves more or less in the earth: several had fissures in them.

Their weight in general was between a quarter of a pound and two pounds: some of them, however, are said to have weighed from twenty to twenty-five pounds; and Mr. St. Amand saw in the museum of Bourdeaux one of these fragments about fifteen inches in length, taken from a stone which, as said in the accompanying note, crushed a cottage, made a conical hole of about five feet in depth, and killed a farmer and some cattle, at the time of the explosion of July 24, 1790, near Roquefort in Landes.

There is no stone in the place where they fell which has any resemblance to them.

Mr. Darcet was so kind as to transmit to me a fragment of one of these stones, which fell near Barbotan, a place situated within the boundaries above traced out. It was that subjected to analysis by Vauquelin.

This stone has so great a resemblance to that of Sales, even in the most minute particulars, that the mineralogical description would be the same, and therefore I refer to the latter. It has the same vitrified crust; the same substances included, and nearly in the same proportions; the same texture, hardness, and aspect; the same chemical characters.

The analysis made of it by Vauquelin gave him the same chemical substances as the other stones, and in proportions nearly the same.

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* No. 154. p. 85. † No. 23. and 24. 1790.

3. *Notice respecting a Mass of Iron which fell in the Mogul Territories. Communicated by Mr.* Greville *to the Royal Society of London.*

All the masses of iron of unknown origin, such as those found in Siberia, America, &c. contain nickel, and have besides characters analogous to those of the stones which have fallen from the clouds. These circumstances induced Mr. Howard and Count de Bournon to conclude that these substances might have had the same origin. The following fact tends to confirm this opinion. Mr. Greville, who communicated it to the Royal Society, extracted it verbatim from the Memoirs of Jehangire emperor of the Moguls, written in Persian by himself, and translated by colonel William Kirkpatrick.

" *A. H.* 1030, *or the* 16*th of the reign.* The following is among the extraordinary occurrences of the period.

" Early on the 30th of Furverdeen of the present year *, and in the eastern quarter (of the heavens), there arose in one of the villages of the purgunnah † of Jalindher such a great and tremendous noise as had nearly, by its dreadful nature, deprived the inhabitants of the place of their senses. During this noise a luminous body (was observed) to fall from above on the earth, suggesting to the beholders the idea that the firmament was raining fire. In a short time, the noise having subsided, and the inhabitants having recovered from their alarm, a courier was dispatched (by them) to Mahomed Syeed, the aumil ‡ of the aforesaid purgunnah, to advertise him of this event. The aumil instantly mounting (his horse) proceeded to the spot (where the luminous body had fallen). Here he perceived the earth, to the extent of ten or twelve guz § in length and breadth, to be burnt to such a degree, that not the least trace of verdure, or a blade of grass, remained; nor had the heat (which had been communicated to it) yet subsided entirely.

" Mahomed Syeed hereupon directed the aforesaid space of ground to be dug up; when the deeper it was dug the greater was the heat found to be. At length a lump of iron made its appearance, the heat of which was so violent that one might have supposed it to have been a furnace. After some time it became cold; when the aumil conveyed it to his own habitation, from whence he afterwards dispatched it, in a sealed bag, to court.

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* The first of Furverdeen of this year (A.H. 1030) corresponded with Saturday the 27th of Rubbi ul Akhir; consequently the 30th of Furverdeen fell on the 26th of Jummad ul Ouwul, or A. D. 1620.

† A purgunnah is a territorial division of arbitrary extent. The purgunnah of Jalindher is situated in the Punjaub, and about 100 miles south-east of Lahor.

‡ Aumil is a manager or fiscal superintendant of a district.

§ A guz is rather less than a yard.

" Here I had (this substance) weighed in my presence. Its weight was 160 tolahs*. I committed it to a skilful artisan, with orders to make of it a sabre, a knife, and a dagger. The workman (soon) reported that the substance was not *malleable, but shivered into pieces under the hammer* †.

" Upon this I ordered it to be mixed with other iron. Conformably to my orders three parts of the *iron of lightning* ‡ were mixed with one part of common iron; and from the mixture were made two sabres, one knife, and one dagger.

" By the addition of the common iron, the (new) substance acquired a fine temper; the blade (fabricated from it) proving as elastic as the most genuine blade of Ulmanny § and of the south, and bending like them without leaving any mark of the bend. I had them tried in my presence, and found them cut excellently; as well (indeed) as the best genuine sabres. One of these sabres I named *katai*, or *the cutter*; and the other, *burk-serisht*, or *the lightning natured*.

" A poet ‖ composed and presented to me, on this occasion, the following tetrastich:

" 'This earth has attained order and regularity through
the emperor Jehangire.
" In his time fell *raw* iron from lightning:
" That iron was by his word-subduing authority converted into a dagger, a knife, and two sabres.'

" The chronogram of this occurrence is contained in words which signify 'the flame of the imperial lightning,' and give the year (of the Hegira) 1030.

" N.B. The foregoing translation (which is nearly literal) has been made from a manuscript that has been several years in my possession; and which, although without a date, bears marks of having been written at a remote period.

" WILLIAM KIRKPATRICK."

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* A tolah is about 180 grains Troy weight.

† Literally, "it did not stand beneath the hammer, but fell to pieces."

‡ This expression is equivalent to our term *thunder-bolt*.

§ The name of the place here designed is doubtful.

‖ The poet is named in the original, but the name is not perfectly legible.

Substances.	*Places where they fell*	*Period of their Fall.*	*Testimonies.*
Shower of stones	At Rome	Under Tullus Hostilius	Livy
Shower of stones	At Rome	Consuls C. Martius & M. Torquatus	J. Obsequens
Shower of iron	In Lucania	Year before the defeat of Crassus	Pliny
Shower of mercury	In Italy	- - - - - - - - - - - - -	Dion
A very large stone	Near the river Negos, Thrace	Second year of the 78th Olympiad	Pliny
Three large stones	In Thrace	Year before J. C. 452	Ch. of count Marcellin.
Shower of fire	At Quesnoy	Jan. 4th, 1717	Geoffroy le Cadet
Stone of 72 lbs.	Near Larissa, Macedonia	Jan. 1706	Paul Lucas
About 1200 stones—one of 120 lbs. Another of 60 lbs.	Near Padua in Italy	In 1510	Cardan, Varcit
A stone of 59 lbs.	On Mount Vaiser, Provence	Nov. 27th, 1627	Gassendi
Shower of sand for 15 hours	In the Atlantic	April 6th, 1719	Pére la Feuillée
Shower of sulphur	Sodom and Gomorra	- - - - - - - - - - - - -	Moses
Sulphureous rain	In the duchy of Mansfeld	In 1658	Spangenberg
The same	Copenhagen	In 1646	Olaus Wurmius
Shower of sulphur	Brunswick	Oct. 1721	Siegesber
Ditto of a viscid unknown matter	Ireland	In 1695	Muschembroek
Two large stones weighing 20 lbs.	Liponas in Bresse	Sept. 1753	Delalande
A stony mass	Niort, Normandy	In 1750	Delalande
A stone of 7¼ lbs.	At Luce in Le Maine	Sept. 13th, 1768	Bachelay
A stone	At Aire in Artois	In 1768	Gurson de Boyaval
A stone	In Le Cotentin	In 1768	Morand
Extensive shower of stones	Environs of Agen	July 24th, 1790	St. Amand, Baudin, &c.
About 12 stones	Sienna, Tuscany	July 1794	Earl of Bristol
A large stone of 56 lbs.	Wold-Cottage, Yorkshire	Dec. 13th, 1795	Capt. Topham
A stone of about 20 lbs.	Salé, department of the Rhone	March 17th, 1798	Lelievre and De Drée
A stone of 10 lbs.	In Portugal	Feb. 19th, 1796	Southey
Shower of stones	Benares, East Indies	Dec. 19th, 1798	J. Lloyd Williams, esq.
Shower of stones	At Plann, near Tabor, Bohemia	July 3d, 1753	B. de Born
Mass of iron 70 cubic feet	America	April 5th, 1800	Philosophical Magazine
Mass of ditto, 14 quintals	Abakank, Siberia	Very old	Pallas, Chladni, &c.
Shower of stones	Barboutan, near Roquefort	July 1789	Darcet jun., Lomet, &c.
Large stone, 260 lbs.	Ensisheim, Upper Rhine	Nov. 7th, 1492	Butenschoen
Two stones, 200 and 300 lbs.	Near Verona	In 1762	Acad. de Bourd.
A stone of 20 lbs.	Sales, near Ville-Franche	March 12th, 1798	De Drée
Several ditto from 10 to 17 lbs.	Near L'Aigle, Normandy	April 26th, 1803	Fourcroy

LV. *Memoir on the Stones which have fallen from the Atmosphere, and particularly near Laigle, in the Department of l'Orne, on the 26th of April last. Read by* C. FOURCROY, *in the public Sitting of the Class of the Mathematical and Philosophical Sciences of the Institute, June* 19*th*, 1803.

NATURE sometimes exhibits to us facts insulated, as we may say, and so different from any thing with which we are acquainted, that their existence is long problematical even to men who are most accustomed to observe its wonderful works, and to calculate its powers. It was in this manner that naturalists and philosophers for a long time classed among fables and popular errors the fall of solid and stony bodies on our globe.

Exact accounts, however, which have been multiplied for six or eight years past; the coincidence of meteoric circumstances which in all these accounts accompany the principal phænomenon; the analogy of the form, structure, and colour, observed in several of these stones, which have fallen at different times and in different places very distant from each other; and the difficulty of referring these stones to any of the species with which we are acquainted, induced Mr. Howard, an English chemist, to analyse these productions hitherto so little known.

By chemical examination he not only found that they were all composed of the same principles, but that there was a striking difference between them and all the other mineral substances hitherto analysed. He found that they contained in general from a fourth to two-thirds of their weight of silex, a third of iron, a sixth or seventh of magnesia, and some hundredth parts of sulphur and nickel. He found also that the general mass of these stones contains inclosed in it globules of iron allayed with nickel and a little sulphur, and fragments of pyrites composed of sulphurated iron and nickel.

C. Vauquelin obtained the same results from three of the same stones analysed by Mr. Howard, and from two others that fell in France, one at Barbotan in 1789, and the other at Creon, in the parish of Juliac, on July 24, 1790.

The attention of philosophers was much excited by the novelty of these results, while the ability of the chemists who presented them commanded the utmost confidence. Hence, instead of rejecting the existence of the phænomenon,

non, as had before been done, the greatest philosophers were desirous that it should be carefully studied, fully confirmed, and accurately described. With this view C. Izarn composed his *Lithologie Atmosphérique,* which was soon presented to the National Institute. In this interesting work, the first ever written on the subject, are found a multitude of similar facts having all the characters of authenticity; and all the opinions hitherto expressed, both in regard to the existence and causes of the phænomenon, are clearly detailed and discussed.

At the time when we were most occupied with this new problem of natural philosophy, and while, uncertain in regard to its existence, we were discussing the authenticity of the accounts given of it by the antients and moderns, the inhabitants of Laigle and of a vast extent of surrounding district were witnesses of the phænomenon: it appeared over their heads on the 26th of April, with circumstances capable of striking with terror and astonishment.

SECTION I.

Description and Analysis of the Stones which fell at Laigle, in the Department of l'Orne, on the 26th of April 1803.

From all the letters I received, and which I successively communicated to the Institute, of which I may mention as the most authentic those of our fellow-member Leblond, who has resided in Laigle for several years, it results,

1st, That about one in the afternoon, on the 26th of April, the air being rather cold than warm, and the sky without clouds, there was seen, at the distance of twelve or fifteen leagues west-south-west from Laigle, a luminous globe moving towards the north-west with great velocity.

2d, That nearly at the same hour there was heard at Laigle and in several of the surrounding villages a violent explosion, succeeded by two others no less extraordinary, which were followed by a rumbling noise, the more terrible as no one knew to what it could be compared or ascribed, and which continued about ten minutes.

3d, That after this noise, by which the animals were as much frightened as the inhabitants, there were seen to fall, with a hissing noise, stones very much scattered, and of different sizes, from 2 or 3 gros to 17 pounds in weight; that these stones at first exhaled a strong smell of sulphur, which was gradually dissipated; that those who picked up some of them at the time found them very warm; and that to judge by the number collected, and by the extent of the ground on which they were found, an astonishing quantity must have fallen.

These stones in general are irregular, polygonal, often cuboid, sometimes sub-cuneiform, and exceedingly various in their diameters and weight; they are all covered with a black gravelly crust consisting of a fused matter, and filled with small agglutinated grains of iron. The greater part of them are broken at the corners, either by their shock against each other, or by falling on hard bodies. The interior parts resemble those of all the stones analysed by Messrs. Howard and Vauquelin; they are gray, a little varied in their shades, granulated, and as it were scaly, split in many points, and filled with brilliant metallic points exactly of the same aspect as those of other stones of the like kind.

In conjunction with C. Vauquelin I analysed them in the following manner, which has been already employed on similar occasions: The stone being reduced to fine powder, we poured over it muriatic acid somewhat weakened. A pretty strong effervescence was produced; an odour of sulphurated hydrogen gas was disengaged, and the liquor assumed a very evident green colour: the gas collected was not entirely sulphurated. Muriatic acid was twice in succession poured over it to deprive of its colour the insoluble part, which after being well washed was found to be pure silex, forming more than half of the whole weight of the stone. The muriatic solution with excess of acid was treated with ammonia, which precipitated from it the oxidated iron, and retained the magnesia and the nickel. The iron was completely separated by boiling the liquor, and nearly 36 per cent. of that metal, weakly oxidated, was obtained. The liquor, containing a triple muriate of ammonia, nickel, and magnesia, was mixed with a solution of potash to precipitate the magnesia, which carried with it a small portion of nickel. Nearly 9 per cent. of magnesian earth was obtained. The water charged with sulphurated hydrogen was afterwards employed to separate the oxide of nickel, of which we found about 3 per cent.

I shall omit saying any thing here of some difficulties which occurred in the details of this analysis: as I reserve these for a particular memoir, I shall content myself with announcing the result of the analysis. It gave us as the constituent materials of the stone of Laigle the following proportions nearly:

Silex - - -	54
Oxidated iron - -	36
Magnesia - -	9
Nickel - - -	3
Sulphur - - -	2
Lime - - -	1
	105

The 5 per cent. of increase arose from the oxidation of the metals produced by the analysis.

SECTION II.

Analysis of the Stone of Ensisheim.

The stone which fell at Ensisheim about the end of the 15th century has given rise to many accounts more or less fabulous. Almost all contemporary authors speak of it. M. Butenschoen, professor of history in the central school of Colmar, has communicated to me several interesting extracts from them: but I shall give only the principal facts of this interesting history.

We read in a manuscript chronicle, written in German, that between the hours of eleven and twelve in the forenoon, on the 7th of November 1492, there was heard in the environs of Ensisheim a terrible clap of thunder, and that a child saw fall in a field sown with wheat an enormous stone, which entered the earth to the depth of about three feet: it weighed at that time 260 pounds. Maximilian, king of the Romans, after causing some fragments to be detached from it, gave orders that it should be suspended in the parish church of Ensisheim. Since the revolution it has been transported to Colmar, and placed in the library: at present it weighs only 171 pounds.

M. Barthold, professor of chemistry in the central school of the Upper Rhine, gave in the year 8 an analysis of this stone. Besides silex, iron, sulphur, and magnesia, he announced 0·17 of alumine, and considers it as a secondary argillo-ferruginous stone, arising from the decomposition of primitive rocks, and detached from a neighbouring mountain.

The method of analysis which the professor followed did not allow him to distinguish very accurately the earths which enter into the composition of this production. He admits also alumine, which we did not find in any of our experiments; while, on the other hand, he observed no nickel; which, indeed, it was impossible to discover by the means he employed.

C. Felix Desportes, præfect of the Upper Rhine, always disposed to favour researches useful to the sciences, sent me a fragment of the stone of Ensisheim weighing several kilogrammes, which contained on one side a portion of the black fused crust a little oxidated, and exhibited all the other properties of the other stones which have fallen from the atmosphere. There are found in it a kind of small veins of gray brilliant sulphuret of iron and nickel. We did not meet with any very apparent globules of iron.

A hundred parts of this stone, treated according to the processes already described, gave

Silex	56
Oxidated iron	30.
Magnesia	12
Nickel	2·4
Sulphur	3·5
Lime	1·4
	105·3

It contains then the same principles as the stone of Laigle, and differs from it only by a little less iron and nickel, and by a little more magnesia and silex: but this difference amounts only to a few hundredth parts.

On comparing the analysis of these two stones with those already made by Messrs. Howard and Vauquelin, it is impossible not to observe a striking identity in their composition.

SECTION III.

Conclusion and Reflections on the Origin of these Stones.

Here then we have nine stones all well ascertained to have fallen from the atmosphere with noise, detonation, luminous meteors; all gray, granulated, and metalliferous in the interior parts; which give the same products by analysis, containing no alumine, but a great deal of silex, a little magnesia, and a singular combination of iron, nickel, and sulphur: in a word, all perfectly similar to each other, and all different from the other known minerals of the earth.

It cannot therefore appear surprising that so striking a physical and chemical analogy should induce a belief that all these stones have the same origin, and that, as they form an order of compounds different from any thing ever yet observed

served among minerals, some philosophers should conclude that they do not belong to the fossils of our globe. Several hypotheses have consequently, for some months past, been devised to explain the formation of these singular productions.

It has long been asserted that they are nothing else but minerals elevated and projected from the earth by volcanoes. Other philosophers considered them as stones of our globe struck and fused on the outside by lightning on the spot where they were found; and lately they have been considered as earthy and metallic substances raised into the air, which being there collected and agglutinated have formed these masses, which immediately fell down by their own weight.

The manifest contradictions exhibited by these opinions, either with the principal circumstances or the fact itself, of the fall of these stones, have given rise to one less improbable, though perhaps more extraordinary. It is that of some geometricians, who consider them as volcanic productions projected from the moon beyond the sphere of its attraction, and to the confines of that of the earth.

If this opinion, on the first view, seems to be contradicted by all the ideas hitherto entertained, it is at any rate seen that it is much less susceptible of solid objections than any of the preceding hypotheses. The same may be said of that of Chladni, who with some other philosophers considers all the masses which have fallen to the earth as solid bodies detached from some other planet at the time of their formation, and which move about in infinite space till they meet with another, which becoming to them a new centre of gravity attracts them to its surface.

An analytical examination of all these hypotheses, and the little agreement between them and the aggregate of the circumstances which constantly accompany the phænomenon of the fall of stones, and which are essential to them, have induced the author of the *Lithologie Atmosphérique* to suppose that these stones are formed of the elements of those earths and metals which they exhibit by analysis; elements which he supposes to be in the gaseous state at a great height in the atmosphere, and the combination of which he ascribes to unknown circumstances that rarely occur. This opinion admits of several hypotheses, too far distant from what is yet known not to present difficulties which cannot be solved in the present state of our knowledge.

To conclude: In such situations one is obliged to choose among ideas each as uncommon as the other; but it is only by rejecting what is absurd and impossible that we can adopt what at first would have appeared incredible.

LVI. *Of the general Relation between the Specific Gravities and the Strengths and Values of Spirituous Liquors, and the Circumstances by which the former are influenced.*

[Continued from p. 211.]

Of Over-Proofs and Under-Proofs, and the Modes of appreciating them.

§ 21. THE first idea respecting the denomination of the relative values of spirituous liquors appears to have been that of Mr. Clarke, the inventor of the hydrometer now known by his name, which was founded on the supposed respective proportions of water which would be requisite to reduce an over-proof spirit to proof, or proof to an under-proof: the quantity of water was, however, considered as invariable, being always one gallon; whilst that of the spirit was regarded as variable, and as being so modified as to produce the required ratio between the two. Thus, a liquor which was so strong as to be supposed to require the addition of half its measure of water to reduce it to proof was called "one to two over proof," indicating that one gallon of water added to two of the spirit would make proof spirit of it; a spirit, with respect to which it was conceived that one-third of its measure of water would render it of proof strength, was called "one to three over proof," and so on. With regard to such liquors as were below the proof strength, an analogous mode of denomination was used. Thus, a liquor which was considered as being of equal strength with a mixture of one gallon of water with three of proof spirit was called "one *to* three," or more commonly "one *in* four under proof." This latter denomination became at length almost generally employed by those who used Clarke's hydrometer; so that "one *to* four" was regarded as signifying that a liquor was 25 per cent. over proof; and "one *in* four," that it was 25 per cent. under proof, without the use of these epithets themselves.

§ 22. It was impossible, however, not to feel the inconvenience of a system of denomination which was so complex and indefinite. Every series by which successive quantities, qualities, or values of any description are defined,

ought

ought necessarily to be as nearly equi-differential as is consistent with the nature of the thing. This, however, is by no means the case with that which is here employed. The differences between the successive terms of the harmonic series one-half, one-third, one-fourth, one-fifth, one-sixth, &c., are one-sixth, one-twelfth, one-twentieth, one-thirtieth, &c.; or such that they are constantly to each other as the products of the terms between which they fall; and their inequality, therefore, becomes very soon exceedingly great.

It will perhaps, however, be more intelligible to some of our readers, if we explain this matter in other words. The first objection, then, which must naturally occur to every one with regard to this method of denominating strengths, is, that in one part of the series the difference between those of two denominations immediately succeeding each other is vastly too great, whilst at another part of it this difference is as disproportionally minute. Let us suppose, for example, that spirit of any particular kind, of proof strength, is worth 12s. per gallon. Then, what is *meant* by Clarke's "one to two," or that spirit of which 2 gallons would make 3 of proof, would be worth 12 multiplied by 3 divided by 2, or 18s. per gallon; and what is *meant* by his "one to three," or that of which 3 gallons would make 4 of proof, would be worth 12 multiplied by 4 divided by 3, or 16s. per gallon only. The difference between the strengths and values of these two kinds of spirit is enormous when regarded in a commercial point of view; and yet we have no denomination according to his system (without using more complex ratios and higher terms) for any intermediate strength. If we wished to express the strength of a spirit of the same kind which was in this respect worth 17s. per gallon, we have no name for it. Now let us look at two other terms of his series, and see what is the difference in value between his "one to nine" and "one to ten." The former of these is worth 12 multiplied by 10 divided by 9, or 13s. 4d.; and the latter, 12 multiplied by 11 divided by 10, or somewhat less than 13s. 2½d.; so that here the difference is but little more than 1½d. a gallon, instead of 2s.; and the further we proceed according to this system, the more the disproportion increases. But this is not all. There are many kinds of spirit which are above his "one *to* two," though we have not yet discovered any which would require an equal measure of water to reduce it to proof; and there are, on the other hand, faints and low wines which are worth preserving, which are below his "one *in* two:" and for all these we have no term whatever.

§ 23. The next mode of denomination of which we shall speak is that which is founded on the consideration of the proportion of water which it would be necessary to add to or subtract from a given quantity of any liquor, in order to render it of proof strength, and which proportion is estimated in hundredth parts of the quantity of the liquor in question. Thus a liquor of which 100 gallons would require the addition of 20 gallons of water to reduce it to the proof strength was said to be 20 per cent. over proof; and one of which 100 gallons would have required the *subtraction,* if that were possible, of 20 gallons of water to render it of proof strength (or, which is the same thing, of which 100 gallons might be produced by making up 20 of water to 100, by the addition of proof spirit), was said to be 20 per cent. under proof.

§ 24. This latter method of denominating the strengths of these liquors which obtained in consequence of the defects and inconveniences already stated in § 21 to have belonged to that of Mr. Clarke, was, as well as that, founded on the supposition that the quantity by measure of any compound of spirit and water would be equal to the sum of their quantities before mixture, the principle of concentration (§ 10) being, when it was first adopted, scarcely known.

If, indeed, this supposition were true, either mode of denomination would, though with different degrees of convenience, convey an idea of both the relative strengths and values of the compounds. This, however, being now known to be by no means the case, as has been already stated, it is, perhaps, rather unfortunate that the system of denomination mentioned in the last section has now acquired such a very general acceptation, that we may expect that it will not be without some difficulty that the same system, only so far changed as to render it consistent with the present state of science and truly indicative of relative strengths and values, will be received and understood.

§ 25. If the gallon of proof spirit is to be our standard of comparison, we should of course indicate its temperature, for quantities by *measure* are not the same at different degrees of heat (§ 8). We have already supposed this to be 60° (§ 20): but the gauging or measuring and proving of spirituous liquors, both by the revenue officers and dealers, are performed at various temperatures; and the quantity of the spirit itself which its measure indicates therefore varies accordingly. Now if this variation in bulk was equal in spirituous compounds of all strengths, a gallon of any such liquor would at any degree of heat contain a certain proportion of its bulk of alcohol, and its per-centage would therefore not vary, whatever was its temperature, though a correction would be necessary with respect to the actual measure of each. We have already said, however, that this is not the case (§ 9); and it is also true, that not only the quantity of this variation is different in respect of their strengths, but the law of its progression varies still more. If the expansion of mercury be considered as uniform, as it appears to be very nearly between the freezing and boiling water points, the expansion of alcohol is progressively increasing, but in a very small degree, between 30° and 80° of Fahrenheit's thermometer; whereas water actually contracts by elevation of its temperature till it reaches 40°, after which it again begins to expand in a very increasing progression, its expansion of bulk between 70° and 80° being more than 5 times as great as that which takes place between 40° and 50°. The expansion of mixtures of these two fluids will in both respects approach nearest to that of the predominant ingredient; but the exact quantity of it, or the law of its progression in any such compound, can only be ascertained by experiment.

We must therefore apply such corrections in our process as may give us the quantity of proof spirit by weight; or, which amounts to the same thing, by measure when reduced to 60°, which is equivalent to 100 parts by measure of any spirit at any given temperature at which it may be measured and proved.

§ 26. The ultimate conclusion from the premises laid down in this chapter with respect to the mode of denominating the strengths of spirits differing from proof is, that it will be convenient that the scale should be graduated in per cents indicating the relative values of each compound with respect to the common standard of proof; or the number of parts of proof spirit by measure at 60°, which would produce or be producible from 100 parts by measure at any given temperature of any given spirituous liquor. Taking, therefore, the strength or value (for, *cæteris paribus*, they are in the same ratio) of proof spirit as denoted by the number 100, some other number between unity and 170 will represent that of every other spirituous compound.

If, for example, we mean to express a kind of spirit of such a strength, that, on 100 parts of it by measure at the existing temperature being reduced to 60° of Fahrenheit's thermometer, and then made up to 134 with water, it should become equal to proof, we should call it "134," or "34 per cent. over proof;" adding the quantity of the concentration, if we should wish to ascertain the necessary quantity of water for this purpose, which would in this case be about 2 parts more, or 36 parts in the whole. If we speak of a spirit of which 100 parts would be producible by the addition of water to 80 of proof spirit, we should call it "80," or "20 per cent. under proof." It appears to the authors that it would save unnecessary periphrasis, and be more convenient in several respects to use the former of these modes of expression, and omit the terms *over proof* and *under proof* altogether in the denomination of these strengths. If this system were adopted, the values of all spirituous liquors, when equal in other respects, would be in the direct ratio of their per-centages thus appreciated, and the duties might be estimated accordingly. If proof paid 5s., 120, or 20 per cent. over proof, should pay 6s.; and 80, or 20 per cent. under proof, only 4s.: and, in short, the relative values of proof spirit and of any given compound, or of any two liquors of different strengths, or the equivalent quantities of each, would in this case be to be determined in a moment.

We shall here, merely for the purpose of illustrating its advantages, give rules for the solution of two or three questions which occur hourly with respect to this subject, with an example or two to each, founded on the supposition of the establishment of that system of denomination which is recommended in this section.

I. *To determine the Value per Gallon of Spirit whose Per-Centage is known, when that of Proof of the same Value in other respects is given.*

Practical Rule.—*Multiply the value per gallon of proof by the per-centage of the liquor, and the product, pointing off two decimals on the right, gives the answer.*

EXAMPLES.

1. What is the value per gallon of rum of 127 (or 27 O.P.), when proof of the same quality in other respects is at 14s.?

Ans. It is worth 168d. = 14s.) × 127 ÷ 100 = 213·36 pence, or 17s. 9⅓d. per gallon.

2. What is the value per gallon of rum at 73 (or 27 U.P.), proof being worth 14s.?

Ans. It is worth 168d. (= 14s.) × 73 ÷ 100 = 124·64 pence, or 10s. 4⅔d. per gallon.

II. *To determine the Value per Gallon of any Spirituous Liquor differing in Strength from Proof, when that of any other Liquor, which is similar in all other respects except Strength, is known—the Per-Centage of each being given.*

Practical Rule.—*Multiply the value per gallon given by the per-centage of the liquor whose value is required; divide the product by the per-centage of the liquor whose value is given, and the quotient is the answer.*

EXAMPLES.

1. What is the value of brandy of 115 (or 15 O.P.) when that of 87 (or 13 U.P.) is 11 s. per gallon?

Ans. It is worth 132 d. (= 11 s.) × 115 ÷ 87 = 174½ pence, or 14 s. 6½ d.

2. What is the value of brandy of 87 (or 13 U.P.) when that of 115 (or 15 O.P.) is 14 s. 6½ d. per gallon?

Ans. It is worth 174·5 d. (= 14 s. 6½ d.) × 87 ÷ 115 = 132 pence, or 11 s. per gallon.

III. *To determine the Quantity of Spirit of any given Strength which is equivalent in Value to a given Quantity of any other Strength.*

Practical Rule.—*Multiply the quantity given by the per-centage of the liquor; divide the product by the per-centage of the liquor whose quantity is required, and the quotient is the answer.*

EXAMPLES.

1. How much Hollands of 113 (or 13 O.P.) is equivalent to 556 gallons of 94 (or 6 U.P.)?

Ans. 556 × 94 ÷ 113 = 462½ gallons.

2. How much Hollands of 94 (or 6 U.P.) is equivalent to 462½ gallons of 113 (or 13 O.P.)?

Ans. 462½ × 113 ÷ 94 = 556 gallons.

§ 27. It will, perhaps, be expected from the authors of this essay that they should here enter into a considerable detail respecting the various constructions and uses of those instruments which they are known to have manufactured so extensively for several years past for these purposes. This it is by no means their intention to do in the present instance; a few observations, however, on the subject in general may not be unimportant.

It has been a very common error amongst the makers of these instruments to conceive that their stem should be very slender in proportion to their bulb; a construction which rendered a great number of weights necessary, and which was by no means conducive to their accuracy. The surface of a slender stem is proportionally greater when compared with its solid content than that of a small one: the capillary attraction, therefore, and the weight of the liquor which adheres to it, both operate more powerfully. If we take any of those instruments whose stem is a mere wire, we shall find them, from these causes, so sluggish in their motion that they will generally stand at any point of the stem at which they are placed, within a quarter of an inch of a certain part of it, whereas those with a thicker stem regain the same point instantaneously. The thicker the stem, therefore, the better, so as it be only within such limits as to render the difference in the points at which the instrument will stand, in two liquors which differ in a very minute degree with respect to their specific gravities, sufficiently perceptible.

Let us calculate for a moment how many weights we must have to a spirit hydrometer. The stem may very conveniently be 4 inches long, and in this length we could easily graduate 40 or 50 divisions, so that a difference of one-fourth of each shall be very visible. But we want to express a difference of nearly 200 in specific gravity (or from 800 to 1000) by this instrument within about one-fourth of an unit. We shall therefore do it extremely well by the virtual lengthening of the stem to four or five times its real length by means of three or four weights. It is true, the divisions, unless they be arbitrary ones with a scale of reference, are not equal; if they be units of specific gravity, they must be graduated from several harmonic scales: but this will not affect the truth of our general deduction; and three or four weights, therefore, will be fully sufficient for the nicest spirit hydrometer, whatever scale of graduation we may use on its stem.

This kind of instrument would be far preferable, even if superior accuracy were alone the object, if simplicity were entirely out of the question, and 40 weights as easy and as little liable to error in their application as 4: but when we reflect that simplicity and facility of use are, perhaps, even of more consequence than accuracy in the result, the difference in the advantages of these modes of construction becomes enormous.

It has been a favourite opinion with some gentlemen, that glass hydrometers would be preferable to metallic ones, because they would not be liable to have their bulk altered by any contusion without being broken to pieces; whereas the latter might be so delved by rough usage as to give fallacious indications of strength without its being perceived by the owner of them. The fact is, however, that although, for certain purposes, where metallic instruments cannot be used on account of their being liable to corrosion (as in the case of the mineral acids) we are obliged to employ glass, yet they are by no means capable of being rendered so accurate as those which are made of metal. The proportion of the bulb to the stem, and the consequent extent of the graduations, are, of course, within certain limits, matters of mere chance; and they are, therefore, not the subject of calculation. Every such instrument can only be graduated by direct experiment: and to those who would contend for the eligibility of such a method, where the other can be used, we can only say, that they have not sufficiently considered the subject. The metallic stems and bulbs, on the other hand, are by the tools employed for these purposes capable of being reduced so accurately to the required bulk, that the error in any point of the scale on this account will not amount to one-tenth of an unit in the specific gravity. Nor is it true that it is easily possible for the effect of such an accident to pass unnoticed by any one who ought to be entrusted with the use of an instrument at all. The least want of proper convexity in the bulb strikes the eye in an instant, even before its effect would be such as to be perceptible in the indication on the stem; and if it were not so, the simple operation of immerging the hydrometer, loaded with its proper weight in distilled or rain water at 60°, would at once detect such a circumstance, so that no probable inconvenience can ever result from such a cause,

[To be continued.]

LVII. *On the Purification of Nickel, with some Remarks on the Solutions of metallic Oxides in Ammonia, &c. By Mr.* R. PHILLIPS, *Member of the Askesian and British Mineralogical Societies*[*].

ALTHOUGH nickel has been discovered more than a century, yet its existence as a peculiar metal has not been so well established but that several chemists, even of late years, have entertained considerable doubt on the subject. This has probably been occasioned by the difficulty in separating it from the metals with which it is usually mixed. Of these, copper and cobalt, and more particularly the latter, strongly resemble it in some of its chemical habitudes. On account of its magnetic property, it has been supposed impossible to divest it of iron; and until the method lately adopted by R. Chenevix, esq. no certain means appear to have been known for the separation of the arsenic.

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[*] Communicated by the Author.

In order to purify this metal the following method may be adopted, which includes that above alluded to for the separation of the arsenic. Let nickel be dissolved in dilute nitric acid to complete saturation; to the filtered solution add nitrate of lead in sufficient quantity to precipitate the arsenic acid. If more should be employed than is required, the excess occasions no inconvenience. Having separated the arseniate of lead by the filter, add a small quantity of nitric acid to the solution, and immerse a bar of iron to precipitate the copper. This being done, there remain in solution the oxides of nickel, cobalt, iron, and lead, which may be precipitated by carbonate of potash. The precipitate, after sufficient washing, is to be put, while moist, into a solution of ammonia, which, dissolving the oxides of nickel and cobalt, leaves those of iron and lead to be separated by filtration.

All that is now necessary is to separate the oxides of cobalt and nickel, in order to complete the purification of the latter: but before the experiments for this purpose are related, it may not be amiss to state the properties of the ammoniacal solutions of some of the metallic oxides, although the habitudes of all of them are not immediately connected with the present inquiry. These I prepared by precipitating the oxides from their solutions in acid, and putting them, while moist, into solution of ammonia. Notwithstanding this is the most favourable state for the ammonia to act upon them, yet in every case, even after long digestion, the ammonia is in excess, since a part of it may be evaporated without causing any precipitation of the oxide. As these solutions possess different properties when the ammonia is in excess and when it is not, in the following experiments I shall call the former *solution*, and the latter *evaporated solution*. The evaporation was continued until moist turmeric paper, when held over the solutions, suffered no change.

Solution of oxide of silver in ammonia. Colourless: is not decomposed by water. *Evaporated solution*—Speedily decomposed by water: the precipitate blueish white, becoming gradually brown by exposure to light.

Solution of oxide of copper in ammonia.—Colour purple.

It is not decomposed by water.—The *evaporated solution* is immediately decomposed by it.

Solution of oxide of cobalt in ammonia.—Colour deep red; not decomposed by water. Nor is the *evaporated solution* decomposed by water, even when so diluted as to be nearly colourless.

Solution of oxide of nickel in ammonia.—Colour greenish blue: slowly decomposed by water. *Evaporated solution*—Colour pure green: decomposed immediately by water.

As silver and copper may be easily obtained from their solutions by well-known methods, the above-related experiments are more particularly applicable for the separation of cobalt and nickel; especially as the properties of their solutions differ very materially. But as the precipitation by water is often incomplete, and always inconvenient on account of the quantity necessarily employed, without examining the extent of its effects in the present instance, I tried other methods to effect the separation of these oxides, and, after some fruitless attempts, found potash answer this purpose extremely well. When a solution of it is added to the ammoniacal solutions of metallic oxides, the effects produced are as follow:

Solution of oxide of silver in ammonia.—Slowly decomposed, requiring three or four days for its completion. The precipitate is blackish brown. I have not examined whether it is fulminating. *Evaporated solution*—Immediately decomposed: the precipitate is of a lighter brown than the former.

Solution of oxide of copper in ammonia.—Slowly decomposed, and in very small quantity. *Evaporated solution*—Quickly decomposed.

Solution of oxide of cobalt in ammonia.—Very slowly and sparingly decomposed, even by large quantities of the solution of potash, and more slowly as the solution is more dilute. *Evaporated solution,* by the addition of potash, gradually changes from red to pink; then becomes scarlet, at length turns brown, and deposits brown oxide of cobalt. If a quantity of solution of potash be added to a small quantity of the evaporated solution, precipitation ensues in a few hours; but if a considerable quantity of water be added to similar quantities of the evaporated solution, and of potash, five or six days are required to complete the precipitation.

Solution of oxide of nickel in ammonia.—Immediately decomposed, and the more readily as it is more diluted. *Evaporated solution*—The same properties in a greater degree.

By comparing the above statements it appears proper, for the separation of the oxides in question, that the solution containing them should have the ammonia in excess, and be largely diluted before the addition of the potash. The excess of ammonia prevents, for a considerable length of time, any precipitation of the oxide of cobalt, while it produces no delay in the precipitation of the oxide of nickel. Mere dilution precipitates a part of the oxide of nickel; and at the same time that it renders the remainder more easy of precipitation, it prevents for several days any deposition of the oxide of cobalt. The following experiment will show that this method may be relied upon:

To a measure of solution of oxide of nickel in ammonia I added solution of potash as long as precipitation took place. The precipitate was washed, dried, and weighed. As it appeared probable that all the oxide of nickel might not be precipitated, I heated the solution after filtration till the ammonia had evaporated, but I did not obtain any further quantity of oxide.

To a similar measure of the same solution of oxide of nickel I added a measure of solution of oxide of cobalt in ammonia, and precipitated by potash. The precipitate appeared to be pure oxide of nickel; and, after drying, its weight did not differ 1-10th of a grain from that of the oxide of nickel obtained in the former experiment. This experiment was repeated with nearly similar results.

Soda produces the same effects as potash, and appears to act rather more readily; but whether in smaller quantity than the latter I have not tried. Carbonate of potash produces no effect. The decomposition of the ammoniacal solutions by potash appears to depend upon a combination of the two alkalies; the compound possessing, as is usual in chemical combinations, properties either partially or totally differing from those of its constituents. That this precipitation does not depend upon dilution has, I think, been already shown; and the following experiments more clearly determine that it must be ascribed to some other cause:

To one measure of solution of ammonia I added three measures of water. Moist oxide of nickel put into this dilute solution quickly coloured it. After several days digestion the solution decomposed by potash furnished nearly four grains of oxide.

One measure of solution of ammonia was mixed with three measures of dilute solution of potash. After as long digestion with moist oxide of nickel as in the former case, the solution had not acquired any colour; and on evaporating the ammonia no oxide of nickel was obtained.

As far as I have examined, the solutions of oxides in carbonate of ammonia possess the same properties as those in ammonia; for although carbonate of potash does not decompose the latter, yet the former are decomposed by potash. This is easily explained. Potash has a stronger affinity for carbonic acid than ammonia has: when, therefore, a carbonated ammoniacal solution is decomposed by it, a part of the potash combines with the carbonic acid of the carbonate of ammonia, and the remaining part decomposes what by the action of the other has become ammoniacal solution.

Considering that the supposition of a combination of the two alkalies would be strengthened if the experiments which gave rise to it could be reversed, i. e. if it were possible to precipitate with ammonia substances dissolved by potash, I dissolved in the latter silica, alumina, and several metallic oxides. On adding ammonia to these solutions, I at first thought I had succeeded in obtaining precipitates; but upon examining the ammonia employed, I found that it contained a small quantity of carbonic acid; and that when ammonia free from it was used, no precipitate was in any case obtained. Although these experiments did not succeed in supporting the above supposition, yet they are by no means fatal to it. It is probable that potash has a stronger affinity for the substances which it dissolves than it has for ammonia; and in this case, as no combination could be effected, no precipitation would ensue. In the first experiments potash and ammonia seem to possess a stronger affinity for each other than ammonia has for the oxides soluble in it.

Judging by the effects produced by water and potash, the affinity of ammonia for the metallic oxides appears to be as follows:

Oxide of cobalt,
Oxide of copper,
Oxide of silver,
Oxide of nickel.

I reduced a quantity of the oxide of nickel, obtained by the above-described process, and obtained a button of metal which exhibited the following properties:

Colour—Dull yellowish white.
Fracture—Foliated. Specific gravity 8·51.
Fragile, but capable of slight extension by hammering.
Strongly magnetic.

In one of the late French journals it is proposed to separate the oxides of cobalt and nickel by oxidizing the former by means of the hyperoxygenized muriatic acid. I have tried the method recommended without success.

LVIII. *On Machines for measuring Elasticity. By a Friend to Physical Inquiries.*

To the Editor of the Philosophical Magazine.

SIR,

A FEW years ago an eminent botanist, now abroad, being desirous of ascertaining the comparative elasticity of different woods, was wishing to have some machine made for that purpose; but whether or not any such machine has been yet made I am not acquainted. At the same time the subject was mentioned the following contrivances occurred to my mind, which, if carried into execution, might in some measure answer the purpose; and if you think them worth communicating to the public, I shall be glad to see them noticed in your magazine. They may serve for some of your readers as hints to improve on, and be the means of some much more accurate method of ascertaining the elasticity of bodies being invented.

First Contrivance.—A machine might be made which should have a flap on which balls of different sorts of wood, or other substances, might be placed, and suddenly let fall on a slab of ivory or marble: on this machine there should be an upright graduated post in order to see to what height each ball rebounded; by which, in *some* instances, the comparative elasticity of the body subjected to trial might be judged of.

Observations.—This method, in order to be accurate, should be tried in the exhausted receiver of an air-pump; otherwise with *light bodies*, such as cork and elder-pith, both very elastic, the experiment will not answer. The instrument for performing the common *guinea-and-feather* operation might, perhaps, be as good a contrivance as any: a wire cage might be placed under the receiver in order to protect the glass. Bodies of nearly the same gravity might be tried in the air. The standard ball might be ivory let fall on ivory, or marble let fall on marble.

Second Contrivance.—Thin slips of different sorts of wood, &c. might be bent half round a circular piece of board, and suddenly let loose to fly back by their own force against a piece of wood hanging like a door on hinges*. The piece of wood should move on a graduated arch, in order to determine the degree of force with which the different slips fly back.

Observations.—How far this contrivance will answer I am not able to determine; but it appears on consideration that some judgment may be formed of the elastic quality of bodies by this method, where the substance is capable of being cut into slips, which will bend easily and recover their form again. Should either of these proposed schemes, or others more preferable, be adopted, it appears to me that the knowledge obtained by them might be of considerable use in various branches of art, as well as satisfactory to the inquiring naturalist.

London,
September 7, 1803.

LIX. *Account of two Fœtuses produced by the same Parrots which in the Year* 1801 *produced a young one at Rome. By* C. L. Morozzo, *Member of the Institute of Bologna, of the Italian Society of the Sciences, of the Academy of Stockholm, and of that of Padua* †.

In the letter which I addressed to C. Lacépède in the year 1801 on the birth of a parrot at Rome (see Philosophical Magazine, No. XLVII.), I contracted an engagement with the public, as I promised to employ all my care on the eggs which these parrots might lay in the spring of the next year (1802). I shall now discharge my promise; and though my care was not crowned with success, I flatter myself that the amateurs of natural history will find in it some interesting details.

I did every thing in my power to get again into my hands these parrots, promising to the owner to take the greatest care of them possible. By these means I should have been enabled to observe more exactly their habits; but J. A. Passeri, the owner of them, who was governor of Orvieto, gave absolute orders that his son, in whose keeping they had been the preceding year, and at whose house I had made all my observations, should send them to him.

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* Or it may be made to strike the end of a rod or arrow in the manner of the catapulta. The distance to which the arrow is carried will determine the elastic force.—Edit.

† From the *Journal de Physique*, Floreal, an. 11.

These birds had already begun their amours. No reasons were able to persuade him to recall his orders. I was therefore obliged to suffer them to depart: happily they sustained no injury by the way.

I therefore employed the only resource that remained; which was, to request him to take every possible care of them; to cause materials for building their nests to be placed within their reach; and to transmit to me a journal of every thing that might take place. I even delivered to him a small memorandum in regard to the observations which I more particularly wished him to make.

He informed me, that on the 21st of June the female deposited her first egg; that on the 25th she deposited another; and on the 29th a third. I flattered myself with the happiest success; but though the female sat on them continually, after the term of forty days, which we found the preceding years to be the term of incubation, observing that the first egg had been dragged out of the nest, and that the other two had been bruised, we thought proper to examine them; and the following is a literal translation of the procès-verbal, written in Italian, which was sent to me on the occasion:

“ We the undersigned, physician and surgeon of the town of Orvieto, declare to whom it may concern, that we saw in the house of J. A. Passeri, governor of the town, two parrots of the kind called Amazons, viz. a male and a female, and that on a requisition from the said governor we paid several visits to these birds after the female seemed ready to lay. We observed that the female on the 21st of June 1802 deposited an egg in a nest which she had constructed under a chest of drawers, and lined with linen rags which had been placed within her reach.

“ On the 25th of the same month she deposited a second, and on the 29th a third.

“ We observed that the female sat on the eggs, and that she continued to do so with great assiduity, and that during the whole time the male supplied her with food.

“ On the 18th of July we observed the first egg she had laid to be without the nest: a small round hole, suspected to have been made by the beak of the mother, was found in one side of it. On this egg being opened, nothing was found in it but a little coagulated matter in one corner, almost black, and putrid, without any appearance of an embryo.

“ On the 22d of the same month we examined one of

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the eggs which was in the nest, which was cracked, and depressed on one side, without any hole being observed as in the former. On the 24th it was remarked that the third egg had the same depression and fracture as the other.

"The female, however, continued her incubation with great assiduity; and on the 2d of August, finding that the eggs would not hatch, and that the fœtus was dead, we determined that day, in presence of the canon Felix Albarici, and M. Bernard Piermattei, merchant of that city, to break the eggs with great caution; and when this was done we found the following results:

"In one of the eggs we found the shell very hard, with an almost round depression in one of its sides: on the shell being broken it was observed that the pellicle by which eggs in general are enveloped was dry, and adhered to the fœtus. This pellicle being gently removed, we found the fœtus, which was completely formed, and we distinguished in it a small parrot. We observed that the head was placed between the thighs, with the beak large and long in the upper part, the tongue thick, the eyes shut, and covered by the eyelids. It had the two wings formed, as well as the thighs and legs, with claws on the toes, two of which were before and two behind. On the upper part of the head, the back, and as far as the tail, it was covered with white down; on the breast and belly there was none; on the middle of the belly was observed a carneous mass adhering to it, which resembled the placenta. We found also near the thorax a depression which corresponded to the contusion observed in the shell, and which there is reason to suppose was the cause of the death of the young.

"In the shell of the other egg a contusion was found as in the first: the pellicle and fœtus were completely formed, though smaller than in the preceding, which gave us reason to believe that this egg had been the last that was laid. In this fœtus were observed on the right wing, and near the thorax, a depression and contusion corresponding to the fracture in the egg.

"We remarked that the beak of the former was of an ash colour, like that of the male, and that the beak of the latter was blacker, like that of the female: we therefore supposed that the first was a male, and the second a female.

"These two fœtuses were put into a flask with spirit of wine, closed and sealed in the presence of the above witnesses, to be kept as an authentic testimony of the prolific powers of these two parrots; which did not surprise us, as we saw them produce young ones at Rome the preceding year, and as that produced in 1801 is lively, and now in the possession of the princess Venosa."

The witnesses who attest the above are:

Dr. Louis Bernardi,
Joseph Taruchi, surgeon,
Felix Alberici, canon,
Bernard Piermattei.

It is to be regretted that the hatching of these parrots has not been attended with the same success as it was last year.

We here see that the climate of Orvieto, which is more temperate than that of Rome, was no obstacle to the copulation or laying of these birds. What in all probability prevented the female from hatching the eggs was, that the parrots were not in separate apartments as the preceding year, and that they were deranged by the too great number of people who approached them either through curiosity or by accident, since they were placed in a chamber which served as a kitchen.

These birds are suspicious and timid, and it is probable that the female bruised the eggs in endeavouring to change their position with a view of placing them in more safety.

But as these birds, according to my calculation, are in the vigour of youth, it is to be hoped they will produce young hereafter; and that the owner, taught by the experience of this year, will take the necessary precaution of placing them in an apartment less exposed to noise and bustle, where the solitude of the place may bring them nearer to the wild state, and suffer them to complete their incubation as during the preceding year.

Turin,
September 25, 1802.

LX. *Account of a new Spring of Petroleum discovered in Italy; in a Letter to the Editors of the Annales de Chimie.* By J. Poggi *.

Paris, Dec. 22, 1802.

Being detained in the capital by the great means it affords of applying to the study of nature, I think it my duty to communicate to you an account of an interesting phænomenon with which nature has enriched my country. It consists in a very abundant and permanent spring of *petroleum* or *naptha,* which has appeared for some months at

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* From the *Annales de Chimie,* No. 134.

Amiano, a village in the state of Parma, near Josnovo and Varese, on the confines of Liguria. Being vexed that the government of the country did not take this object into consideration, I have heard lately with satisfaction that the government of Liguria has converted it to a useful purpose. After being analysed by an able chemist, it has been applied with great advantage to the purpose of lighting the city of Genoa.

It was C. Mojon, professor of chemistry in the university of Genoa, who made the most conclusive experiments on this combustible substance. He read a report on this subject before the National Institute of Liguria on the 4th of July 1802; and it is an extract from this report, procured from the author himself, that I now have the honour of presenting to you.

Professor Mojon, having been on the spot, was enabled to see that the richness of the spring of petroleum at Amiano is so great, that, though people have continually drawn from it since its discovery, it still keeps at the same level. He has found in it the following characters:

This petroleum is exceedingly limpid, of a vinous yellow colour, or rather like that of the topase of Saxony. Its smell is strong, penetrating, and less empyreumatic than that of the common and brownish petroleum. Its specific gravity is to that of water as 83 to 100, and to that of olive oil as 91 to 100.

If a few drops of it be poured on writing paper and exposed to heat, it is entirely volatilised, and leaves no stain. If distilled in a glass retort, in a moderate heat, it passes entirely into the receiver, and leaves no residuum.

If it be mixed with concentrated sulphuric acid, it burns, becomes thick and dark-coloured. It readily dissolves amber, sulphur, and resins; and when mixed with gum copal forms a varnish, which, when spread over a piece of board, and exposed for some days to the sun, dries, and loses its odour. When poured on alcohol, it floats at the surface without dissolving, even when heated and strongly stirred. It floats also over fixed and volatile oils, with which it combines by agitation. Its combustibility is so great, that it inflames when brought near to a burning body: it seems even to attract the flame in consequence of its great volatility. Its own flame is white and lively, like that of oil of turpentine, but it emits much more smoke.

Having thus determined the characters of petroleum, professor Mojon, mindful of his commission, examined whether it might not be employed for lighting the city of Genoa. He made experiments to ascertain the intensity and degree of the light which this liquid produces in comparison of olive oil; and was able to make an exact calculation how far it would be advantageous to employ it.

He put an ounce of petroleum into a glass lamp; added a flat wick of four lines in breadth, placing it in such a manner that the bottom of the flame should be about an inch above the fluid. He then put an ounce of olive oil into another lamp of the same kind, and kindled both lamps at the same time. They gave a flame equally strong, and a light of the same intensity; but with this difference, that the former alone produced a little smoke, and burnt an hour and a half, while the lamp containing the olive oil burnt an hour and thirty-five minutes. Both of them were then dry, without leaving any residuum.

In order to try whether the smoke might not be destroyed or diminished, he burnt the petroleum in a lamp with a current of air furnished with a glass cylinder. He indeed found that the liquid, while it burned completely with a white and very bright flame, emitted no longer any smoke or bad smell. The combustion was so rapid that the flame was agitated by it. Having then introduced into the same lamp a mixture of equal parts of petroleum and olive oil, he obtained a slower combustion, with a more tranquil and uniform flame. He obtained the same result by the union of these two substances in a reverberating lamp with a wick of about an inch in breadth.

From these experiments he concluded that this petroleum might be advantageously employed for lighting the streets, observing the following precautions:

1st, That the flame should be about an inch above the oil.

2d, That the lamp should be covered and closed in such a manner as to prevent the flame from being communicated to the petroleum.

3d, To use a flat wick, to prevent smoke and render the combustion complete.

It was in consequence of this report of professor Mojon that the Ligurian government ordered petroleum to be employed for lighting the streets of Genoa. It is used at present without any mixture: the reverberators are constructed with the improvements above indicated; but care has been taken to add a kind of conical tube or chimney of tin plate, to convey off the smoke which may be disengaged. By these means the same quantity of light is obtained as with olive oil, and at a fourth of the expense, as the petroleum costs only two Genoese sous per pound, (which is less than a penny English.)

LXI. *Report on the Discovery of a Deposit of bituminous Wood. Read in the Ligurian Institute July* 1802 *by* C. Mojon, *public Professor of Chemistry* *.

At the period when nature opened in the state of Parma, on the north side of the Appenines, an extraordinary spring of petroleum †, it presented on the opposite side, in Liguria, at the distance of about fifteen leagues, a considerable depôt of bituminous wood. The discovery of this fossil is even a little more recent, and is equally interesting to the naturalist and to the state. For a knowledge of it we are indebted to the useful labours of professor Mojon, as will be seen by the following extract from the report which he made to the National Institute of Liguria:

"Castel Nuovo is a country of Lunigiana on the confines of the Italian republic. It is in the plain of that country, half a league from the mouth of the Magra, that the mine of the substance in question has been discovered. It is situated in a soil formed of argillaceous and calcareous strata, more or less thick, and inclined in different directions throughout their whole extent. The nature of the fossil, as well as the constitution of the soil, evidently shows that these strata were formed only by great floods, which carried with them and buried whole forests ‡. The extent of this deposit, however, cannot be determined, because the pits proposed to be sunk at different distances for that purpose have not yet been constructed. Hitherto one only has been made, about 40 feet in depth, the bottom of which is inundated by a spring that issues from an excavation attempted in a lateral direction; and it is to be observed, that in this pit no disengagement has been remarked of carbonic acid gas, nor of those gases generally developed from such excavations.

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* From the *Annales de Chimie*, No. 135.

† See the preceding article.

‡ Is there not reason to believe that the *lucus sacer* of the antient *Luna* must form part of this deposit? The ruins of the town, in digging among which monuments worthy the attention of the antiquary have lately been discovered, are found very near, and at the distance of the third of a league from the mouth of the Magra.

"This bituminous wood, which in some places appears almost uncovered, at the surface of the ground still retains its primitive figure. In the course of the search which has been made by digging, trunks of different thickness up to two feet diameter have been dug up; and among these there are some compressed, which exhibit in their transverse section an elliptical form.

"The colour of this fossil is sometimes a perfect black, sometimes a grayish black, and sometimes a wood brown. The blackest is sometimes pretty shining, and has even the splendour of glass; the grayish black, and brown, are dull, but shine very much on being polished.

"Its texture is entirely that of wood, as it has not been altered by the bitumen with which it is impregnated; so that in some pieces the tree to which they belonged may be determined: and in particular those which have the transverse fracture conchoid and shining, and which are perfectly black, exhibit all the characters of fir: others have a texture similar to that of oak.

"It is not so brittle as coal, and, when sawn in a direction perpendicular to the axis of the trunks, exhibits a solid, compact, and very smooth surface. The concentric strata of the fibres of which the trunk is composed may be easily separated by introducing the point of any instrument whatever.

"It readily kindles, and without the aid of any other combustible: it gives in burning a lively and brilliant flame. The heat it produces is more intense and durable than that of any other vegetable coal; and, when once kindled, never becomes extinct till entirely consumed, leaving very little ashes. The combustion may be interrupted and renewed at pleasure.

"Lighter than coal, its specific gravity is to that of distilled water as 1235 to 1000: the fragments which exhibit the characters of oak are still lighter.

"Sulphuret of iron is sometimes found in this fossil disseminated in lumps and small grains, which when long exposed to the air are decomposed, and cause the fragments to which they belong to fall in pieces.

"It has besides the properties common to coal: but if naturalists have long entertained doubts respecting the origin of coals, in consequence of the variety of their texture, the irregularity of their form, their fragility, their strata analogous to those of schistous stones, &c., the fossil of Castel Nuovo presents no difficulty to their researches. The uniformity of the tissue; the constant direction of its fibres;

the bark of the different trunks; its knots; fracture; the colour of some kinds; the facility with which it inflames; the alkali contained in its ashes;—all these characters, very distinct, render it perfectly similar to charred wood.

"It must also be added, that it possesses this singular property, that it is susceptible of being worked in the lath with the greatest facility, and of acquiring a polish and splendour which render it superior to ebony.

"In the last place, it is a conductor of the electric fluid, and transmits it with facility."

After this short account of the situation and nature of this fossil, I shall describe some of the experiments which the author made, in order to determine what advantages may be derived from the employment of this substance.

He burnt 12 pounds of this bituminous wood in a furnace, adapting to it a thermometer at the distance of eight inches, the temperature at the time being 10 degrees of Reaumur. Sixteen minutes after, the whole wood was completely kindled, and gave a clear brilliant flame, with less smoke than coal and a slight odour of bitumen, which not being sulphureous did not incommode those persons even who were nearest to it. The thermometer had risen to 42 degrees. Twenty-four pounds of water, which were in a copper vessel on the furnace, then began to boil. The thermometer stood at the same degree for 12 minutes, and the water continued to boil for 20. When the ebullition had ceased the thermometer fell to 26 degrees, and the water was reduced to three pounds. The fossil was entirely consumed in the course of an hour and a quarter, and left 225 grains of yellowish impalpable ashes in the form of flakes.

He then repeated the experiment under the same circumstances, and with oak charcoal. The latter kindled in twelve minutes, and in four more the 24 pounds of water contained in the copper boiler entered into ebullition. The thermometer rose to 38 degrees, at which it maintained itself for 18 minutes, and when the ebullition ceased the thermometer fell to 25 degrees. Almost the whole of the water was evaporated, and the coal, being consumed in the course of an hour and a half, left only 154 grains of ashes.

By this experiment, calculating the sum total of the heat respectively extricated during the combustion of the two substances, the author was able to remark, that the bituminous wood produced 816 degrees of heat during an hour and a quarter; while oak charcoal, in an hour and a half, produced only 784.

He also tried whether this fossil could be used with advantage in forges for forging iron. He kept at a red heat for some time the extremities of an iron rod, one in bituminous wood and the other in oak charcoal, employing equal parts. When the iron rod was taken out, he found its extremities equally ductile, malleable, and tenacious. There is every reason to think that this new combustible may even be employed in the reduction of iron ore; but this the author has not been able to ascertain, for want of materials.

Professor Mojon terminates his report with the following analysis:

This fossil wood gives almost the same products as coals, if we except the ashes, from which a little potash is extracted.

By distillation he obtained phlegm, yellowish bituminous oil, a quantity of carbonic acid gas, carbonated hydrogen gas, and an empyreumatic oil thicker than the former.

In alcohol a portion of this wood, at the end of some time, gave a blackish resinous substance.

By ebullition in distilled water it suffers to be precipitated calcareous earth and argil.

By pouring nitric acid over this fossil, it is decomposed, disengaging nitrous gas.

In the last place, the 225 grains of ashes, obtained by the above combustion of 12 pounds of this fossil, gave by lixiviation and filtration 1½ grain of potash, oxide of iron, alumine, lime, and magnesia.

LXII. *Memoir on aëriform cutaneous Perspiration. By* C. Trousset, *M. D. Professor of Natural Philosophy and Chemistry in the Central School of the Department of Isere, &c.*[*]

Physicians at all times have endeavoured to ascertain the influence which the air has on the human body; but how can we conceive that the antients, who were not acquainted with the gravity of that fluid, should have been able to determine its action? If we therefore except Hippocrates, who formally asserts in his works that air is digested in the lungs as the aliments are in the stomach, his contemporaries and successors have left us on this subject incoherent ideas, often ridiculous, and always erroneous, the fruits of an imagination not guided by any certain experience.

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[*] From *Annales de Chimie*, No. 133.

If I intended to consider the influence of the air under every point of view, I should begin with respiration; a function of so much importance, that without it life could not exist, while it alone can for some time maintain and preserve it.

But the modern chemists, after an exact analysis of atmospheric air, have given a theory of respiration so ingenious, so complete, and so well founded on correct experiments, that all the attempts hitherto made to overturn it have only tended to establish it more and more.

I could therefore only repeat here what has been said before me by Lavoisier, Seguin, Crawford, Fourcroy, Chaptal, &c.; and this matter has been so well elucidated by the labours of these celebrated chemists, that it is in some measure exhausted; and their theory in this respect has been so widely diffused, that it is now adopted by all men of science. I shall therefore abstain from repeating what would be here tiresome and superfluous.

But if what takes place in the lungs is exactly known, the same light has not yet been thrown on the functions of the skin. Independently of cutaneous perspiration, observed with so much care and correctness by Sanctorius and several others, does there escape through the skin one or more aëriform fluids? and, in this case, of what nature are they? Such are the two questions which I purpose to examine in this memoir.

The antients had no idea of this aëriform cutaneous perspiration, and they make no mention of it in their works.

Count de Milly first announced, in 1777, the discovery of an elastic fluid * which escapes through the skin. He asserts that a person in a warm bath may collect half a pint of it in the course of three hours; and it results from his analysis, which indeed is very imperfect and incorrect, that it is fixed air (carbonic acid gas).

Dr. Ingenhousz announced some time after that an aëriform fluid escapes through the skin; but he believed it to be phlogisticated air (azotic gas.)

Dr. Priestley and M. Fontana repeated the experiments of the two preceding philosophers, and observed no aëriform emanation through the skin.

M. Jurine, a surgeon at Geneva, being desirous to become a candidate for the prize proposed by the Royal Society of Medicine †, repeated the experiments of Milly and Ingenhousz,

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* Mem. de l'Acad. Royale des Sciences de Berlin, ann. 1777, p. 32.

† See Histoire et Memoires de la Societé de Medecine, tom. x. p. 54 et seq.

Ingenhousz, both on himself and on several individuals of different ages, employing different kinds of water, the temperature of which he varied, and asserts that he never observed the least aëriform emanation. Presuming that the water, according to its gravity, might impede the escape of the air, or that it might crisp the exhaling vessels of the skin, he continued his researches, varying the processes employed by Dr. Priestley and M. Fontana; and he thinks he has proved by experiments, the inexactness of which might be easily shown, that a small quantity of fixed air (carbonic acid gas) is continually escaping through the skin.

Fourcroy on this subject expresses himself as follows: " It is not true that elastic fluids, and particularly carbonic acid gas, escape through the skin, as some of the moderns have asserted *.

Such, a few years ago, was the state of the question which forms the subject of this article. Incorrect experiments, the contradictory results of which were contested either in the whole or in part, left philosophers in uncertainty, and seemed to call for new researches, in order to fix the opinion of philosophers on this point.

I often reflected on it, and had formed a design of employing myself with it, when in the spring of the year 8, being near one of my patients who was in the bath, I perceived that he was entirely covered with small bubbles of air. The hairs on his body were surrounded by bubbles decreasing from the base to the summit, so that a great number of them exhibited the appearance of pyramids more or less elevated. I made all these bubbles disappear; but in the course of half an hour they were succeeded by an equal number. In consequence of observing this phænomenon, I caused my patient to continue his bathing; and having collected several bell-fulls of this gas, I examined it carefully several times, and found that it was azotic gas, perfectly pure, without any mixture of carbonic acid.

I was then desirous to know whether this phænomenon was general, or whether it depended on the pathological state of the subject. I made experiments on myself, and on several other individuals, but never observed any thing of the kind.

In the beginning of the year 9 I communicated my experiments, and the result of them, to C. Fourcroy, who requested that I would continue them. Encouraged by the

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* Systême des Connoissances Chimiques, tom. ix, p. 203.

approbation of so celebrated a professor, I resolved to do so. It was not sufficient that I was certain of the exactness of my experiments; it was necessary also that I should convince philosophers of it.

Having collected with great care, in the spring of the year 9, a certain quantity of this gas, I filled with it a small bell 10 lines in diameter and 8 inches in height. A taper was immediately extinguished in it eleven times in succession.

It traversed lime-water without rendering it turbid, and without decreasing in volume.

It experienced no change from ammoniacal gas.

It produced no alteration in blue vegetable colours.

Phosphorus which remained immersed in it for more than a month had not decreased in volume.

I thought these experiments more than sufficient to convince me that the subject in question was azotic gas.

I again transmitted the result to C. Fourcroy in the month of Fructidor, year 9; but it is probable that the hurry of business prevented him from returning me an answer.

Having reflected a good deal since that period on the importance of this discovery, I did not cease to attend to it; but my ideas are entirely changed in regard to the consequences which may be deduced from it.

In letters written to Fourcroy, and particularly in the last, I considered my discovery as a particular fact depending on a pathological state; but at present I am obliged to consider it as a general phænomenon belonging to the human species.

1st, Because it is probable that the gas which passed so abundantly through the skin of count de Milly was azotic gas. One will easily be convinced of it by reading his memoir with attention.

2dly, Dr. Ingenhousz, convinced from his own experiments that an aëriform fluid escapes through the skin, believed that it was phlogisticated gas (azotic gas).

3dly, The idea of Ingenhousz is confirmed by my experiments.

4thly, I some time ago met with another individual who perspires abundantly in the bath. The bubbles with which he is constantly covered do not liquefy in the water; they are probably azotic gas: but I confess that I never made any exact experiment on the subject.

5thly, The experiments made by Dr. Priestley, Fontana, and Jurine, which consisted in placing open flasks under the armpits, afford no proof against what I have already advanced; for it is evident that, these flasks being filled with atmospheric air, the latter could not be displaced by the azotic gas, the specific gravity of which is less; while the case ought to have been different with carbonic acid gas, if any be constantly disengaged from the skin, as Jurine infers from his experiments.

Those which Jurine made, by placing his arm in a cover of glass, are not more conclusive; because, having been made with a view to prove that carbonic acid gas escaped, he did not employ the means proper for indicating the presence and quantity of azotic gas, of which he did not suspect the slightest disengagement. But why has not this phænomenon, which I believe to be general, been observed by Dr. Priestley, Fontana, Jurine, &c.? and why is it not remarked in all individuals placed under the same circumstances? It is probable, as I have said at the commencement of this memoir, according to M. Jurine, that the water acting by its gravity on the exhaling vessels of the skin, which in different individuals are endowed with a different energy, opposes in the greater number the escape of any gaseous substance.

I shall here terminate this memoir, to which I might have given more extent had I been inclined to treat of all the questions which naturally arise from the discovery of this phænomenon; but as I had no other object in view than to call the attention of philosophers to this subject, and being satisfied with having laid the first stone of the edifice, I leave to abler architects the glory of finishing the building.

LXIII. *Extract from the third Volume of the Analyses of M. Klaproth.*

This celebrated chemist, so well known for the precision of his analyses, and by the valuable discoveries with which he has enriched the province of chemistry, has published a third volume of his Chemical Researches, dedicated to Vauquelin. Among the number of ingenious analyses it contains, there are some particularly interesting to geologues: such as those of cryolite, sonorous porphyry, and basaltes; in which we are suprised to see soda form, and even in considerable quantity, one of the constituent principles of rocks and compact stones. That of the ombre earth of Cyprus is, we may say, the first with which we are acquainted, and the most correct. The rest, some extracts from which we shall here

Amiano, a village in the state of Parma, near Josnovo and Varese, on the confines of Liguria. Being vexed that the government of the country did not take this object into consideration, I have heard lately with satisfaction that the government of Liguria has converted it to a useful purpose. After being analysed by an able chemist, it has been applied with great advantage to the purpose of lighting the city of Genoa.

It was C. Mojon, professor of chemistry in the university of Genoa, who made the most conclusive experiments on this combustible substance. He read a report on this subject before the National Institute of Liguria on the 4th of July 1802; and it is an extract from this report, procured from the author himself, that I now have the honour of presenting to you.

Professor Mojon, having been on the spot, was enabled to see that the richness of the spring of petroleum at Amiano is so great, that, though people have continually drawn from it since its discovery, it still keeps at the same level. He has found in it the following characters:

This petroleum is exceedingly limpid, of a vinous yellow colour, or rather like that of the topase of Saxony. Its smell is strong, penetrating, and less empyreumatic than that of the common and brownish petroleum. Its specific gravity is to that of water as 83 to 100, and to that of olive oil as 91 to 100.

If a few drops of it be poured on writing paper and exposed to heat, it is entirely volatilised, and leaves no stain. If distilled in a glass retort, in a moderate heat, it passes entirely into the receiver, and leaves no residuum.

If it be mixed with concentrated sulphuric acid, it burns, becomes thick and dark-coloured. It readily dissolves amber, sulphur, and resins; and when mixed with gum copal forms a varnish, which, when spread over a piece of board, and exposed for some days to the sun, dries, and loses its odour. When poured on alcohol, it floats at the surface without dissolving, even when heated and strongly stirred. It floats also over fixed and volatile oils, with which it combines by agitation. Its combustibility is so great, that it inflames when brought near to a burning body: it seems even to attract the flame in consequence of its great volatility. Its own flame is white and lively, like that of oil of turpentine, but it emits much more smoke.

Having thus determined the characters of petroleum, professor Mojon, mindful of his commission, examined whether it might not be employed for lighting the city of Genoa. He made experiments to ascertain the intensity and degree of the light which this liquid produces in comparison of olive oil; and was able to make an exact calculation how far it would be advantageous to employ it.

He put an ounce of petroleum into a glass lamp; added a flat wick of four lines in breadth, placing it in such a manner that the bottom of the flame should be about an inch above the fluid. He then put an ounce of olive oil into another lamp of the same kind, and kindled both lamps at the same time. They gave a flame equally strong, and a light of the same intensity; but with this difference, that the former alone produced a little smoke, and burnt an hour and a half, while the lamp containing the olive oil burnt an hour and thirty-five minutes. Both of them were then dry, without leaving any residuum.

In order to try whether the smoke might not be destroyed or diminished, he burnt the petroleum in a lamp with a current of air furnished with a glass cylinder. He indeed found that the liquid, while it burned completely with a white and very bright flame, emitted no longer any smoke or bad smell. The combustion was so rapid that the flame was agitated by it. Having then introduced into the same lamp a mixture of equal parts of petroleum and olive oil, he obtained a slower combustion, with a more tranquil and uniform flame. He obtained the same result by the union of these two substances in a reverberating lamp with a wick of about an inch in breadth.

From these experiments he concluded that this petroleum might be advantageously employed for lighting the streets, observing the following precautions:

1st, That the flame should be about an inch above the oil.

2d, That the lamp should be covered and closed in such a manner as to prevent the flame from being communicated to the petroleum.

3d, To use a flat wick, to prevent smoke and render the combustion complete.

It was in consequence of this report of professor Mojon that the Ligurian government ordered petroleum to be employed for lighting the streets of Genoa. It is used at present without any mixture: the reverberators are constructed with the improvements above indicated; but care has been taken to add a kind of conical tube or chimney of tin plate, to convey off the smoke which may be disengaged. By these means the same quantity of light is obtained as with

olive

olive oil, and at a fourth of the expense, as the petroleum costs only two Genoese sous per pound, (which is less than a penny English.)

LXI. *Report on the Discovery of a Deposit of bituminous Wood. Read in the Ligurian Institute July* 1802 *by* C. MOJON, *public Professor of Chemistry* *.

At the period when nature opened in the state of Parma, on the north side of the Appenines, an extraordinary spring of petroleum †, it presented on the opposite side, in Liguria, at the distance of about fifteen leagues, a considerable depôt of bituminous wood. The discovery of this fossil is even a little more recent, and is equally interesting to the naturalist and to the state. For a knowledge of it we are indebted to the useful labours of professor Mojon, as will be seen by the following extract from the report which he made to the National Institute of Liguria:

"Castel Nuovo is a country of Lunigiana on the confines of the Italian republic. It is in the plain of that country, half a league from the mouth of the Magra, that the mine of the substance in question has been discovered. It is situated in a soil formed of argillaceous and calcareous strata, more or less thick, and inclined in different directions throughout their whole extent. The nature of the fossil, as well as the constitution of the soil, evidently shows that these strata were formed only by great floods, which carried with them and buried whole forests ‡. The extent of this deposit, however, cannot be determined, because the pits proposed to be sunk at different distances for that purpose have not yet been constructed. Hitherto one only has been made, about 40 feet in depth, the bottom of which is inundated by a spring that issues from an excavation attempted in a lateral direction; and it is to be observed, that in this pit no disengagement has been remarked of carbonic acid gas, nor of those gases generally developed from such excavations.

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* From the *Annales de Chimie*, No. 135.

† See the preceding article.

‡ Is there not reason to believe that the *lucus sacer* of the antient *Luna* must form part of this deposit? The ruins of the town, in digging among which monuments worthy the attention of the antiquary have lately been discovered, are found very near, and at the distance of the third of a league from the mouth of the Magra.

"This bituminous wood, which in some places appears almost uncovered, at the surface of the ground still retains its primitive figure. In the course of the search which has been made by digging, trunks of different thickness up to two feet diameter have been dug up; and among these there are some compressed, which exhibit in their transverse section an elliptical form.

"The colour of this fossil is sometimes a perfect black, sometimes a grayish black, and sometimes a wood brown. The blackest is sometimes pretty shining, and has even the splendour of glass; the grayish black, and brown, are dull, but shine very much on being polished.

"Its texture is entirely that of wood, as it has not been altered by the bitumen with which it is impregnated; so that in some pieces the tree to which they belonged may be determined: and in particular those which have the transverse fracture conchoid and shining, and which are perfectly black, exhibit all the characters of fir: others have a texture similar to that of oak.

"It is not so brittle as coal, and, when sawn in a direction perpendicular to the axis of the trunks, exhibits a solid, compact, and very smooth surface. The concentric strata of the fibres of which the trunk is composed may be easily separated by introducing the point of any instrument whatever.

"It readily kindles, and without the aid of any other combustible: it gives in burning a lively and brilliant flame. The heat it produces is more intense and durable than that of any other vegetable coal; and, when once kindled, never becomes extinct till entirely consumed, leaving very little ashes. The combustion may be interrupted and renewed at pleasure.

"Lighter than coal, its specific gravity is to that of distilled water as 1235 to 1000: the fragments which exhibit the characters of oak are still lighter.

"Sulphuret of iron is sometimes found in this fossil disseminated in lumps and small grains, which when long exposed to the air are decomposed, and cause the fragments to which they belong to fall in pieces.

"It has besides the properties common to coal: but if naturalists have long entertained doubts respecting the origin of coals, in consequence of the variety of their texture, the irregularity of their form, their fragility, their strata analogous to those of schistous stones, &c., the fossil of Castel Nuovo presents no difficulty to their researches. The uniformity of the tissue; the constant direction of its fibres;

the bark of the different trunks; its knots; fracture; the colour of some kinds; the facility with which it inflames; the alkali contained in its ashes;—all these characters, very distinct, render it perfectly similar to charred wood.

" It must also be added, that it possesses this singular property, that it is susceptible of being worked in the lath with the greatest facility, and of acquiring a polish and splendour which render it superior to ebony.

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After this short account of the situation and nature of this fossil, I shall describe some of the experiments which the author made, in order to determine what advantages may be derived from the employment of this substance.

He burnt 12 pounds of this bituminous wood in a furnace, adapting to it a thermometer at the distance of eight inches, the temperature at the time being 10 degrees of Reaumur. Sixteen minutes after, the whole wood was completely kindled, and gave a clear brilliant flame, with less smoke than coal and a slight odour of bitumen, which not being sulphureous did not incommode those persons even who were nearest to it. The thermometer had risen to 42 degrees. Twenty-four pounds of water, which were in a copper vessel on the furnace, then began to boil. The thermometer stood at the same degree for 12 minutes, and the water continued to boil for 20. When the ebullition had ceased the thermometer fell to 26 degrees, and the water was reduced to three pounds. The fossil was entirely consumed in the course of an hour and a quarter, and left 225 grains of yellowish impalpable ashes in the form of flakes.

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By ebullition in distilled water it suffers to be precipitated calcareous earth and argil.

By pouring nitric acid over this fossil, it is decomposed, disengaging nitrous gas.

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If I intended to consider the influence of the air under every point of view, I should begin with respiration; a function of so much importance, that without it life could not exist, while it alone can for some time maintain and preserve it.

But the modern chemists, after an exact analysis of atmospheric air, have given a theory of respiration so ingenious, so complete, and so well founded on correct experiments, that all the attempts hitherto made to overturn it have only tended to establish it more and more.

I could therefore only repeat here what has been said before me by Lavoisier, Seguin, Crawford, Fourcroy, Chaptal, &c.; and this matter has been so well elucidated by the labours of these celebrated chemists, that it is in some measure exhausted; and their theory in this respect has been so widely diffused, that it is now adopted by all men of science. I shall therefore abstain from repeating what would be here tiresome and superfluous.

But if what takes place in the lungs is exactly known, the same light has not yet been thrown on the functions of the skin. Independently of cutaneous perspiration, observed with so much care and correctness by Sanctorius and several others, does there escape through the skin one or more aëriform fluids? and, in this case, of what nature are they? Such are the two questions which I purpose to examine in this memoir.

The antients had no idea of this aëriform cutaneous perspiration, and they make no mention of it in their works.

Count de Milly first announced, in 1777, the discovery of an elastic fluid * which escapes through the skin. He asserts that a person in a warm bath may collect half a pint of it in the course of three hours; and it results from his analysis, which indeed is very imperfect and incorrect, that it is fixed air (carbonic acid gas).

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Fourcroy on this subject expresses himself as follows: "It is not true that elastic fluids, and particularly carbonic acid gas, escape through the skin, as some of the moderns have asserted *.

Such, a few years ago, was the state of the question which forms the subject of this article. Incorrect experiments, the contradictory results of which were contested either in the whole or in part, left philosophers in uncertainty, and seemed to call for new researches, in order to fix the opinion of philosophers on this point.

I often reflected on it, and had formed a design of employing myself with it, when in the spring of the year 8, being near one of my patients who was in the bath, I perceived that he was entirely covered with small bubbles of air. The hairs on his body were surrounded by bubbles decreasing from the base to the summit, so that a great number of them exhibited the appearance of pyramids more or less elevated. I made all these bubbles disappear; but in the course of half an hour they were succeeded by an equal number. In consequence of observing this phænomenon, I caused my patient to continue his bathing; and having collected several bell-fulls of this gas, I examined it carefully several times, and found that it was azotic gas, perfectly pure, without any mixture of carbonic acid.

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* Systême des Connoissances Chimiques, tom. ix, p. 203.

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In letters written to Fourcroy, and particularly in the last, I considered my discovery as a particular fact depending on a pathological state; but at present I am obliged to consider it as a general phænomenon belonging to the human species.

1st, Because it is probable that the gas which passed so abundantly through the skin of count de Milly was azotic gas. One will easily be convinced of it by reading his memoir with attention.

2dly, Dr. Ingenhousz, convinced from his own experiments that an aëriform fluid escapes through the skin, believed that it was phlogisticated gas (azotic gas).

3dly, The idea of Ingenhousz is confirmed by my experiments.

4thly, I some time ago met with another individual who perspires abundantly in the bath. The bubbles with which he is constantly covered do not liquefy in the water; they are probably azotic gas: but I confess that I never made any exact experiment on the subject.

5thly, The experiments made by Dr. Priestley, Fontana, and Jurine, which consisted in placing open flasks under the armpits, afford no proof against what I have already advanced; for it is evident that, these flasks being filled with atmospheric air, the latter could not be displaced by the azotic gas, the specific gravity of which is less; while the case ought to have been different with carbonic acid gas, if any be constantly disengaged from the skin, as Jurine infers from his experiments.

Those which Jurine made, by placing his arm in a cover of glass, are not more conclusive; because, having been made with a view to prove that carbonic acid gas escaped, he did not employ the means proper for indicating the presence and quantity of azotic gas, of which he did not suspect the slightest disengagement. But why has not this phænomenon, which I believe to be general, been observed by Dr. Priestley, Fontana, Jurine, &c.? and why is it not remarked in all individuals placed under the same circumstances? It is probable, as I have said at the commencement of this memoir, according to M. Jurine, that the water acting by its gravity on the exhaling vessels of the skin, which in different individuals are endowed with a different energy, opposes in the greater number the escape of any gaseous substance.

I shall here terminate this memoir, to which I might have given more extent had I been inclined to treat of all the questions which naturally arise from the discovery of this phænomenon; but as I had no other object in view than to call the attention of philosophers to this subject, and being satisfied with having laid the first stone of the edifice, I leave to abler architects the glory of finishing the building.

LXIII. *Extract from the third Volume of the Analyses of M. Klaproth.*

This celebrated chemist, so well known for the precision of his analyses, and by the valuable discoveries with which he has enriched the province of chemistry, has published a third volume of his Chemical Researches, dedicated to Vauquelin. Among the number of ingenious analyses it contains, there are some particularly interesting to geologues: such as those of cryolite, sonorous porphyry, and basaltes; in which we are suprised to see soda form, and even in considerable quantity, one of the constituent principles of rocks and compact stones. That of the embre earth of Cyprus is, we may say, the first with which we are acquainted, and the most correct. The rest, some extracts from which we shall here present, confirm or rectify those before made by the most celebrated chemists.

Analysis of sonorous Porphyry.

Sonorous porphyry unites in it all the mineralogical characters of the other porphyries. Its substance, equally hard, is composed of silex and alumine, interspersed with some small leaves of feld-spath, and small grains, not very numerous, of amphibolite. But it differs from it by its foliaceous fracture. Besides, when broken, it has a sound almost metallic. The name of sonorous porphyry appears, then, to be perfectly suited to it.

It is found in the mountains of Bohemia, in those of Upper Lusatia, and in the country of Fulda. It never forms these continued chains, but only insulated mountains, and commonly situated in the neighbourhood of those of basaltes. It forms mountains of the hardest kind, and which present the greatest resistance to degradation. It effloresces only at the surface; and the argillaceous crust which covers it renders these stones very slippery, and of difficult ascent.

The colour of the sonorous porphyry is gray, sometimes inclining a little to green. It is always compact, rough, split in its fracture. It breaks into thick leaves, very thin fragments of which have pellucid edges. Its substance, the grain of which is fine, is very hard and smooth. When reduced to powder it is of a gray colour. Its specific gravity is 2·575.

Sonorous porphyry reduced into small fragments, and freed as much as possible from the leaves of feld-spar and grains of amphibolite disseminated throughout its mass, lost 3 per cent. of its weight by calcination. Its gray colour became lighter, but it experienced no other alteration.

When exposed to a porcelain furnace it fuses into a compact glass.

A hundred grains of sonorous porphyry reduced to powder, and treated in succession with potash, muriatic acid, barytes, and succinate of ammonia, gave

Silex - - - - - -	57·25
Alumine - - - - - -	23·50
Lime - - - - - -	2·75
Oxide of iron - - - - -	3·25
Oxide of manganese - -	·25
Soda - - - - - - -	8·10
Water - - - - - -	3·00
	98·10

The

was by no means conducive to their accuracy. The surface of a slender stem is proportionally greater when compared with its solid content than that of a small one: the capillary attraction, therefore, and the weight of the liquor which adheres to it, both operate more powerfully. If we take any of those instruments whose stem is a mere wire, we shall find them, from these causes, so sluggish in their motion that they will generally stand at any point of the stem at which they are placed, within a quarter of an inch of a certain part of it, whereas those with a thicker stem regain the same point instantaneously. The thicker the stem, therefore, the better, so as it be only within such limits as to render the difference in the points at which the instrument will stand, in two liquors which differ in a very minute degree with respect to their specific gravities, sufficiently perceptible.

Let us calculate for a moment how many weights we must have to a spirit hydrometer. The stem may very conveniently be 4 inches long, and in this length we could easily graduate 40 or 50 divisions, so that a difference of one-fourth of each shall be very visible. But we want to express a difference of nearly 200 in specific gravity (or from 800 to 1000) by this instrument within about one-fourth of an unit. We shall therefore do it extremely well by the virtual lengthening of the stem to four or five times its real length by means of three or four weights. It is true, the divisions, unless they be arbitrary ones with a scale of reference, are not equal; if they be units of specific gravity, they must be graduated from several harmonic scales: but this will not affect the truth of our general deduction; and three or four weights, therefore, will be fully sufficient for the nicest spirit hydrometer, whatever scale of graduation we may use on its stem.

This kind of instrument would be far preferable, even if superior accuracy were alone the object, if simplicity were entirely out of the question, and 40 weights as easy and as little liable to error in their application as 4: but when we reflect that simplicity and facility of use are, perhaps, even of more consequence than accuracy in the result, the difference in the advantages of these modes of construction becomes enormous.

It has been a favourite opinion with some gentlemen, that glass hydrometers would be preferable to metallic ones, because they would not be liable to have their bulk altered by any contusion without being broken to pieces; whereas the latter might be so delved by rough usage as to give fallacious

Silex - - - -	48
Alumine - - - -	16
Oxide of iron - - -	16
Lime - - - -	9
Soda - - - - -	4
Muriatic acid - - -	1
Water and volatile parts -	5
	99

Dr. Kennedy makes no mention of magnesia, but he indicates muriatic acid as one of the principles of basaltes. M. Klaproth analysed it again, in order to try to discover it. He decomposed basaltes by nitric acid, and poured into the solution a solution of nitrate of silver. He observed a slight cloud, and collected a deposit which weighed 3-10ths of a grain. On this muriate of silver he found nitric acid, which reduced it to 1-20th of a grain; a quantity so small, that it does not announce the hundredth part of a grain of muriatic acid in the basaltes of Hasenberg.

Analysis of the Gold Ore of Transylvania.

It was by the analysis of these gold ores, so rich, that M. Klaproth discovered tellurium; and as this new metal constitutes the greater part of them, they have since been called the auriferous ores of Tellurium. Under this name M. Klaproth comprehends,

1st, That of paradoxal gold, or the problematic metal taken from Mariahilf, near Zalathna, in the mountains of Faczebayer in Transylvania.

A thousand parts of this ore of tellurium are composed of

Tellurium - -	925.50
Iron - - -	72·00
Gold - - -	2·50
	1000

2d, The ore of graphic gold of Offenbanya, which is very rich. It contains

Tellurium - - -	60
Gold - - -	30
Silver - - -	10
	100

3d, That of Nagyac, which is more compounded. It contains

Tellurium	44·75
Gold	26·75
Lead	19·50
Silver	8·50
Sulphur	0·50
	100

4th, The foliaceous ore of Nagyac. It contains

Lead	54·0
Tellurium	32·2
Gold	9·0
Silver	0·5
Copper	1·3
Sulphur	3·0
	100

An Account of some new Properties of Tellurium.

Mercury seems only to be weakly attracted by this metal. One part of tellurium pulverised and heated in a small retort with six parts of mercury, seemed to be united into an amalgam with a crystallized surface. But M. Klaproth found that the mercury had scarcely dissolved any of the tellurium, and that the former had only covered the surface of it under the form of small scales.

The solution of tellurium in muriatic acid is clear. If water be added to the saturated solution, it first produces a precipitate, which a greater quantity of water afterwards redissolves. But if alcohol be poured in instead of water, and if the precipitate be edulcorated with alcohol, scarcely any tellurium will remain in the solution. Precipitates obtained by alcohol or by water are not pure oxides of tellurium. They always retain a little muriatic acid.

Solutions of tellurium where the acid predominates a little, are neither rendered turbid nor produce any precipitate by those of prussiate of potash. This is a remarkable property of this metal, which it however participates with gold, platina and antimony. Tincture of gall-nuts poured into a solution of it gives a flaky precipitate of an Isabella colour. Phosphorus immersed in a muriatic solution of tellurium becomes covered in the course of time with metallic leaves.

Analysis of the Tungsten Ore of Schlakenwald.

Scheele found in the calcareous *scheelin* of the ore of Bitzberg in Sweden,

Oxide of tungsten	65
Lime	31
Silex	4
	100

Messrs. d'Ellyar found tungsten also in analysing the wolfram ore of Linnwald. At the same time they gave the following proportions for the white tungsten of Schlakenwald:

Yellow oxide of tungsten	68
Lime	30
	98

Klaproth, in his new analysis of the grayish white scheelin of Schlakenwald, pellucid and crystallized in octaëdra, found them to be very different. Its specific gravity is 6·015.

A hundred parts of these crystals of scheelin gave:

Yellow oxide of tungsten	77·75
Lime	17·60
Silex	3·00
	98·35

He extracted from 100 parts of the calcareous scheelin of Pengilly in Cornwall:

Yellow oxide of tungsten	75·25
Lime	18·70
Silex	1·50
Oxide of iron	1·25
Oxide of manganese	0·75
	97·45

New Analysis of Gadolinite found at Itterby in Sweden;

Yttria	59·75
Silex	21·25
Black oxide of iron	17·50
Alumine	0·50
Water	0·50
	99·50

Analysis of the Black Ore of Szekeremb in Transylvania.

Besides the red ore of manganese found at Szekeremb, which varies both in regard to its form and colour, and serves as matrix to the auriferous ores of tellurium worked there, there is one which exhibits the following characters:

It has a mean colour between the brown and the black of iron (ethiops). It is almost always compact, sometimes veined with red ore of manganese, or it forms alternate strata with that ore.

It has a semi-metallic state.

Its fracture is unequal, of a small grain, and foliated in every direction.

It splits into fragments irregularly angular, the edges of which are not very obtuse.

It is opake.

When scratched the trace appears of the dark yellow colour of brass, inclining much to green, and almost entirely dull. It is of a mean hardness, and pretty smooth to the touch. Its specific gravity is 3·950. This ore contains

Oxidulous manganese soluble in nitric acid	82
Carbonic acid - - - - - -	5
Sulphur - - - - - -	11
	98

New Analysis of Cryolite by M. Klaproth.

Cryolite is one of the most interesting of the new discoveries in mineralogy. It was found in Greenland, and a small quantity of it was some time ago received at Copenhagen. Professor Abilgard, who was of great service to the mineralogy of chemistry, undertook the analysis of it. He found it to be composed of fluoric acid and alumine; a very unexpected combination, no instance of which had before been found.

External Characters of Cryolite.

External form as yet undetermined; oolour a bright whitish gray; fracture, when longitudinal, brilliant; when parallel to the axis, less brilliant. Both have a vitreous splendour: its fractures are foliated, and intersect each other at right angles; in the other directions they are unequal. Cryolite breaks into cubic fragments: it is semi-transparent, tender, and pretty soft to the touch; exceedingly fragile; and, according to Andrada, of the mean specific gravity of 2·9695, or according to Haüy 2·949.

Exposed to the blow-pipe on coals, it first forms itself into a white opake round ball; but it afterwards loses its fusibility, and resembles earth strongly calcined. The name of cryolite then is not suited to this fossil because it fuses by the blow-pipe like glass, but perhaps because it has some external resemblance to it. Klaproth repeated the analysis of cryolite in the following manner, in order to determine with the greater precision the proportions of the constituent principles of this fossil: he heated to dryness in a platina crucible 100 grains of cryolite in powder with 300 grains of sulphuric acid, to effect a complete separation of the fluoric acid. The mixture at first boiled up with a disengagement of fluoric acid vapours. The residuum being dissolved in water was precipitated by evaporation under the form of a soft saline matter, which was easily redissolved by the addition of a little water.

2d. M. Klaproth precipitated from the solution, by means of caustic ammonia, the alumine, which when washed and dried weighed 46 grains, and only 24 when afterwards calcined. This earth, when dissolved by heat with diluted sulphuric acid, was entirely precipitated by the addition of potash in regular crystals of alum.

M. Klaproth neutralised, by means of acetous acid, the solution from which the alumine had been separated by ammonia: he poured into it acetite of barytes; and the liquor being filtered and evaporated, he brought the residuum to a red heat in a platina crucible. He then caused it to redissolve, and freed it by the filter from a small quantity of carbonaceous matter. By evaporation to dryness he obtained 62½ grains of very dry carbonate of soda, which contained 36 grains of pure soda. Saturated with acetous acid, it wholly crystallized into acetite of soda.

Deducting from the 100 grains subjected to analysis the weight of the quantities of alumine and soda found in them, we shall have that of the fluoric acid and of the water of crystallization.

Cryolite then is composed of alumine, soda, and fluoric acid in the following proportions:

Soda - - - - -	36
Alumine - - - -	24
Fluoric acid and water of crystallization	48
	100

Analysis of Umber Earth by M. Klaproth.

The old mineralogists gave the name of umber earth to a brown earthy powder. It is composed of a brown carbonaceous earth, which may easily be known, as it is entirely converted into ashes by exposure to fire. On this account Cronstedt gave it the name of *mumia vegetalis*, and Wallerius calls it *humus umbra*. This substance has nothing in common with the real umber earth but the colour.

On the other hand, umber earth is incombustible, and, according to its constituent principles, ought to be classed among the ores of iron. It may be considered as a variety of bog iron ore. The only analysis made of it before that of Klaproth was by M. Santi, who analysed that of Castel del Piaro. The proportions he indicates are as follow:

Oxide of iron - - -	53
Alumine - - -	24
Silex - - - -	19
Magnesia - - -	4
	100

But as M. Santi makes no mention of manganese, which however is one of the essential principles of umber earth, this omission must excite some doubts in regard to the correctness of the proportions which he indicates. The umber earth employed in the following analysis was procured from the island of Cyprus: it resembles externally that known in commerce under the name of fine Turkish umber, and is equally good for painting; it would therefore be superfluous to describe its exterior characters.

1st. Kept at a red heat for half an hour in a crucible, it lost 14 per cent. of its weight. But it experienced no other alteration except that its colour had become a darker brown.

2d. Exposed to a more violent heat, it enters into fusion. M. Klaproth put a fragment weighing 200 grains into a charcoal crucible, which he placed in a porcelain furnace. He took out the crucible quite safe, and containing a button well fused under a thick vitreous scoria of a hyacinth colour, and covered with points at the exterior surface. This metallic button was a little tenacious in its fracture under the hammer, and presented a granulated tissue like that of steel. It weighed 80 grains, and the vitreous scoria 47. The loss consequently amounted to 36½ per cent.

A hundred grains of umber earth well pulverized, mixed with 200 grains of concentrated sulphuric acid evaporated to dryness, were brought to a red heat in a crucible by means of a very strong fire. The red mass appeared of a brick colour: it was porous, and easy to be pulverized.

When washed with water and filtered, the liquor became brown, and contained sulphate of magnesia.

A hundred grains of umber earth reduced to fine powder, and digested with boiling muriatic acid, left a residuum which weighed 19 grains. The solution when filtered was condensed by evaporation, and mixed with a concentrated solution of tartrite of potash; but the mixture remained clear. M. Klaproth then endeavoured to obtain a precipitate by the solution of caustic soda, but did not succeed: he even did not observe any cloud, and the saturated solution assumed a reddish brown colour. He poured into it muriatic acid, and the colour of the solution was heightened: he then tried, but without success, to precipitate it by ammonia; for the mixture reappeared with the brownish red colour before indicated, and without any cloud.

M. Klaproth's object was to separate the manganese from the iron by means of these reagents; but this trial only confirmed a peculiar property of the tartareous acid, which is, that by its presence it renders iron soluble in alkaline solutions.

A hundred grains of pulverized umber earth were mixed with a solution of 200 grains of caustic soda, then desiccated to dryness, and exposed in a crucible to a moderate fire for half an hour. The mass dissolved in the water gave it a beautiful dark emerald green colour, which became of an amethyst red by the addition of muriatic acid. This acid added to saturation dissolved completely, and the liquor had acquired a red colour inclining to yellow. There were disengaged at the same time vapours of oxygenated muriatic acid. The solution, when evaporated almost to dryness, assumed at last the form of a jelly: diluted with new water, it left on the filter silex, which when well dried weighed 13 grains.

2d. M. Klaproth poured into the liquor a solution of caustic soda, which he added in excess, and the brown oxide of iron precipitated by it was washed. The remaining solution was precipitated by sulphuric acid, and neutralized by carbonate of potash: alumine was deposited, which when dried weighed 5 grains.

3d. The oxide of iron after being dried was black and brilliant, and resembled a piece of coal: it weighed 68 grains. Redissolved in muriatic acid added only to the point of saturation, it was precipitated from it by the succinate of ammonia. The rest of the solution become colourless, mixed with the abundant water arising from the washing of the flaky deposit of iron, was again precipitated by that of caustic soda, with which Klaproth boiled it. The blackish brown flaky deposit, which was blacker after being washed and dried, weighed 20 grains: it was formed of oxide of manganese. This quantity deducted from the 68 grains of iron previously obtained, reduced the real proportion of the oxide of iron to 48 grains.

The proportions of the constituent principles of the umber earth of Cyprus then are:

Oxide of iron - -	48
Oxide of manganese -	20
Silex - - -	13
Alumine - - -	5
Water - - - -	14
	100

Analysis of Spar in Tablets.

Spar in tablets* is generally found in a mixture of brown crystallized granite and blue calcareous spar, of which it forms the third substance. It is of a milky white colour. M. Karsten has described hexaëdral crystals of it. M. Stutz calls it spar in tablets, because it exhibits in its fracture long leaves a little brilliant. It is found, according to M. Stutz, at Dognaska in the canal of Temeswar, and according to M. Estner, at Oravuza.

The analysis of it proves that it is not a kind of tremelite. Spar in tablets contains

Silex - - - -	50
Lime - - - -	43
Water - - - -	5
	100 †

The lime in it is not carbonated, for nitric acid poured over this fossil produces neither bubbles nor loss in dissolving.

Analysis of Miemite.

Miemite takes its name from Miemo in Tuscany, where it was found by Dr. Thompson in 1791, and made known by him under the appellation of magnesian spar: it is of an asparagus green colour, crystallized in flattened triangular pyramids: it is of mean hardness and gravity, rough to the touch, &c. A hundred parts of miemite contain

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* The analysis of spar in tablets is found in Klaproth's work next to that of pharmacolite and scorza sand, as well as that of miemite.

† There appears here to be some mistake.

Carbonate of lime	53·00
Carbonate of magnesia	42·50
Carbonate of iron mixed with a little magnesia	3·00
	98·50

The proportion of the principles of miemite, which approaches very near to that of the magnesian spar of the Tyrol, ought to make them be considered as two species of the same kind.

Analysis of prismatic Magnesian Spar.

This spar, discovered at Gluuk Bronn, in the country of Gotha, in veins of cobalt, is very rare: its crystals are tetraedral, almost rectangular, of an asparagus green colour, darker in several varieties than that of the chryso-beryl, and rarely so clear as that of the apatite of Cape Gates. They exhibit a vitreous splendour in their fracture; they break in irregular angular fragments; they are very pellucid, and of a mean hardness; they leave a snow-white trace; their specific gravity is 2·885.

Some crystals of magnesian spar, calcined in a platina crucible for half an hour, came from it entire; but exceedingly friable, and altogether opake. Zones of different colours, the interior of which was Isabella yellow, were observed in them; the second was reddish white, and the nucleus pink brown: they retained also some splendour, and had lost 45 per cent. of carbonic acid, though the whole acid was not then entirely volatilized. A hundred parts of magnesian spar contain

Lime	33·00
Magnesia	14·50
Oxide of iron	2·50
Carbonic acid	47·25
Water and loss	2·75
	100

Analysis of Egyptian Natrum.

We are indebted to M. Berthollet for the most correct description of the lakes of natrum, and of the deserts of Makaria in Lower Egypt. The natrum found there is not all of equal purity: from time immemorial it has been an important article of commerce.

The natrum of Egypt, when well freed from the earthy parts mixed with it, is composed of

Dry carbonate of soda - -	163
Dry sulphate of soda - -	104
Dry muriate of soda - -	75
Water - - - -	158
	500

Analysis of the radiated Natrum of Trona.

The natrum of Egypt is often found in very hard crystalline masses: it is indebted for its hardness to the muriate of soda, which is generally combined with it: so great is its hardness, that it has been even employed in constructing the walls of Fort Quasir.

That of Debrezin in Hungary, and of Monte Nuovo near Naples, having, on the other hand, lost its water of crystallization, is found only under the form of dust.

Radiated natrum is not subject to effloresce, though formed in the hottest climates of Africa: this phænomenon will be explained by the analysis hereafter given. The following is the description of this remarkable kind of natrum by M. Bagge, the Swedish consul at Tripoli:

"It is found at a place called Trona, in the province of Sukena, two days journey from Fessand, at the foot of a rocky mountain, where it forms a crust of an inch in thickness at most, and sometimes of only a few lines: it is always crystallized: its fracture exhibits long crystals, agglutinated, parallel, and sometimes radiated: it resembles uncalcined plaster. Besides a large quantity of Trona, a name it has received from the place where it is found, which is carried to Negroland and Egypt, a thousand quintals of it are carried every year to Tripoli. It is not mixed with muriate of soda; and the salt mines are on the sea coast, twenty-eight miles from Trona, which is situated in the interior of the country."

The natrum employed by M. Klaproth in his analysis forms a crystalline crust of from four to six lines in thickness, composed of vertical laminæ of a foliated and radiated texture.

The natrum of Tripoli, according to analysis, contains

Water of crystallization -	22·50
Carbonic acid - -	38·00
Pure soda - - -	37·00
Sulphate of soda - -	2·00
	99·50

If the proportions of this natural carbonate of soda be compared with that of the recently prepared crystals of artificial carbonate of soda, which contain

Soda - - - -	22
Carbonic acid - - -	16
Water of crystallization -	62
	100

a striking difference will be found in the proportion of the carbonic acid; for that of the carbonic acid is at most 73 parts in a hundred in the common carbonate, while it is nearly 103 in the natrum of Tripoli, which makes 30 more.

It is this more complete saturation of soda by the carbonic acid which gives to trona the property of not efflorescing. There are certainly some local circumstances which tend to favour it. But these must be determined by future naturalists who may visit the places where it is found.

Common carbonate of soda is not at its maximum of saturation; consequently it is susceptible of combining with a larger quantity of carbonic acid. This may be effected in the same manner as the complete saturation of the carbonate of potash. That of artificial soda completely saturated approaches, therefore, to trona, both externally in regard to the foliated form of its crystals, and the property which it acquires at the same time of presenting greater resistance to efflorescence.

[To be continued.]

LXIV. *On the Modifications of Clouds, and on the Principles of their Production, Suspension, and Destruction; being the Substance of an Essay read before the Askesian Society in the Session* 1802-3. *By* Luke Howard, *Esq.*

[Continued from p. 107.]

Explanation of the Plates.

Plate VI. *a, a,* represents different appearances of the cirrus: *b,* a regular cumulus: *c,* a stratus occupying a valley at sun-set, in the midst of which is supposed a spot of higher ground, with trees, &c.

Plate VII. *a,a,* exhibits the character of the cirro-cumulus, as also its appearance in the distance. *b, b,* a light and a dark cirro-stratus; the former taken just before the commencement of wet weather, the latter in the twilight of the evening, when dew was falling: the smaller ones show its appearance in the distance. *c, c,* the mixed and the distinct cumulo-stratus; the latter in its most regular state, as sometimes seen at the approach of thunder storms and after showers.

Plate VIII. A distant shower coming from behind an elevated point of land, in which are represented the superior sheet stretching in different parts to windward, and cumuli advancing towards and entering the mass, the whole of which constitutes the nimbus.

As the establishing distinctive characters for clouds has been heretofore deemed a desirable object, and it is consequently probable that the author's modifications will begin to be noted in meteorological registers as they occur, a practice which may be productive of considerable advantage to science, the following system of abbreviations may, perhaps, be found of some use in this respect. They will save room and the labour of writing, and types may be easily formed for printing them. These are advantages not to be despised, when observations are to be noted once or oftener in the day. It is only necessary that they be inserted in a column headed *Clouds*; that the modifications which appear together be placed side by side, and those which succeed to each other in the usual succession of the column, but separated by a line or space from the preceding and succeeding day's notations.

╲	Cirrus.
∩	Cumulus.
—	Stratus.
╲∩	Cirro-cumulus.
╲—	Cirro-stratus.
∩—	Cumulo-stratus.
╲∩—	Cirro-cumulo-stratus, or Nimbus.

In tracing the various appearances of clouds, we have only adverted to their connection with the different states of the atmosphere (on which, indeed, their diversity in a great measure depends), having purposely avoided mixing difficult and doubtful explanation with a simple descriptive arrangement.

Of Evaporation.

On the remote and universal origin of clouds there can be but one opinion—that the water of which they consist has been carried into the atmosphere by evaporation. It is on the nature of this process, the state in which the vapour subsists for a time, and the means by which the water becomes again visible, that the greatest diversity of opinion has prevailed.

The chemical philosopher, seduced by analogy, and accustomed more to the action of liquids on solids, naturally regards evaporation as a solution of water in the atmosphere, and the appearance of cloud as the first indication of its precipitation; which becoming afterwards (under favourable circumstances) more abundant, produces rain. The theory of Dr. Hutton goes a step further, assumes a certain rate of solution differing from that of the advance of temperature by which it is effected, and deduces a general explanation of clouds and rain from the precipitation which, according to his rule, should result from every mixture of different portions of saturated air. The fundamental principle of this theory has been disproved in an essay heretofore presented to the society*, and which was written under the opinion, at present generally adopted by chemists, that evaporation depends on a solvent power in the atmosphere, and follows the general rules of chemical solution.

The author has since espoused a theory of evaporation which altogether excludes the above-mentioned opinion (and consequently Dr. Hutton's also), and considers himself in a considerable degree indebted to it for the origin of the explanation he is about to offer. It will be proper, therefore, to state the fundamental propositions of this theory, with such other parts as appear immediately necessary, referring for mathematical demonstrations and detail of experiments to the work itself, which is entitled "Experimental Essays on the Constitution of mixed Gases; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Elastic Fluids by Heat. By John Dalton."—See Memoirs of the Literary and Philosophical Society of Manchester, vol. v. part 2.—The propositions are as follow:

" 1. When two elastic fluids, denoted by *A* and *B*, are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of *A* do not repel those of *B*, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

" 2. The force of steam from all liquids is the same at equal distances above or below the several temperatures at which they boil in the open air; and that force is the same under any pressure of another elastic fluid as it is *in vacuo.* Thus the force of aqueous vapour of 212° is equal to 30 inches of mercury; at 30° below, or 182°, it is of half that force; and at 40° above, or 252°, it is of double the force: so likewise the vapour from sulphuric ether which boils at 102°, then supporting 30 inches of mercury, at 30° below that temperature it has half the force, and at 40° above it, double the force: and so in other liquids. Moreover, the force of aqueous vapour of 60° is nearly equal to half an inch of mercury when admitted into a Torricellian vacuum; and water of the same temperature, confined with perfectly dry air, increases the elasticity to just the same amount.

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* See Phil. Mag. vol. xiv. p. 55.

"3. The quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same."

The following is part of the Essay on Evaporation:

"When a liquid is exposed to the air, it becomes gradually dissipated in it; the process by which this effect is produced we call *evaporation.*

"Many philosophers concur in the theory of chemical solution: atmospheric air, it is said, has an affinity for water; it is a menstruum in which water is soluble to a certain degree. It is allowed notwithstanding by all, that each liquid is convertible into an elastic vapour *in vacuo,* which can subsist independently in any temperature; but as the utmost forces of these vapours are inferior to the pressure of the atmosphere in ordinary temperatures, they are supposed to be incapable of existing in it in the same way as they do in a Torricellian vacuum: hence the notion of affinity is induced. According to this theory of evaporation, atmospheric air (and every other species of air for aught that appears) dissolves water, alcohol, ether, acids, and even metals. Water below 212° is chemically combined with the gases; above 212° it assumes a new form, and becomes a distinct elastic fluid, called *steam:* whether water first chemically combined with air, and then heated above 212°, is detached from the air or remains with it, the advocates of the theory have not determined. This theory has always been considered as complex, and attended with difficulties; so much that M. Pictet and others have rejected it, and adopted that which admits of distinct elastic vapours in the atmosphere at all temperatures, uncombined with either of the principal constituent gases, as being much more simple and easy of explication than the other; though they do not remove the grand objection to it, arising from atmospheric pressure."

"On

" *On the Evaporation of Water below* 212°.

" I have frequently tried the evaporation at all the temperatures below 212: it would be tedious to enter into detail of all the experiments, but shall give the results at some remarkable points. In all the high temperatures I used the vessel above mentioned*, keeping a thermometer in it, by which I could secure a constant heat, or at least keep it oscillating within narrow limits.

" The evaporation from water of 180° was from 18 to 22 grains per minute, according to circumstances; or about one-half of that at 212°.

" At 164° it was about one-third of the quantity at the boiling temperature, or from 10 to 16 grains per minute.

" At 152° it was only one-fourth of that at boiling, or from 8 to 12 grains, according to circumstances.

" The temperature of 144° affords one-fifth of the effect at boiling; 138° gave one-sixth, &c.

" Having previously to these experiments determined the force of aqueous vapour at all the temperatures under 212°, I was naturally led to examine whether the quantity of water evaporated in a given time bore any proportion to the force of vapour of the same temperature, and was agreeably surprised to find that they exactly corresponded in every part of the thermometric scale: thus the forces of vapour at 212°, 180°, 164°, 152°, 144°, and 138°, are equal to 30, 15, 10, 7½, 6, and 5 inches of mercury respectively; and the grains of water evaporated per minute in those temperatures were 30, 15, 10, 7½, 6, and 5, also; or numbers proportional to these. Indeed it should be so from the established law of mechanics, that all effects are proportional to the causes producing them. The atmosphere, it should seem, obstructs the diffusion of vapour, which would otherwise be almost instantaneous, as *in vacuo*; but this obstruction is overcome in proportion to the force of the vapour. The obstruction, however, cannot arise from the weight of the atmosphere, as has till now been supposed; for then it would effectually prevent any vapour from arising under 212°: but it is caused by the *vis inertiæ* of the particles of air, and is similar to that which a stream of water meets with in descending amongst pebbles.

" The theory of evaporation being thus manifested from experiments in high temperatures, I found that if it was to be verified by experiments in low temperatures regard must be had to the force of vapour actually existing in the atmosphere at the time. For instance, if water of 59° were the subject, the force of vapour of that temperature is 1-60th of the force at 212°, and one might expect the quantity of evaporation 1-60th also; but if it should happen, as it sometimes does in summer, that an aqueous atmosphere to that amount does already exist, the evaporation, instead of being 1-60th of that from boiling water, would be nothing at all. On the other hand, if the aqueous atmosphere were less than that, suppose one-half of it, corresponding to 39° of heat, then the effective evaporating force would be 1-120th of that from boiling water: in short, the evaporating force must be universally equal to that of the temperature of the water, diminished by that already existing in the atmosphere. In order to find the force of the aqueous atmosphere I usually take a tall cylindrical glass jar, dry on the outside, and fill it with cold spring water fresh from the well: if dew be immediately formed on the outside, I pour the water out, let it stand a while to increase in heat, dry the outside of the glass well with a linen cloth, and then pour the water in again: this operation is to be continued till dew ceases to be formed, and then the temperature of the water must be observed; and opposite to it in the table will be found the force of vapour in the atmosphere. This must be done in the open air, or at a window; because the air within is generally more humid than that without. Spring water is generally about 50°, and will mostly answer the purpose the three hottest months in the year; in other seasons an artificial cold mixture is required. The accuracy of the result obtained this way I think scarcely needs to be insisted upon. Glass, and all other hard, smooth substances I have tried, when cooled to a degree below what the surrounding aqueous vapour can support, cause it to be condensed on their surfaces into water. The degree of cold is usually from 1 to 10 below the mean heat of the 24 hours; in summer I have often observed the point as high as 58° or 59°, corresponding to half an inch of mercury in force; and once or twice have seen it at 62°: in changeable and windy weather it is liable to considerable fluctuation: but this is not the place to enlarge upon it.

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* This refers to experiments on the evaporation of water at 212°; for which see the Essay.

" For the purpose of observing the evaporation in atmospheric temperatures I got two light tin vessels, the one six inches in diameter and half an inch deep, the other eight inches diameter and three-fourths of an inch deep, and made to be suspended from a balance. When any experiment, designed as a test of the theory, was made, a quantity of water was put into one of these (generally the six-inch one, which I preferred), the whole was weighed to a grain; then it was placed in an open window or other exposed situation for 10 or 15 minutes, and again weighed to ascertain the loss by evaporation; at the same time the temperature of the water was observed, the force of the aqueous atmosphere ascertained as above, and the strength of the current of air noticed. From a great variety of experiments made both in the winter and summer, and when the evaporating force was strong and weak, I have found the results entirely conformable with the above theory. The same quantity is evaporated with the same evaporating force thus determined, whatever be the temperature of the air, as near as can be judged; but with the same evaporating force, a strong wind will double the effect produced in a still atmosphere. Thus, if the aqueous atmosphere be correspondent to 40° of temperature and the air be 60°, the evaporation is the same as if the aqueous atmosphere were at 60° of temperature and the air 72°; and in a calm air the evaporation from a vessel of six inches in diameter in such circumstances would be about ·9 of a grain per minute, and about 1·8 grains per minute in a very strong wind; the different intermediate quantities being regulated solely by the force of the wind."

Of the Aqueous Atmosphere.

Having quoted so much of this essay as may suffice to exhibit the principles on which we shall proceed, it may be useful, before we do this, to recapitulate the following circumstances respecting the atmosphere of aqueous gas, or (for brevity) the aqueous atmosphere.

1st, It is supplied by the process of evaporation, which by this theory appears to be reduced to the immediate union of water with caloric into a binary compound, *aqueous gas*.

2dly, The supply of vapour (by which term, for the purposes of meteorology, we may denote aqueous gas), is regulated by the following circumstances:—1. Temperature of the evaporating water, being greater as this is higher, and *vice versâ*. 2. Quantity of surface exposed. Since it is from the surface only of the mass that the vapour in common cases can escape, the supply is in direct proportion thereto. 3. Quantity of vapour already subsisting in the atmosphere; the evaporation being less (with equal temperature and surface) in proportion as this is greater, and *vice versâ*.

3dly, The vapour thus thrown into the atmosphere is diffusible therein by its own elasticity, which suffices for its ascent to any height in a perfect calm. Yet, as in this case the *inertia* of the particles of air considerably resists its diffusion, so in the opposite one of a brisk current, the vapour, by the same rule, must in some measure be drawn along with the mass into which it enters.

4thly, The quantity of vapour which, under equal pressure, can subsist in a given mass of air, will be greater as the common temperature is higher, and *vice versâ* *.

Aqueous vapour is the only gas contained in the atmosphere which is subject to very sensible variations in quantity. These variations arise from its attraction for caloric being inferior to that of all the others. Hence when a cold body, such as the glass of water in the experiment above quoted, is presented to the atmosphere, the other gases, composing the latter, will only be cooled by it (and that at all known temperatures); but the vapour, after being more or less cooled, will begin to be decomposed, its caloric entering the body while the water is left on the surface.

The formation of cloud is in all cases the *remote* consequence of a decomposition thus effected, except that the caloric escapes, not into a solid or liquid, but into the surrounding gases.

Of the Formation of Dew.

Dew is the *immediate* result of this decomposition. The particles of water constituting it are, singly, invisible, on account of their extreme minuteness. The approach of dew is, nevertheless, discoverable by a dark hazy appearance, verging from purple to faint red, extending from the horizon to a small distance upwards, and most conspicuous over valleys and large pieces of water.

The theory of dew seems to be simply this:—During the heat of the day a great quantity of vapour is thrown into the atmosphere from the surface of the earth and waters. When the evening returns, if the vapour has not been carried off in part by currents, it will often happen that more remains diffused in the general atmosphere than the temperature of the night will permit to subsist under the full pressure of the aqueous atmosphere. A decomposition of the latter then commences, and is continued until the general temperature and aqueous pressure arrive at an equilibrium, or until the returning sun puts an end to the process. The caloric of the decomposed vapour goes to maintain the general temperature; while the water is separated in drops, which, minute as they are, arrive successively at the earth in the space of a few hours. That the ordinary production of dew is by a real *descent* of water from the atmosphere, and not by decomposition of vapour on surfaces previously cooled (as in the experiment already mentioned), any one may readily be convinced by observing in what abundance it is collected by substances which are wholly unfit to carry off the requisite quantity of caloric for the latter effect.

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* "The aqueous vapour atmosphere is variable in quantity according to temperature: in the torrid zone its pressure on the surface of the earth is equal to the force of ·6, and from that to one inch of mercury. In these parts it rarely amounts to the pressure of ·6, but I have frequently observed it above half an inch in summer: in winter it is sometimes so low as to be of no more force than ·1 of an inch of mercury, or even half a tenth, in this latitude, and consequently much less where the cold is more severe. This want of equilibrium in the aqueous vapour atmosphere is a principal cause of that constant inundation of it into the temperate and frigid zones, where it becomes in part condensed in its progress by the cold, like the vapour of distillation in the worm of a refrigeratory, and supplies the earth with rain and dew." See the Essays above quoted.

Of the Formation of the Stratus.

The case which has been just stated, *of the decomposition of vapour by the atmosphere in which it is already diffused*, goes but a little way in explanation of the production of a cloud consisting of visible drops, and confined to a certain space in the atmosphere: much less does it enable us to account for the diversity of its situations and appearances. In attempting this we will begin with the stratus, as the most simple in structure, and the next step, as it were, in the progress of *nubification*.

When dew falls upon a surface the temperature of which is superior to that of the atmosphere, it is plain that it will not continue there, but will be evaporated again: and a body so circumstanced will continue to refund into the atmosphere the whole of the water thus *gradually* deposited on it, so long as its substance can supply the requisite temperature to the surface. Moreover, water either in mass, or diffused among sand, clay, vegetable earth, &c. will continue to be evaporated therefrom with a force proportioned to its temperature, so long as the latter continues above that point which counterbalances the pressure of the aqueous atmosphere.

From these causes it happens, that after the earth has been superficially dried by a continuance of sunshine, and heated, together with the lakes and rivers, to a considerable depth, there is an almost continual emission of vapour into the atmosphere by night.

This nocturnal evaporation is usually most powerful in the autumn, about the time that the temperature of the nights undergoes a considerable and sometimes pretty sudden depression attended with a calm.

In this state of things the vapour arising from the heated earth is condensed *in the act of diffusing itself* (if we may be allowed the expression): the cold particles of water thus formed, in *descending* meet the ascending stream of vapour, and condense a portion on their surfaces: if they touch the earth they are again evaporated, which is not necessarily the case if they alight on the herbage. In this way an aggregate of visible drops is sooner or later formed; and as from the temperature thus communicated to the air next the earth the vapour has still further and further to rise in order to be condensed, the cloud will be propagated upward in proportion.

Hence the stratus most usually makes its appearance in the evening succeeding a clear warm day, and in that quiescent state of the atmosphere which attends a succession of them. Hence also the frequency of it during the penetration of the autumnal rains into the earth; while in spring, when the latter is *acquiring* temperature together with the atmosphere, it is rarely seen.

Of the Formation of the Cumulus.

When the sun's rays traverse a clear space of atmosphere, it is well known that they communicate no sensible increase of temperature thereto. It is by the contact, and what may be termed the *radiation*, of opake substances exposed to the light, that caloric is thrown into the atmosphere.

This effect is first produced on the air adjacent to the earth's surface, and proceeds upward, more or less rapidly, according to the season and other attendant circumstances. In the morning, therefore, evaporation usually prevails again; and the vapour which continues to be thrown into air, now increasing in temperature, is no longer condensed: on the contrary, it exerts its elastic force on that which the nocturnal temperature had not been able to decompose, and which consequently remained universally diffused. The latter, in rising *through the atmosphere* to give place to the supply from below, must necessarily change its climate, quit the lower air of equal temperature, and arrive among more elevated and colder air, the pressure from above still continuing unabated. The consequence is a partial decomposition, extending through the portion thus thrown up, and, in short, a recommencement in the superior region, of the same process which in the vicinity of the earth furnished the dew of the night. In this case, however, the particles of water cannot arrive at the earth, as they are necessarily evaporated again in their descent.

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* A plentiful dew may often be found on the grass after a stratus.

It appears that this second evaporation takes place precisely at that elevation where the temperature derived from the action of the sun's rays upon the earth, and decreasing upward, becomes just sufficient to counterbalance the pressure of the superior vapour.

Here is formed a sort of boundary between the region of cloud and the region of permanent vapour, which for the present purpose, and until we are furnished with a nomenclature for the whole science of meteorology, may be denominated the evaporating plane, or, more simply, the vapour plane *.

Immediately above the vapour plane, then, the formation of the cumulus commences (as soon as a sufficient quantity of vapour has been thrown up) by the mixture of descending minute drops of water with vapour newly formed, and just diffusing itself, as in the case of the stratus before described.

A continuance of this process might be expected to produce a uniform sheet of cloud, in short a stratus, only differing in situation from the true one. Instead of which we see the first-formed small masses become so many centres, towards which all the water afterwards precipitated appears to be attracted from the space surrounding them; and this attraction becomes more powerful as the cloud increases in magnitude, insomuch that the small clouds previously formed disappear when a large one approaches them in its increase, and seem to vanish instead of joining it. This is probably owing to the small drops composing them having passed in a loose manner, and successively, into the large one, and consequently so as not to be traced in their passage.

Are the distinct masses into which the drops form themselves, in this case, due to the attraction of aggregation alone, or is the operation of any other cause to be admitted?

A rigid mathematician would perhaps answer the latter clause in the negative; and with such a conclusion we should have great reason to remain satisfied, as cutting short

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* We use the word *plane* here in the same sense in which we apply it to a surface of water. Strictly speaking, it is a portion of a sphere.

much of the inquiry that is to follow, were it not that it leaves us quite in the dark, both as to the cause of the variety so readily observable in clouds, and their long suspension, not to insist on several facts contained in the former part of this paper, which would then remain unaccounted for.

The operation of one simple principle would produce an effect at all times *uniform*, and varying only in degree. We should then see no diversity in clouds but in their magnitude, and the same attraction that could bring minute drops of water together through a considerable space of atmosphere in a few minutes, ought not to end there, but to effect their perfect union into larger, and finally into rain.

In admitting the constant operation of electricity, which is sometimes so manifestly accumulated in clouds, upon their forms and arrangement, we shall not much overstep the limits of experimental inquiry, since it has been ascertained by several eminent philosophers, that "clouds, as well as rain, snow, and hail, that fall from them, are almost always electrified *."

An insulated conductor formed of solid matter retains the charge given to it so much the longer, as it is more nearly spherical, and free from points and projecting parts. The particles of water, when charged, appear to make an effort to separate from each other, or, in other words, become mutually repulsive. Moreover, when a small conducting substance is brought within the reach of a large one similarly electrified, the latter, instead of repelling, will throw the small one into an opposite state, and then attract it. From these and other well-known facts in electricity it would not be difficult to demonstrate, that an assemblage of particles of water floating in the atmosphere, and similarly electrified, ought to arrange themselves in a spherical aggregate, into which all the surrounding particles of water should be attracted (within a certain distance) at the same time that the drops composing such aggregate should be absolutely prevented from uniting with each other *during the equilibrium of their electricity*.

To apply this reasoning to the formation of the cumulus, we may, in the first place, admit that the commencement of distinct aggregation, in the descending particles of water, is due to their mutual attraction, by virtue of which small bodies, floating in any medium, tend to coalesce. The masses thus formed, however, often increase more rapidly than could be expected from the effect of simple attraction

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* Cavallo. Complete Treatise on Electricity, vol. i. p. 74.

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exercised at great distances; and when the cloud has arrived at a considerable size, its protuberances are seen to form, and successively sink down into the mass, in a manner which forces one to suppose a shower of invisible drops rushing upon it from all parts.

In unsettled weather the rapid formation of large cumuli has been observed to clear the sky of a considerable hazy whiteness, which on the other hand has been found to ensue upon their *dispersion* *.

On these considerations we are obliged to admit as a co-operating cause of the increase of this cloud, that sort of attraction which large insulated conducting masses exercise, when charged, on the smaller ones which lie within their influence. Instead of a *spherical* aggregate, however, we have only a sort of hemisphere, because that part of the cloud which presents itself towards the earth can receive no addition from beneath, there being no condensed water. On the contrary, the mass must be continually suffering a diminution there by the tendency of the cloud to subside, and of the vapour plane to rise during the increase of the diurnal temperature. It is this evaporation that cuts off all the cumuli visible at one time in the same plane, and it is reasonable to conclude that much of the vapour thus produced is again condensed without quitting the cloud, as its course would naturally be mostly upward. Thus the drops of which a cumulus consists may become larger the longer it is suspended, and the electricity stronger from the comparative diminution of surface.

Such is probably the manner in which this curious structure is raised, while the base is continually escaping from beneath it. That we may not however be accused of building such a castle in the air by attempting further conjecture, we may leave the present modification, after recapitulating some of its circumstances which appear to be accounted for.

The cumulus appears only in the day time, because the direct action of the sun's rays upon the earth can alone put the atmosphere into that state of inequality of temperature which has been described. It evaporates in the evening from the cessation of this inequality, the superior atmosphere having become warmer, the inferior colder: attended with a decrease of the superficial evaporation. It begins to form some hours after sun-rise, because the vapour requires that space of time to become elevated by the gradual accession from below. When a stratus covers the ground at sun-rise, however, we often see it collect into cumuli upon the evaporation of that part of it which is immediately contiguous to the earth: and this effect ought to happen; for the cloud is then insulated, the *vapour plane* is established, and every thing in the same state, except in point of elevation, as in the ordinary mode of production of the cumulus.

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* That clouds are not always evaporated when they disappear, but sometimes dispersed so as to become invisible as distinct aggregates, is a fact pretty well ascertained by observations. This happens sometimes by the approach of other clouds; at others, the evaporation of part of a cumulus is followed by the dispersion of the remainder. The criterion used was the speedy production of transparency in the one case, and of hazy turbidness in the other.

Lastly, the cumulus, however dense it becomes, does not afford rain, because it consists of drops *similarly* electrified and repelling each other, and is moreover continually evaporating from the plane of its base. The change of form which comes on before it falls in rain, and which indicates a disturbance of its electrical state, will be noticed hereafter.

[To be continued.]

LXV. *Account of a Spaniard who can endure, without being incommoded, the greatest Degrees of Heat.* *By* J. C. DELAMETHERIE *.

A NATIVE of Toledo, in Spain, about 23 years of age, who arrived lately at Paris, has made different experiments to show that he is capable of enduring the greatest degrees of heat without being incommoded. We shall here give an extract of those made at the school of medicine before several of the professors, about three hundred of the pupils, and several other persons.

Care was taken to examine him before, and it was found that his state exhibited nothing different from that of a man in good health. His pulse beat about 75 or 78 times a minute.

EXP. I. A vessel containing oil heated to 85° of Réaumur being prepared, he opened his hand and applied the palm of it several times to the oil; he then washed his hands and face in the oil, and applied the soles of his feet to it. At the end of the experiment the heat of the oil was still from 76 to 78 degrees.

EXP. II. A bar of iron from 18 to 20 inches in length and 2½ inches in breadth was brought to a cherry red heat at one of its extremities, and placed on bricks. The Spaniard placed the sole of one of his feet on the red part: a portion of the oil which still adhered to it immediately inflamed. He applied the sole of the other foot in the like manner, and this he repeated several times.

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* From the *Journal de Physique*, Messidor, an. 11.

Exp. III. The flat part of a large iron spatula 18 inches in length was brought to a cherry red heat. The Spaniard thrust out his tongue, applied it to the red part of the spatula, and repeated the same thing several times. Three glasses of pure water were then brought, into one of which a few drops of sulphuric acid were put, and into another a pretty large quantity of marine salt: the third contained only water. The Spaniard was made to drink these three glassfulls, and was able to distinguish perfectly the savour of them.

Exp. IV. He took a lighted candle, and drew the flame of it several times over the posterior part of his leg from the heel to the ham. He was examined after these trials, and no part of his skin appeared to be in the least altered. The sole of his foot seemed to be smokey—which ought to be ascribed to the carbon of the oil—but his pulse beat from 130 to 140 times in a minute. It appears that since that time he placed himself in an oven heated to 70 degrees, and *remained in it some minutes.*

Dr. Blagden, a fellow of the Royal Society of London, supported a still greater degree of heat. He heated an apartment till Fahrenheit's thermometer rose to 260 degrees, entered it with his clothes on, and remained in it 8 minutes. At last he was much oppressed. Several other persons entered it also. His pulse when he quitted the apartment beat 144 times in a minute.

In another experiment he entered, undressed, into the same apartment heated to 220 degrees of Fahrenheit, and remained in it 12 minutes without being incommoded.

In a third experiment, the chamber being heated to 250 degrees of Fahrenheit, he entered it along with several other persons, and remained in it several minutes without being incommoded.

Some eggs and a beefsteak were placed in the same apartment on a pewter dish; in twenty minutes the eggs were entirely hard, and in forty-seven the beefsteak was not only baked but almost dry.

LXVI. *Letter from Captain* Baudin, *now employed on a Voyage of Discovery, to* C. Jussieu *.

On board Le Geographe, Port Jackson,
New Holland, Nov. 11, 1802.

The return of the Naturaliste to France, under the command of captain Hamelin, will enable you to judge how well our time has been employed in what relates to natural history. I have entrusted him with the care of conveying to their destination all the articles we have hitherto collected, being persuaded that he will discharge this duty with that zeal and attention of which he has given me so many proofs. I recommend him to you on that score.

By my letter to the minister of the marine, containing several extracts of my Journal, you will see that for two years I have done every thing in my power to increase our collections of every kind.

The premature death of C. Riedlé and Maugé, whose memory I must ever respect, has reduced me to the necessity of undertaking myself the departments of both these gentlemen, in which they acquitted themselves with a zeal I can never hope to equal.

I shall not entertain you at present with an account of all the events which have occurred since our departure; I shall only observe, that I never performed a voyage attended with so many hardships. More than once has my health suffered; but if I shall be so fortunate as to terminate this expedition agreeably to the intentions of government, and the expectations of the French nation, I shall have little left to wish for, and my difficulties will soon be forgotten. I have the greater hopes of succeeding, as Lewin's Land, with those of Concord and De Witt, Entrecasteaux's Channel, the island of Maria and its environs, the eastern coast of the large island of Diemen, Basse's and Banks's straits, and the whole south-west coast of New Holland from Cape Wilson to the islands of St. Peter and St. Francis, have been explored in a manner sufficient to ensure the safety of navigation. Much, however, remains to be done for the topography of the country, which no doubt will be long unknown, on account of the natural difficulties presented by the extent of coast which we have explored.

To supply the place of Le Naturaliste I resolved to purchase a small vessel of thirty tons, which I have named the *Casuarina*, because the greater part of it is constructed of

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* From *Annales du Museum d'Histoire Naturelle*, No. 10.

wood distinguished by that appellation. This small vessel will in future accompany me, and will be of the greatest utility. Had I been in possession of it sooner, some places to which I could not penetrate would not have remained unexplored; as it draws little water, it can any where approach the coast.

Another consideration, no less important, which made me resolve to send home the Naturaliste was, the embarrassment occasioned by transporting our collections, which the casualties of the sea and the length of the voyage might have rendered fruitless to government and to the sciences, had I subjected them to the new risks to which we are about to be exposed: as the number is very considerable, and as they are of some value, I am convinced that government will approve of my conduct.

Many of the great number of birds which I have sent to you, and which I received from the inhabitants of Port Jackson, are in a bad state: they will not give you a very high idea of their skill in the art of preparing them, but you will doubtless be indemnified for this loss by those which we prepared ourselves. The quadrupeds, insects, living plants, hortus siccus, seeds, shells, madrepores, &c. are in the best state; and I have no doubt that these articles will be delivered to you in the same condition by the care of captain Hamelin.

If the live plants reach the place of their destination, you will have the most curious and the most beautiful productions of the country, and you will regret that you did not botanise on the soil which gave them birth. The whole country, at the time I am now writing, is covered with the most beautiful flowers. For variety I know no place but the Cape of Good Hope which can be compared to it. Though the greater part of our live plants were collected between the 33d and 42d degree of south latitude, I think it proper to observe, that I much fear whether they can be naturalised in France so speedily as you may wish. The temperature of Van Diemen's Land is not so cold as the latitude where it is situated seems to indicate. That of New Holland is still less so. At the commencement of winter, when we were to the south of Van Diemen's Land, the thermometer was only once at five degrees. At that time a strong south-west wind prevailed, accompanied with hail. At Port Jackson, in the middle of winter, we had it during one night very near zero; in the day time it generally kept at 6 or 8 degrees, and in the night between 4 and 5, and it rarely fell to 3. It appears to me, therefore, that an orangerie will suit them best during our winter in France, which is much more severe, and cannot be compared with that which we have experienced. Here the orange- and lemon-trees stand in the open air. They have thriven exceedingly well, and produce as fine fruit as in Portugal.

The seeds which I send you were in part given to me by the natives of the country; others I collected myself in the interior parts. I have proceeded beyond the most distant parts known to the English; but an almost impenetrable chain of mountains of the first order, known under the name of the *Blue Mountains*, the direction of which, inclining southward, seems to extend as far as Cape Wilson, and which, towards the north, terminates at Port Stephens, did not allow me to go further than 75 or 80 miles, reckoning from Port Jackson. If credit can be given to what is stated by the natives and some English adventurers, a large river of brackish water traverses these mountains, at the extremity of which there is a settlement of white men. (This is the name given by the natives to the Europeans.) Since my return I have often conversed on this subject with governor King, whose conduct I cannot sufficiently praise; but he declared to me that he placed no belief in these reports, which were invented by some deserters, who would never have returned had they met with a settlement of Europeans beyond these mountains.

On leaving Port Jackson, I intend to proceed through Basse's Straits in order to explore an island of considerable extent lately discovered by some English fishermen, and which has been called King's Island. When I have terminated the geographical part of my labour, I shall proceed to Kangaroo Island, on the south-west coast of New Holland, the southern part of which neither I nor captain Flinders was able to examine. I shall then direct my course to the islands of St. Peter and St. Francis, to examine them again, and to ascertain the direction of the continent in that part with which I am unacquainted. Proceeding then from the point where general d'Entrecasteaux stopped, and which we have already seen, I shall proceed directly to Lewin's Land, to finish the survey of the large bay distinguished by the name of the Geographe. As it appeared to me of importance to determine the position of Rosemary Isles discovered by Dampier, and which I have already sought for in vain by the longitude and latitude assigned to them in our charts, I shall make a new attempt to discover them, that I may then proceed to De Witt's Land, the chart of which is not sufficiently correct to ensure

sure the safety of navigation. The reasons which prevented me from undertaking this labour the first time I sailed along that coast, are known to you by the letter which I addressed to the minister of the marine a short time before I left Timor. The northern coast of New Holland and the Gulf of Carpentaria will terminate our labours; but I am afraid that so extensive an undertaking will require more time than the provisions we have laid in here will admit.

I have seen not without admiration the immense establishments formed here by the English during the twelve years they have been settled at Port Jackson. Though they began with great means, and expended a great deal of money, it is still difficult to conceive how they could so soon attain to that state of opulence and splendour in which they are at present. Nature indeed has done every thing for them in the beauty and safety of the port where their establishment is situated; but the quality of the surrounding land obliged them to penetrate to the interior of the country, till they found a soil proper for the different kinds of cultivation, which produce an abundant supply for their subsistence, and for the consumption of the European vessels which visit that coast for the whale fishery and for other purposes. Besides brigs, sloops, and middle-sised vessels, built in the new colony, and belonging to different individuals, we found on our arrival in this port nine large vessels from England and two Americans. Some of them are to return by China; the rest will be employed in fishing for spermaceti whales. The advantage arising from this kind of speculation will add a considerable increase to the British shipping, if the fishery continues to be successful. It is carried on in general on the coasts and in the environs of New Zealand.

The present population of Port Jackson, and other places occupied by the English, amounts to 6000 persons, employed chiefly in agriculture. All the fruit-trees of Europe have become naturalized, but they have not all been attended with equal success. Among this number are the apple-, cherry-, and almond-tree. All kinds of pulse, without exception, thrive, are well tasted, and abundant in the proper season. The vine, which during the first years afforded great hopes, has degenerated so much that it is doubted whether it will maintain itself. The cause of this unexpected degeneration is not well known: it is, however, ascribed to the scorching drought of the north-east wind, the effects of which are pernicious.

The natives settled in the neighbourhood of Port Jackson have retired to the interior part of the country in proportion

tion as the English have penetrated into it. They are, however, often met with in the town and villages, and on the highways, but never in considerable numbers. They have lost very little of their primitive habits; it is only remarked that they have made more progress in the English language than the English have done in theirs. They are of no use, and little to be feared. I am strongly inclined to believe that they are of a different origin from those of Van Diemen's Land.

As the English government has omitted nothing that could tend to the prosperity of the establishment, it has not suffered in its infancy. A stock of cows, sheep, and goats, was sent hither at its expense; and these animals have multiplied so much, that at the enumeration made of them in the month of August last, there were reckoned to be 800 bulls, 3600 cows, 6000 sheep, 1800 goats, and more than 10,000 hogs. The horses brought from the Cape of Good Hope and Bengal are of all the quadrupeds those which have thriven the least, though the cause is still unknown.

I shall not enlarge further on these details in this letter, because I have sent you a copy of that which I have addressed to the minister of the marine. You will find there a particular account of the articles sent home in Le Naturaliste, and which you are authorized to receive.

I beg to be remembered to you, and shall use my utmost endeavours to complete a new collection as numerous as that sent to you by Le Naturaliste.

LXVII. *Statement of the Improvement and Progress of the Breed of fine-woolled Sheep in New South Wales; presented by Captain* M'ARTHUR *at the Right Honourable Lord* HOBART'S *Office, 26th July,* 1803.

THE samples of wool brought from New South Wales having excited the particular attention of the merchants and principal English manufacturers, captain M'Arthur considers it his duty respectfully to represent to his majesty's ministers, that he has found, from an experience of many years, the climate of New South Wales is peculiarly adapted to the increase of fine-woolled sheep; and that, from the unlimited extent of luxuriant pastures with which that country abounds, millions of those valuable animals may be raised in a few years, with but little other expense than the hire of a few shepherds.

The specimens of wool that captain M'Arthur has with him, have been inspected by the best judges of wool in this kingdom; and they are of opinion that it possesses a softness superior to many of the wools of Spain, and that it certainly is equal in every valuable property to the very best that is to be obtained from thence.

The sheep producing this fine wool are of the Spanish kind, sent originally from Holland to the Cape of Good Hope, and taken from thence to Port Jackson.

Captain M'Arthur, being persuaded that the propagation of those animals would be of the utmost consequence to this country, procured in 1797 three rams and five ewes; and he has since had the satisfaction to see them rapidly increase, their fleeces augment in weight, and the wool very visibly improve in quality. When captain M'Arthur left Port Jackson in 1801, the heaviest fleece that had then been shorn weighed only three pounds and a half: but he has received reports of 1802, from which he learns that the fleeces of his sheep were increased to five pounds each, and that the wool is finer and softer than the wool of the preceding year. The fleece of one of the sheep originally imported from the Cape of Good Hope has been valued here at four shillings and sixpence per pound, and a fleece of the same kind bred in New South Wales is estimated at six shillings per pound.

Being once in possession of this valuable breed, and having ascertained that they improved in that climate, he became anxious to extend them as much as possible; he therefore crossed all the mixed bred ewes, of which his flocks were composed, with Spanish rams. The lambs produced from this cross were much improved; but, when they were again crossed, the change far exceeded his most sanguine expectations. In four crosses, he is of opinion, no distinction will be perceptible between the pure and the mixed breed. As a proof of the extraordinary and rapid improvement of his flocks, captain M'Arthur has exhibited the fleece of a coarse-woolled ewe that has been valued at ninepence a pound; and the fleece of her lamb, begotten by a Spanish ram, which is allowed to be worth three shillings a pound.

Captain M'Arthur has now about four thousand sheep, amongst which there are no rams but of the Spanish breed. He calculates that they will, with proper care, double themselves every two years and a half; and that in twenty years they will be so increased as to produce as much fine wool as is now imported from Spain and other countries at an annual expense of 1,800,000l. sterling. To make the principle perfectly plain upon which captain M'Arthur founds this expectation, he begs to state, that half his flock has been raised from thirty ewes, purchased in 1793 out of a ship from India, and from about eight or ten Spanish and Irish sheep purchased since. The other half of his flock were obtained in 1801, by purchases from an officer who had raised them in the same time, and from about the same number of ewes that captain M'Arthur commenced with. This statement proves that the sheep have hitherto multiplied more rapidly than it is calculated they will do in future; but this is attributed to the first ewes being of a more prolific kind than the Spanish sheep are found to be; for, since captain M'Arthur has directed his attention to that breed, he has observed the ewes do not so often produce double lambs.

As a further confirmation of the principle of increase that captain M'Arthur has endeavoured to establish, and which he is positive time will prove to be correct, he would refer to the general returns transmitted from New South Wales. In 1796 (since when not one hundred sheep have been imported) 1531 were returned as the public and private stock of the colony. In 1801, 6757 were returned; and although between those periods all the males have been killed as soon as they became fit, yet there is a surplus over the calculation of 633.

Captain M'Arthur is so convinced of the practicability of supplying this country with any quantity of fine wool it may require, that he is earnestly solicitous to prosecute this, as it appears to him an important object; and on his return to New South Wales to devote his whole attention to accelerate its complete attainment. All the risk attendant on the undertaking he will cheerfully bear: he will require no pecuniary aid: and all the encouragement he humbly solicits for is the protection of government, permission to occupy a sufficient tract of unoccupied lands to feed his flocks, and the indulgence of selecting from amongst the convicts such men for shepherds as may, from their previous occupations, know something of the business.

London, (Signed) JOHN M'ARTHUR.
July 26, 1803.

LXVIII. *Proceedings of Learned and other Societies.*

SOCIETY OF THE FRIENDS OF THE SCIENCES AT WARSAW.

In their public sitting of May 5, this Society proposed the following

Prize Questions:

I. The formation of saltpetre was formerly much more considerable than at present in several provinces of the ci-devant Poland; so that it formed an important article of commerce. The Society, therefore, requires an answer to the following question:—1st, Are there in these provinces any kinds of earth from which saltpetre can be obtained by lixiviation without any further preparation; and where are these earths found? 2d, The Society requires a chemical examination of the mould or upper strata of earth in those parts of the Ukraine and Podolia which formerly produced, or still produce, the greatest quantity of saltpetre, in regard to the formation of this salt at the place of observation. 3d, A comparison of the Polish saltpetre, as sold in commerce, in regard to purity, with that manufactured in other countries, and particularly that of Bengal. 4th, An accurate description of the process by which saltpetre is obtained and refined in the different manufactories of the Ukraine and Podolia. 5th, Whether the new chemistry affords any means of improving these processes, so that saltpetre may be obtained at a cheaper rate and in a greater state of purity. 6th, A comparison of the expenses of manufacturing saltpetre according to the new French, the German, and the Polish method. 7th, A comparison of the East India saltpetre with the saltpetre made in the different manufactories of Europe, both before the year 1790 and at the present time. 8th, Plan for carrying on trade with this production in the most advantageous manner, so as to make it pay for the expenses of manufacturing it.

II. It is well known that a considerable and advantageous trade was formerly carried on, and particularly at Venice, with the Polish cochineal or czerwiec (*coccus Polonicus tinctorius*). The Society therefore requires, 1st, A complete natural history of czerwiec, with an account of the difference between the *real* Polish cochineal and the *false*, which is so often sold for the real czerwiec; and how this deception can be detected. 2d, What is the essential difference between the American cochineal and the Polish czerwiec? 3d, For what reasons has the American cochineal been preferred in modern times to the Polish? and by what means can czerwiec be again introduced into commerce? 4th, Can barren sandy land, useful for no other œconomical purpose, be employed in the regular cultivation of czerwiec? 5th, In what manner can czerwiec be raised at the least expense, and in the greatest abundance? 7th, Whether czerwiec possesses any medicinal properties?

III. As the plague formerly occasioned great devastation in Poland, the Society requires, 1st, A complete history of the plague in Poland, from the most authentic sources. The author must examine, 2d, Whether this epidemia did not sometimes originate in Poland, or whether it was brought to these provinces from foreign countries? 3d, In both these cases, what were the regular symptoms by which it was known; and could the place where the disease chiefly originated be determined from these symptoms? 4th, Whether the disease by passing from one province to another became more destructive; or whether its malignant nature was thereby moderated? 5th, What means of preventing the infection were employed at different periods in this country with success; and what mode of curing the disease has been found most effectual? 6th, Whether the total extirpation of the plague be possible; and in what manner this is to be accomplished?

The papers, written in the Polish, Latin, German, or French languages, must be transmitted, post paid, to the secretary of the Society of the Friends of the Sciences, before the end of August 1804.

The prize for the best essay on each of these subjects is a gold medal of the value of forty ducats.

One of these prizes only is paid from the funds of the Society. The other two are proposed by two worthy members, actuated by a laudable zeal for diffusing useful knowledge.

ACADEMY OF SCIENCES AT BERLIN.

The following papers were read in this society during the first half of the year 1803:

Jan. 6th. A dissertation on the nature, causes, and cure of mania; by Dr. Hufeland.

Jan. 13th. Professor Bode read an extract from his Journal of Astronomical Observations; with a description of a new sextant by M. Klengert, of Breslau.

Jan. 20th. M. Ancillon: continuation and conclusion of his Philosophical and Moral Thoughts.

Jan. 27th, being a public sitting, M. Merian delivered an introductory oration. M. du Verdy read a sketch of a plan of equestrian exercises in imitation of the tournaments of the antient chivalry. M. Erman: Anecdotes of the reign of the elector Frederic William the Great, from a manuscript journal of Th. Sig. de Buch, *maréchal du voyage* of the great elector: first memoir. M. Klaproth: On meteoric stones and metallic masses. M. Klein: On diseases of the mind and mental debility in a juridical point of view.

Feb. 3d. M. Erman read a dissertation of baron de Chambrier, ambassador at the court of Sardinia, on Casimir, margrave of Brandenbourg-Bareith: second memoir.

Feb. 10th. Dissertation on propping fruit-trees, with observations by M. Treff.

Feb. 17th. Professor Burja: On the relation which exists between music and declamation: first memoir.

Feb. 24th. M. Klein: A continuation of his dissertation on diseases of the mind and mental debility.

March 3d. M. Trembley: Memoir on the philosophy of the poets.

March 10th. M. Klaproth: Continuation of his treatise on meteoric stones and metallic masses.

March 17th. M. Bernoulli: On the atmospheres of the celestial bodies, by M. D. Melanderhjelm, perpetual secretary of the Academy of Sciences at Stockholm; and a memoir, transmitted by M. La Grange, entitled Researches on several Points of Analysis relating to preceding Memoirs.

March 24th. M. Teller: A psychological dissertation on imitation in style.

March 31st. M. Denina: A continuation of observations on the common origin of the Sclavonic, Latin, Celtic, and German languages.

April 21st. M. Gerhard: Observations and conjectures on simple earths. Part i.

April 28th. M. Trembley: On the methods of approximation: second memoir.

May 5th. M. Biester delivered an oration on character.

May 12th. M. Erman: Anecdotes of the reign of the elector Frederic William the Great: second memoir.

June 9th. Professor Fischer: On measuring heights by the barometer.

June 16th. M. Bastide: On the alopecurus pratensis. Professor Fischer: A continuation of his dissertation.

June 23d. M. Bastide: A dissertation of professor Buttman on the philosophical meaning of the Grecian deities, and particularly Apollo and Diana.

June 30th. M. Bastide: A new commentary on Montaigne.

The Physical Class has proposed the following questions as the subject of prizes for the year 1805:

I. Is Mariott's law a general law for all elastic fluids, or only for atmospheric air?

II. What is the nature of that disease called inflammation of the spleen, which prevails so much among cattle? whence does it arise? and what is the method of cure?

The prize for each of the above questions is fifty ducats.

III. As the lungs consist of a cartilaginous air-pipe and cellular tissue, to which lymphatic vessels, bronchial arteries, veins and nerves, proceed—and as the pulmonary artery and veins convey the whole mass of the blood through the lungs, answers to the following queries are required:—In what manner, and where, does the cartilaginous air-pipe terminate? Does it proceed into the cellular tissue of the lungs, or has it determinate boundaries? Does it remain cartilaginous in its minutest divisions, and, as such, does it terminate in the surrounding cellular tissue? Do the bronchial vessels belong merely to this cartilaginous pipe, or to the cellular tissue and the lungs also?—that is to say, Do the bronchial vessels convey nourishment to the air-pipe alone, or to the cellular tissue also? Where does the pulmonary artery of the lungs end? Does it convey the blood by help of the cellular tissue merely through the lungs, and transmit it immediately to the veins of the lungs, or does it expire a fluid into the cellular tissue of the lungs, which in breathing flows out through the wind-pipe?—or, Does the pulmonary artery separate a moisture from the exterior surface of the lungs? Where do the pulmonary veins arise? Do they arise alone from the arteries, or, as absorbing vessels, do they take their origin in part also from the bronchia, the cellular tissue of the lungs, and from the exterior surface of the lungs? How do the nerves of the eight pair and the intercostal nerves terminate? Do those of the eight pair terminate alone in the bronchia, or do they run into the cellular tissue of the lungs? Are the eight pair connected with the branches which the intercostal nerve sends out to the finest vessels in the lungs? The prize is a golden medal of the value of 80 ducats, or 80 ducats in money. The answers must be grounded on microscopical observations.

LXIX. *Intelligence and Miscellaneous Articles.*

AËROSTATION.

Hamburgh, Aug. 12.

PROFESSOR ROBERTSON yesterday made his second ascent, which, like the former, was exceedingly brilliant. At a quarter past twelve he stepped into the car, with his friend M. Lhoert, carrying with him barometers, thermometers, a speaking trumpet, &c. At 35 mintutes after twelve, the balloon being at the height of 600 toises, M. Robertson launched a parachute, which fell very slowly. At three quarters after twelve the balloon was at the height of about 1200 toises. During this ascent Mr. Robertson made several new experiments, which had been suggested to him by some of our philosophical men.

Aug. 13.

Professor Robertson and his friend returned hither at four in the afternoon. Their second aërial voyage has been as successful as the first. As the wind carried them towards the sea, and the sky was so obscured by thick clouds that they could not see to a great distance, they did not rise to so great a height as the first time. The aëronauts descended near the village of Rehout, in Holstein, having traversed about eight miles in the course of an hour. Mr. Robertson, during this voyage, made some new experiments, an account of which he has promised to publish.

Aug. 14.

By observations made during his last ascent, Mr. Robertson has found that barometric calculation does not show with precision the real heights in the atmosphere. He weighed different bodies by means of a spring balance, and found a great difference between their gravities in the elevated regions of the atmosphere compared with those at the surface of the earth. He ascertained that the magnetic virtue decreases as the square of the distances. He observed that sounds may be conveyed upwards to the height of 200 toises, while downwards they can be conveyed only half that distance. The solar rays collected in the focus of a lens lose one-third of their intensity. Mr. Robertson is preparing to continue his tour to Petersburgh.

Aug. 15.

It was 42 minutes past twelve when Mr. Robertson detached his balloon from the earth when he made his last ascent. The barometer being at 21 inches 12 lines, and the thermometer at 21 degrees, he launched two parachutes of different sizes, and loaded with equal weights, in order to calculate the resistance of the air. The second, which was launched a hundred toises higher than the first, fell with much greater velocity, but did not unfold itself till it had passed over a double space. At 51 minutes after twelve he passed between two large clouds, which seemed to open to afford a passage to the balloon. The form of these masses of vapour is oblong. They resemble rags suspended above the earth. Their upper parts do not form in their aggregate a smooth surface, as appears to those who look at them from the earth; on the contrary, they resemble long pyramids. This effect ought to be ascribed to caloric, which, if we may use the expression, converts these masses into Montgolfiers, the elevation of which is proportioned to the density of the atmosphere. They appeared to Mr. Robertson to plunge towards the earth, in consequence of an optic effect resulting from the apparent immobility of the balloon, which, however, was rising at the rate of 50 feet per second. When the thermometer indicated one degree above freezing, and the barometer stood at 15 inches, Mr. Robertson set at liberty two pigeons, which descended with the rapidity of lightning, without moving their wings, and in a plane slightly inclined. When the barometer stood at 14 inches he let off a third pigeon, which, having fluttered about for a moment with difficulty, perched on the net-work, and would not quit it. Two butterflies let go at the same time tried to use their wings, but in vain, as the air was too rare: they never quitted the car, and fluttered, but in a very feeble manner. Tinder exposed to a convex glass of six inches focus did not catch fire till the end of some minutes. The rays refracted by the prism no longer exhibited lively and distinct, but weak and confused, colours. Weights attached to a spring balance had lost one half of their gravity. The magnetic needle began again to put itself in motion. By means of a very ingenious instrument invented by M. Hez, mechanician, he inclosed four inches of the surrounding air along with mercury, and marked exactly the point where the air and the mercury were united. When he reached the earth, he found that the latter filled the whole tube within a tenth. This important experiment seems to prove, that in the upper regions there exists nothing but vapours, and no atmospheric air. If this conjecture of Mr. Robertson be verified, there will be no reason why people may not ascend to a much greater height than that of 3670 toises, to which we know some have ascended: but for this purpose a balloon a balloon of 40 or 50 feet in diameter would be necessary; otherwise the loss of gas experienced by the balloon might make it descend with a velocity which would endanger the lives of the aëronauts. Mr. Robertson experienced this during his ascent before the last, when he was in danger of being killed.

ASTRONOMY.

The eclipse of the sun which took place on the morning of Aug. 17th was observed at Paris, the weather being very fine, by all the astronomers, Delambre, Messier, Bouvard, Lalande nephew, Burckhardt, and myself. I saw the commencement at 5^h 59^m 18^s, at the college of France; and M. Messier observed the end at 7^h 46^m 8^s. In consequence of the rule I have formed to calculate such observations, the same day I found the conjunction at 6^h 30^m 24^s true time, reduced to the meridian of the observatory. This will serve as a term of comparison for finding the longitude of all those countries where it may have been observed. It was annular in Egypt. DELALANDE.

DREADFUL ACCIDENT.

On Thursday the 8th of September a steam engine employed to assist in clearing the works from water at the tide-mills now erecting in the marsh between Greenwich and Woolwich, was blown up by the force of the contained steam. The explosion was as sudden and dreadful as that of a powder-mill, and was accompanied with a similar noise, which was heard at a great distance from the place. The engine was on Mr. Trevethick's plan, worked by the expansive force of steam only, without employing condensation as in the engines in common use. It was literally blown to pieces; and we are sorry to state, that by the accident three people were killed on the spot, and three others, all that were there at the time, so much hurt that two of them are not expected to recover. It was a fortunate circumstance that the accident happened at a time when the other workmen were at dinner, or a much greater number might have lost their lives.

Steam-engines on Mr. Trevethick's plan require a boiler of immense strength; for they work with a power sometimes equal to 60 pounds on the square inch, while common engines, even Mr. Watts's, seldom work with more than 5 pounds. We are happy to state, however, that the present accident arose, not from the impossibility of making a boiler strong enough, but from a culpable mismanagement of a boy appointed to attend the engine. Impatient to finish his work, he had put a piece of timber between the top of the safety valve and a beam above it, so that it could not rise to allow steam to escape when produced in greater quantity than required. He even went away to fish in the river. In the mean time the engine was stopped by another workman, who knew not what the boy had done, and in a short time the mischief we have stated followed. The boy had returned, and was in the very act of removing the piece of wood he had so imprudently put over the valve when the explosion took place. He was the least hurt of all who were near the spot.

This accident ought to serve as a warning to engineers to construct their safety valves in such a manner that common workmen cannot stop them at their pleasure; which may be easily done.

From the way in which part of the boiler was bent, which was constructed of cast iron nearly an inch in thickness, it is thought the steam must have acquired an expansive force equal to 500 pounds on the square inch before it gave way —a force much beyond any that can ever be required. But though this shows that engines on Mr. Trevethick's plan may, with proper precautions, be worked with as much safety as those on the common principle, such an accident as the one we have stated cannot fail to intimidate some people from adopting them. It is therefore with much pleasure we state that a boiler on a new construction, calculated to bear a much higher degree of expansive force than can ever in any case be required, has been lately invented by a very able engineer, Mr. Woolf. It consists of a combination of cylindrical tubes, which unite the double advantage of exposing a much larger surface to the action of the fire than the common boiler, while they possess a much greater degree of strength. This invention appears to us so important, that we shall take an early opportunity of laying a description of it before the public.

METHOD OF PREVENTING IRON AND STEEL FROM RUSTING *.

C. Conté has found out a method of preventing the oxidation of iron and steel, or, to speak in language more generally understood, to prevent iron and steel from rusting. This method consists in mixing with fat oil varnish a half, at least, or rather four-fifths, of well rectified spirit of turpentine.

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* From the *Magasin Encyclopédique.*

pentine. This varnish is applied slightly, and in an equal manner, by means of a sponge; after which the article is suffered to dry in a place sheltered from dust. Articles varnished in this manner, it is said, will retain their metallic brilliancy, and never contract any spots of rust. This varnish may be applied also to copper, of which it preserves the polish, and brightens the colour. It may be employed in particular, with advantage, for preserving from alteration philosophical instruments, which, in the course of experiments, are brought into contact with water, and by those means are liable to lose their splendour, and to become tarnished.

A SHOWER OF MUD.

The *Journal de Physique* for Germinal contains a letter from De Fortis to the editor, in which he gives an account of a shower of mud which fell in the evening of the 27th of March near Udina. "The wind (says the author) had blown with violence from the east for three days. The extent of country which was abundantly besprinkled by this strange rain was twelve miles in diameter from the borders of the sea to the bottom of the Alps of Carnia. I do not know whether the partisans of the opinion which makes lava come to us from the moon, can derive any arguments in their favour from the mud which has covered the plains of Friouli; but, for my part, I first imagined that the wind, being charged in Sicily or near Naples with clouds of volcanic dust, had deposited them at the bottom of the Carnian mountains, which prevented the clouds from going further. But having then observed, through a very powerful magnifying glass, a specimen of the sediment in question which a friend sent me from Udina, I convinced myself that it had not the least resemblance to that *detritus* which is raised by volcanoes to the superior regions of the atmosphere. It appears to me more natural to suppose that a storm, or perhaps water-spouts at sea, having sucked up some of the muddy water which the rivers by their inundation leave on the plains, raised them to the upper regions, where they were carried away by the winds. It is in consequence of similar circumstances, very natural and common, that worms, tadpoles, and small fishes have often been seen to fall from the clouds with rain, without any person conceiving the idea of making them come from an aërial race or from another globe."

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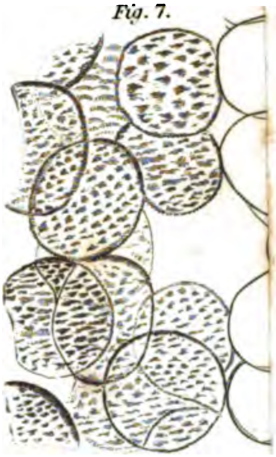
Fig. 7.



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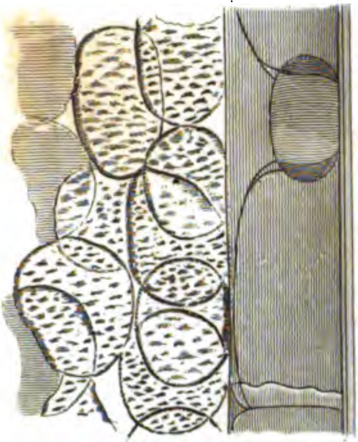

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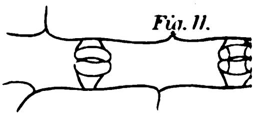

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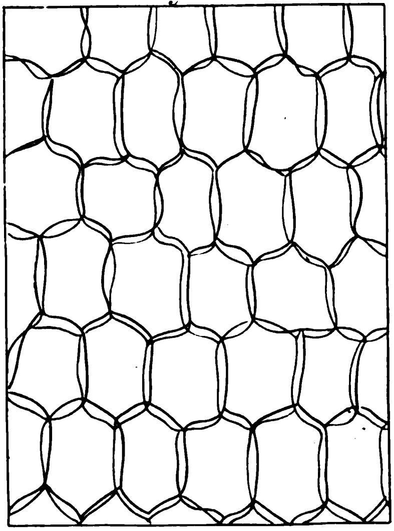

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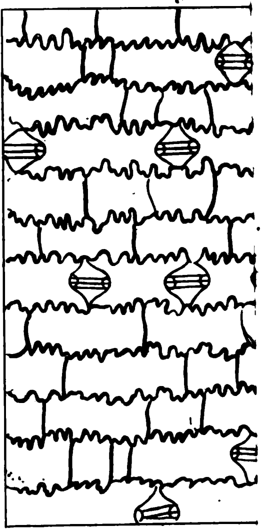

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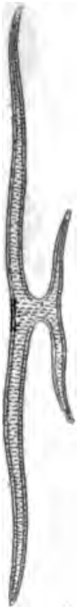

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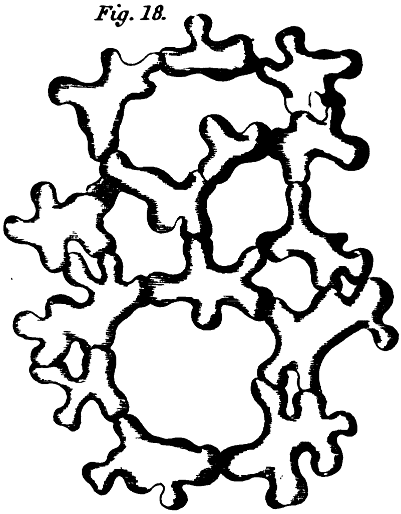

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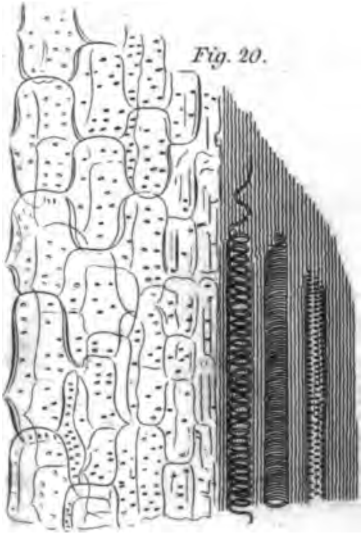

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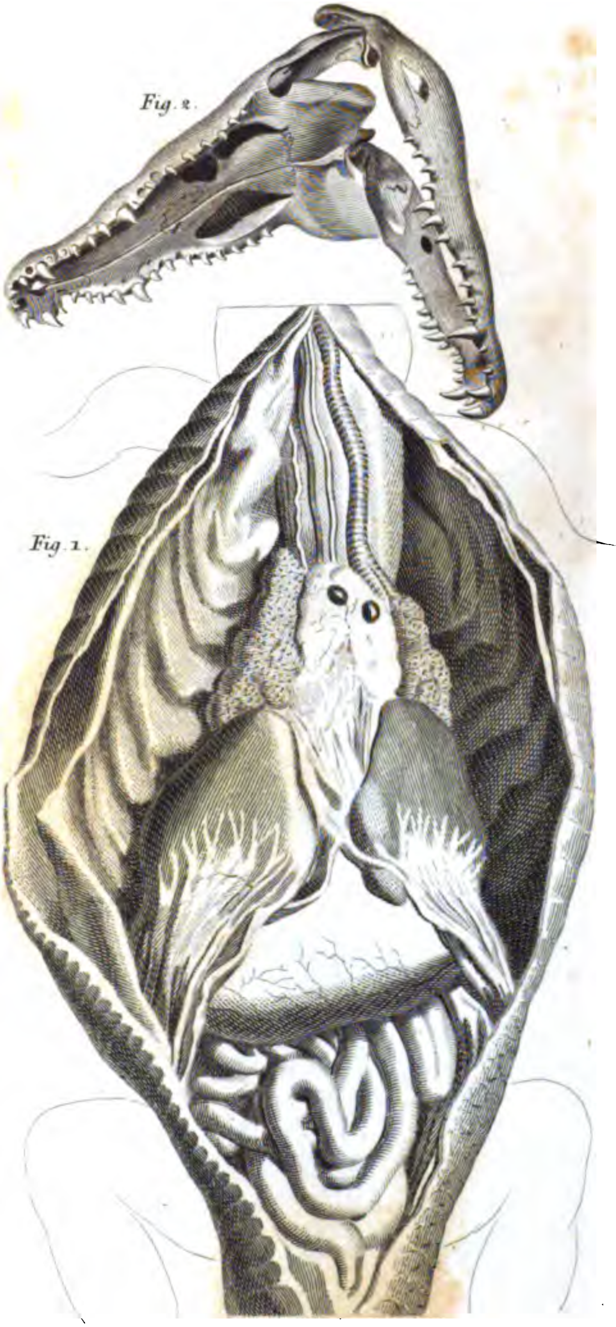

Fig. 2.

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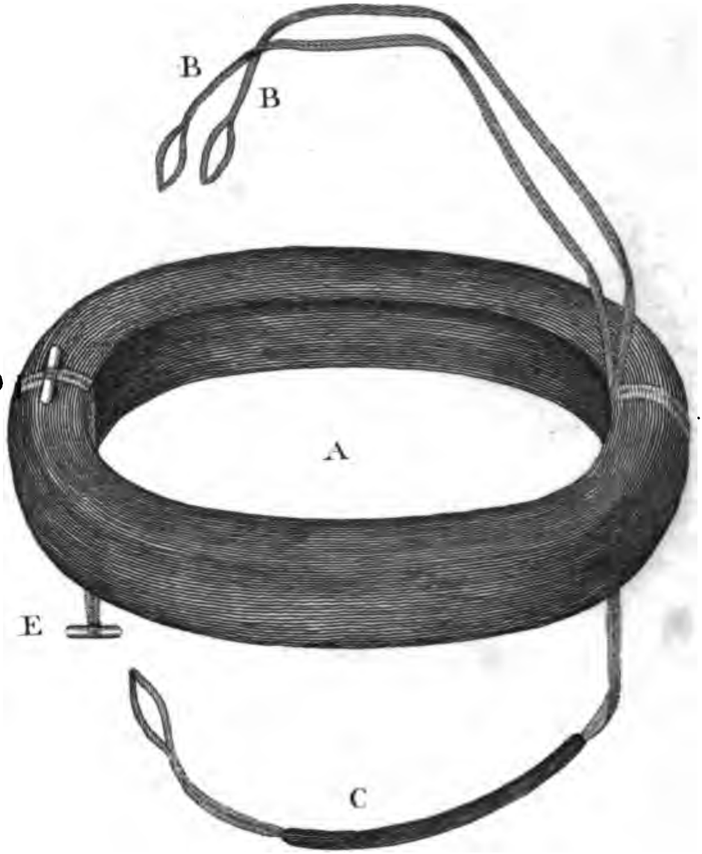

MARINE SPENCER,

For the preservation of Lives
in cases of Shipwreck or other
Accidents at Sea.

Silex - - - -	48
Alumine - - - -	16
Oxide of iron - - -	16
Lime - - - -	9
Soda - - - - -	4
Muriatic acid - - -	1
Water and volatile parts -	5
	99

Dr. Kennedy makes no mention of magnesia, but he indicates muriatic acid as one of the principles of basaltes. M. Klaproth analysed it again, in order to try to discover it. He decomposed basaltes by nitric acid, and poured into the solution a solution of nitrate of silver. He observed a slight cloud, and collected a deposit which weighed 3-10ths of a grain. On this muriate of silver he found nitric acid, which reduced it to 1-20th of a grain; a quantity so small, that it does not announce the hundredth part of a grain of muriatic acid in the basaltes of Hasenberg.

Analysis of the Gold Ore of Transylvania.

It was by the analysis of these gold ores, so rich, that M. Klaproth discovered tellurium; and as this new metal constitutes the greater part of them, they have since been called the auriferous ores of Tellurium. Under this name M. Klaproth comprehends,

1st, That of paradoxal gold, or the problematic metal taken from Mariahilf, near Zalathna, in the mountains of Faczebayer in Transylvania.

A thousand parts of this ore of tellurium are composed of

Tellurium - -	925.50
Iron - - -	72·00
Gold - - -	2·50
	1000

2d, The ore of graphic gold of Offenbanya, which is very rich. It contains

Tellurium - - -	60
Gold - - -	30
Silver - - -	10
	100

3d, That